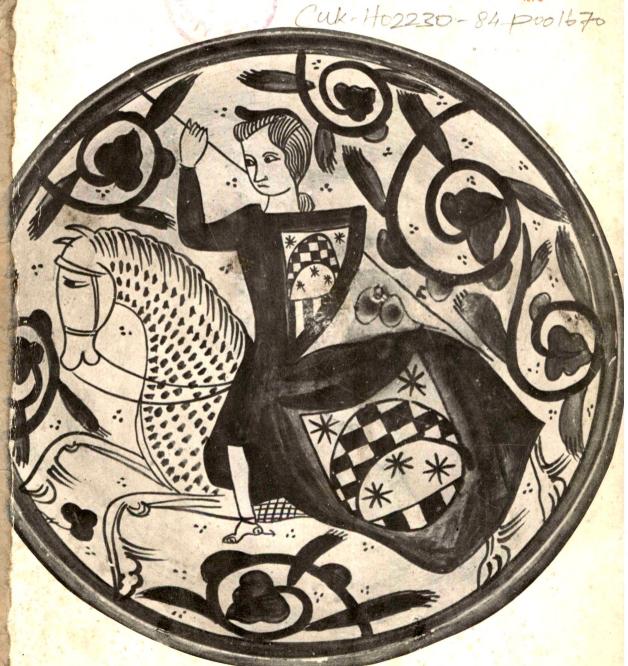
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Robert Wiebe in his provocative new examination of two centuries of social change in America. Arguing that "segmentation" is the fundamental enduring quality of our society, Wiebe shows how distinctive institutions have developed to accommodate and reinforce the tough, autonomous, competitive units that are the source of both the strength and the tensions of modern America.

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<sup>†</sup> Mr. Appleby died on December 19, 1974. An obituary will appear in the April issue. The staff of the AHR wish to record here the immense debt they—and the historical profession—owe to him for his wise and meticulous administration of the book reviewing operation and for his painstaking compilation of the indexes.

### The American Historical Review

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#### Presidential Address

American Historians and the World Today: Responsibilities and Opportunities, by Lewis Hanke

#### Articles

In	migrants from	Islam:	The C	rusaders'	Use	of Muslims	As.	Settlers
in	Thirteenth-Ce	ntury Sp	ain, by	Y ROBERT	I. BUI	RNS, S.J.		

21

Feminism in the French Revolution, BY JANE ABRAY

43

#### Reviews of Books

#### GENERAL

WILLIAM TODD. History as Applied Science: A Philosophical Study; WESLEY MORRIS. Toward a New Historicism. By Robert Rieke
PARDON E. TILLINGHAST. The Specious Past: Historians and Others. By Richard Reinitz 64
RADOSLAV A. TSANOFF. Civilization and Progress, W. WARREN WAGAR, ed. History and the Idea of Mankind. By David W. Noble 64
E. J. HOBSBAWM et al. Historical Studies Today. Ed. by FELIX GILBERT and STEPHEN R. GRAUBARD. By Pardon E. Tillinghast
ALFRED W. CROSBY, JR. The Columbian Exchange: Biological and Cultural Consequences of 1492. By Edward E. Barry, Jr. 67
RONALD H. CHILCOTE, ed. Protest and Resistance in Angola and Brazil: Comparative Studies. By Richard Graham 67
RONDO CAMERON, ed. Banking and Economic Development: Some Lessons of History.
By Jonathan R. T. Hughes 68

JOSEPH HABERER. Politics and the Community of Science. By Carroll Pursell 69

HERMAN H. GOLDSTINE. The Computer: From Pascal to von Neumann. By Harold Issadore Sharlin 69

GEORGE W. CORNER. Doctor Kane of the Arctic Seas. By Ronald C. Tobey 70

W. WARREN WAGAR. Good Tidings: The Belief in Progress from Darwin to Marcuse; RICHARD KING. The Party of Eros: Radical Social Thought and the Realm of Freedom. By Peter J. King 70

JAY MONAGHAN. Chile, Peru, and the California Gold Rush of 1849. By Frank Safford 72

JACK M. SCHICK. The Berlin Crisis, 1958–1962.

By Lawrence S. Kaplan 72

ROBERT M. SLUSSER. The Berlin Crisis of 1961: Soviet-American Relations and the Struggle for Power in the Kremlin, June-November 1961; ANATOL RAPO-PORT. The Big Two: Soviet-American Perceptions of Foreign Policy. By David S. Patterson 73

International Bibliography of Historical Sciences. Vols. 37-38, 1968-1969. Ed. by MICHEL FRANÇOIS and NICOLAS TOLU. By Oliver H. Orr, Jr. 74

ANCIENT  DEREK ROE. Prehistory: An Introduction; GRAHAME	Church Politics in the Mid-Twelfth Century: The Ecclesiology of Gratian's Decretum.  By Richard Kay 89
CLARK. Aspects of Prehistory. By T. Cuyler Young, Jr. 75	PERCY ERNST SCHRAMM. Kaiser, Könige und Päpste
KATHLEEN KENYON. Royal Cities of the Old Testament. By Cyrus H. Gordon 76	Gesammelte Aufsätze zur Geschichte des Mittelat ters. Vol. 4. Pts. 1, 2. By Richard E. Sullivan 89
CARL ANDRESEN. Einführung in die christliche Archäologie. By Edwin M. Yamauchi 77	HELMUT COING, ed. Handbuch der Quellen und Literatur der neueren europäischen Privatrechts geschichte. Vol. 1. By Janos M. Bak
M. I. FINLEY. Early Greece: The Bronze and Archaic Ages. By J. D. Muhly 78	JOHN H. A. MUNRO. Wool, Cloth, and Gold: The Struggle for Bullion in the Anglo-Burgundian
CHIARA PECORELLA LONGO. "Eterie" e gruppi politici nell'Atene del IV sec. a. C. By Alan L. Boegehold 79	Trade, 1340-1478. By Richard Vaughan 98 NICHOLAS ORME. English Schools in the Middle Ages By Pearl Kibre 98
K. D. WHITE. Roman Farming.	W. L. WARREN. Henry II. By Fred A. Cazel, Jr. 98
By J. Rufus Fears 79 R. M. OGILVIE. The Romans and Their Gods in the Age of Augustus. By James G. Harrison, Jr. 80	JOEL THOMAS ROSENTHAL. The Training of an Elite Group: English Bishops in the Fifteenth Century By Richard W. Pfaff
O. A. W. DILKE. The Roman Land Surveyors: An Introduction to the Agrimensores.  By Mason Hammond 81	JO ANN MCNAMARA. Gilles Aycelin: The Servant of Two Masters. By Charles T. Wood 95
DAVID P. JORDAN. Gibbon and His Roman Empire. By George W. Houston 81	christiane pierard. Les plus anciens comptes de le ville de Mons (1279–1356). Vols. 1, 2. By Bryce Lyon
G. M. DURANT. Britain, Rome's Most Northerly Province: A History of Roman Britain, A.D. 43- A.D. 450. By Donald A. White	HENRI HUGONNARD-ROCHE. L'oeuvre astronomique de Thémon Juif, maître parisien du XIV° siècle. By Sabetai Unguru
evangelos k. chrysos. To Vyzantion kai hoi Gotthoi: Symbolē eis tēn exōterikēn politikēn tou Vyzantiou kata ton tetartov aiona [Byzantium and the Goths: A Study of Byzantine Foreign Policy in	JEANNE LAURENT. Un monde rural en Bretagne an XV° siècle: La quévaise. By Robert S. Trullinger, Jr.
the 4th Century]. By John W. Barker 82	FRANK D. PRAGER and GUSTINA SCAGLIA. Mariano Tac cola and His Book De Ingeneis.
MEDIEVAL	By Bert S. Hall
HELMUT MAURER. Konstanz als ottonischer Bischofssitz: Zum selbstverständnis geistlichen Fürstentums im 10. Jahrhundert. By Bernard S. Smith 83	MODERN EUROPE  JOSÉ ANTONIO MARAVALL. Estado moderno y mentali
PAUL DE VOOCHT. Jacobellus de Stříbro (†1429): Premier théologien du hussitisme.	dad social (Siglos XV a XVII). In 2 vols. By Stanley G. Payne
By Paul L. Nyhus 84	sergio bertelli. Ribelli, libertini e ortodossi nelli storiografia barocca. By Grazia Avitabile
DAVID JACOBY. La féodalité en Grèce médiévale. Les "Assises de Romanie": Sources, application et diffusion. By Andrew Urbansky 84	JAMES M. STAYER. Anabaptists and the Sword. By Paul P. Bernard
CONSTANCE HEAD. Justinian II of Byzantium. By Angeliki E. Laiou 85	HARVEY MITCHELL and PETER N. STEARNS. Workers & Protest: The European Labor Movement, the Work
DONALD M. NICOL. The Last Centuries of Byzantium, 1261-1453. By Deno Geanakoplos 86	ing Classes and the Origins of Social Democracy 1890-1914. By Christopher H. Johnson 101
W. MONTGOMERY WATT. The Influence of Islam on Medieval Europe; W. MONTGOMERY WATT. The Formative Period of Islamic Thought.  By F. E. Peters 87	A. A. GUBER et al., eds. Rossiia i Italiia: Materialy IV Konferentsii sovetskikh i ital'ianskikh istorikov Rim 1969. Russkii i ital'ianskii srednevekovyi gorod Russko-ital'ianskie otnosheniia v 1900–1914 gg [Russia and Italy: Material from the 4th Confer
DEREK BAKER, ed. Relations between East and West in the Middle Ages. By Sidney R. Packard 88	ence of Soviet and Italian Historians, Rome 1969 The Russian and Italian Medieval City; Russian Italian Relations, 1900–1914].
FRIEDRICH PRINZ, Klerus und Krieg im früheren Mittelalter: Untersuchungen zur Rolle der Kirche beim Aufbau der Königsherrschaft. By Bennett D. Hill	By Roderick E. McGrew  KARL J. NEWMAN. European Democracy between the Wars. Tr. by KENNETH MORGAN.  By René Albrecht-Carrié  105
STANLEY CHODOROW. Christian Political Theory and	C. I. C. MOLONY et al. The Mediterranean and Middle

East. Vol. 5, The Campaign in Sicily, 1943, and the Campaign in Italy, 3rd September 1943 to 31st March 1944. By Louis Morton 103	DONALD HARMAN AKENSON. Education and Enmity: The Control of Schooling in Northern Ireland, 1920-50. By Joseph Woods
RALPH MERRIFIELD. Roman London; TIMOTHY BAKER.  Medieval London; MARTIN HOLMES. Elizabethan  London. By Donald J. Olsen 104	JOSEPH GOY and EMMANUEL LE ROY LADURIE, eds. Les fluctuations du produit de la dime: Conjoncture décimale et domanisle de la fin du Moyen Age au XVIIIº siècle. Communications et travaux.
PAUL CERNOVODEANU. England's Trade Policy in the Levant and Her Exchange of Goods with the Romanian Countries under the Latter Stuarts	By William F. Church 119 SALVO MASTELLONE. Venalità e machiavellismo in
(1660-1714). Tr. by MARY LÄZÄRESCU.  By James E. Farnell 105	Francia (1572–1610): All'origine della mentalità politica borghese. By J. Michael Hayden 120
ERIC KERRIDGE. The Farmers of Old England. By John G. Gazley 106	GÉRARD BOUCHARD. Le village immobile: Sennely- en-Sologne au XVIII° siècle.
ERNST SCHULIN. Handelsstaat England: Das po- litische Interesse der Nation am Aussenhandel vom 16. bis ins frühe 18. Jahrhundert. By David Clark	F. V. POTEMKIN. Promyshlennaia revoliutsia vo Frantsii [The Industrial Revolution in France]. Vol. 1, Ot manufaktury k fabrike [From Handi-
CONRAD RUSSELL, ed. The Origins of the English Civil War. By Margaret A. Judson 107	crafts to Factories]. By Lee Kennett 121  JOHN E. N. HEARSEY. Marie Antoinette.  By H. Arnold Barton 122
J. R. JONES. The Revolution of 1688 in England. By J. P. Kenyon 108	By H. Arnold Barton 122 GERLOF D. HOMAN. Jean-François Reubell: French Revolutionary, Patriot, and Director (1747-1807).
WOLFGANG JÄGER. Politische Partei und parlamen- tarische Opposition: Eine Studie zum politischen Danken von Lord Belingbrohe und David Hume	By Charles A. Le Guin 122
Denken von Lord Bolingbroke und David Hume. By Chester H. Kirby 109	MARC BOULOISEAU. Bourgeoisie et révolution: Les Du Pont de Nemours (1788-1799).  By Leo Gershoy 123
JOHN G. GAZLEY. The Life of Arthur Young, 1741– 1820. By David Spring	A. V. ADO. Krest'ianskoe dvizhenie vo Frantsii vo
The Correspondence of Edmund Burke. Vol. 9. Pt. 1, ed. by R. B. MCDOWELL; pt. 2, ed. by JOHN A. WOODS. By E. A. Reitan 110	vremia velikoi burzhuaznoi revoliutsii kontsa XVIII veka [The Peasant Movement in France at the Time of the Great Bourgeois Revolution at the End of the Eighteenth Century].
MICHAEL BROCK. The Great Reform Act. By Richard W. Davis	By Robert F. Byrnes 123
DAMIAN MCELRATH, O.F.M. Richard Simpson, 1820-1876: A Study in XIXth Century English Liberal	c. STEWART GILLMOR. Coulomb and the Evolution of Physics and Engineering in Eighteenth-Century France. By Harcourt Brown 124
Catholicism; FREDERICK J. CWIEKOWSKI, S.S. The English Bishops and the First Vatican Council.  By William J. Schoenl	RENÉ PICHELOUP. Les ecclésiastiques français émigrés ou déportés dans l'État Pontifical, 1792–1800. By Lynn Osen
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Century. By R. K. Webb	WILLIAM SCOTT. Terror and Repression in Revolutionary Marseilles. By William H. Sewell, Jr. 126
PHILIP WARNER. The Crimean War: A Reappraisal; R. L. V. FFRENCH BLAKE. The Crimean War. By Robin Higham	ALF ANDREW HEGGOY. The African Policies of Gabriel Hanotaux, 1894–1898.
PETER HARNETTY. Imperialism and Free Trade:	By Erving E. Beauregard 127
Lancashire and India in the Mid-Nineteenth Century; FRANCIS E. HYDE. Far Eastern Trade, 1860—1914. By Bernard Semmel	DOMINICK LACAPRA. Emile Durkheim: Sociologist and Philosopher. By John E. Talbott 127
ROBERT G. GREGORY. India and East Africa: A History of Race Relations within the British Empire, 1890–1939. By Robert L. Tignor	SUZANNE BERGER. Peasants against Politics: Rural Organization in Brittany, 1311-1967.  By Gilbert Allardyce 128
OZER CARMI. La Grande-Bretagne et la Petite Entente. By Stephen A. Schuker 116	JEAN-JACQUES BECKER. Le carnet B: Les pouvoirs publics et l'antimilitarisme avant la guerre de 1914. By Jonathan M. Houze
NICEL HARRIS. Competition and the Corporate Society: British Conservatives, the State, and Industry, 1945–1964. By Arnold A. Rogow 116	evelio verdera y tuells, ed. El Cardenal Albornoz y el Colegio de España. In 3 vols. By Erika Spivakovsky
L. M. CULLEN. An Economic History of Ireland since 1660. By Homer L. Calkin 117	N. CAULIER-MATHY. La modernisation des charbon- nages liégeois pendant la première moitié du XIXº

siècle: Techniques d'exploitation. By George Fasel 130	Populists in Belorussia (From the 1870s to the Early 1880s) ]. By Alan Kimball
OLE FELDBÆK. Dansk neutralitetspolitik under krigen 1778-1783: Studier i regeringens prioritering af politiske og økonomiske interesser [Denmark's Neutrality Policy during the War of 1778-1783: Studies in the Government's Assessment of Political and Economic Priorities]. By Erik J. Friis 131	P. F. LAPTIN. Obshchina v russkoi istoriografii poslednei treti XIX-nachala XX v. [Society in Russian Historiography in the Last Third of the 19th and the Beginning of the 20th Century].  By Henry S. Robinson 143
JOHNNY LEISNER et al., eds. Festskrift til Povl Bagge på halvfjerdsårsdagen, 30. November 1972: Fra en kreds af elever [A Festschrift for Povl Bagge on His Seventieth Birthday, 30 November 1972: From a Group of His Pupils]. By Raymond E. Lindgren	BRANKO LAZITCH and MILORAD M. DRACHKOVITCH.  Lenin and the Comintern. Vol. 1.  By Thomas T. Hammond 143  K. v. Gusev and v. p. Naumov, eds. Velikii Oktiabr' v rabotakh sovetskikh i zarubezhnykh istorikov [The Great October Revolution in the Work of
OTTO STEIGER. Studien zur Entstehung der Neuen Wirtschaftslehre in Schweden: Eine Anti-Kritik. By Walther Kirchner	Soviet and Foreign Historians]. By George D. Jackson  144  NAUM JASNY. Soviet Economists of the Twenties:
PATRICK RILEY, tr. and ed. The Political Writings of Leibniz. By H. R. Bernstein 133	Names to Be Remembered; RICHARD B. DAY. Leon Trotsky and the Politics of Economic Isolation. By Daniel Mulholland
HARTMUT KAELBLE. Berliner Unternehmer während der frühen Industrialisierung: Herkunft, sozialer Status und politischer Einfluss.  By Helen P. Liebel 134	NEAR EAST  OLEG CRÁBÁR. The Formation of Islamic Art.  By Ira Lapidus 146
ADOLF M. BIRKE, Bischof Ketteler und der deutsche Liberalismus: Eine Untersuchung über das Ver- hältnis des liberalen Katholizismus zum bürger- lichen Liberalismus in der Reichsgründungszeit. By Reginald H. Phelps	OTHMAR PICKL, ed. Die wirtschaftlichen Auswirkungen der Türkenkriege: Die Vorträge des 1. Internationalen Grazer Symposiuns zur Wirtschafts- und Sozialgeschichte Südosteuropas (5. bis 10. Oktober 1970). By Thomas M. Barker
GÜNTHER BORMANN and SIGRID BORMANN-HEISCHKEIL. Theorie und Praxis kirchlicher Organisation: Ein Beitrag zum Problem der Rückständigkeit sozialer Gruppen. By Gerald Strauss	STANLEY E. KERR. The Lions of Marash: Personal Experiences with American Near East Relief, 1919–1922. By Joseph L. Grabill
STANLEY B. KIMBALL. The Austro-Slav Revival: A Study of Nineteenth-Century Literary Foundations. By Jiri Kolaja 136	RIAZUL ISLAM. Indo-Persian Relations: A Study of the Political and Diplomatic Relations between the Mughul Empire and Iran. By Nosratollah Rassekh
ALEXANDER DUȚU. Cărțile de ințelepciune in cultura română [Books of Wisdom in Romanian Culture]. By Stephen Fischer-Galați 137	JON KIMCHE. There Could Have Been Peace. By John B. Christopher 149
Istoriia Rumynii [History of Romania]. Vol. 1, 1848–1917, ed. by V. N. VINOGRADOV et al.; vol. 2, 1918–1970, ed. by N. I. LEBEDEV et al. By Dragos D. Kostich	AFRICA  JAMES W. FERNANDEZ et al. Africa & the West:  Intellectual Responses to European Culture. Ed. by PHILIP D. CURTIN. By Lowell Ragatz 150
CHRISTOS THEODOULOU. Greece and the Entente, August 1, 1914–September 25, 1916. By Allan Cunningham	PROSSER GIFFORD and WM. ROGER LOUIS, eds. France and Britain in Africa: Imperial Rivalry and Co- lonial Rule. By Graham W. Irwin
Istoriia rabochikh Leningrada [History of the Leningrad Workers]. Vol. 1, 1703-fevral' 1917, ed. by s. N. VALK et al.; vol. 2, 1917-1965, ed. by A. R. DZENISKEVICH et al. By Robert W. Campbell 139	MICHAEL CROWDER. Revolt in Bussa: A Study of British 'Native Administration' in Nigerian Borgu, 1902–1935; BONIFACE I. OBICHERE. West African States and European Expansion: The Dahomey-Niger Hinterland, 1885–1898.
L. M. IVANOV and M. S. VOLIN, eds. Istoriia rabochego klassa Rossii, 1861–1900 gg. [History of the Working Class of Russia, 1861–1900]; IU. I. KIR'IANOV. Rabochie Iuga Rossii, 1914–fevral' 1917 g. [The	By L. Gray Cowan  BY L. Gray Cowan  BY Van Mitchell Smith  152
Workers of Southern Russia, 1914–February 1917]; A. A. MUKHIN. Rabochie Sibiri v epokhu kapitalizma (1861–1917 gg.) [Siberian Workers in the Period of Capitalism (1861–1917)].  By Reginald E. Zelnik 140	PHYLLIS M. MARTIN. The External Trade of the Loango Goast, 1576–1870: The Effects of Changing Commercial Relations on the Vili Kingdom of Loango. By Dorothy O. Helly
s. M. SAMBUK. Revoliutsionnye narodniki Belorussii	TADDESSE TAMRAT. Church and State in Ethiopia,

NORMAN H. POLLOCK, JR. Nyasaland and Northern Rhodesia: Corridor to the North. By Franklin Parker	DHARMAVIRA. Lala Har Dayal and Revolutionary Movements of His Times. By Barbara Ramusack
ADRIAN PRESTON, ed. The South African Diaries of Sir Garnet Wolseley, 1875; ADRIAN PRESTON, ed. The South African Journal of Sir Garnet Wolseley, 1879–1880. By William A. Thompson 156	JUDITH M. BROWN. Gandhi's Rise to Power: Indian Politics 1915–1922; FRANCIS G. HUTCHINS. India's Revolution: Gandhi and the Quit India Movement By Stephen Hay
ASIA AND THE EAST	NALINI RANJAN CHAKRAVARTI. The Indian Minority in Burma: The Rise and Decline of an Immigrant
SILAS H. L. WU. Communication and Imperial Control in China: Evolution of the Palace Memorial System, 1693–1735. By E-tu Zen Sun 157	DAVID JOEL STEINBERG et al. In Search of Southeast Asia: A Modern History. Ed. by DAVID JOEL STEIN-
W. E. WILLMOTT, ed. Economic Organization in Chinese Society. By Robert M. Hartwell 158	JEAN CHESNEAUX et al., eds. Tradition et révolution
JEAN CHESNEAUX. Secret Societies in China in the Nineteenth and Twentieth Centuries. Tr. by GILLIAM NETTLE; LUCIEN BIANCO et al. Popular Movements and Secret Societies in China, 1840-1950. Ed. by JEAN CHESNEAUX.  By H. Arthur Steiner 159	au Vietnam. By John K. Whitmore  ANTONIO DE MORGA. Sucesos de las Islas Filipinas Tr. and ed. by J. s. CUMMINS. By Peter W. Stanley  176
JESSIE GREGORY LUTZ. China and the Christian Colleges, 1850–1950; SIDNEY A. FORSYTHE. An American Missionary Community in China, 1895–1905.	PHILIP LOH FOOK SENG. The Malay States, 1877-1895: Political Change and Social Policy. By D. R. SarDesai 176
By Yi Chu Wang 160  E. A. BELOV. Uchanskoe vosstanie v Kitae (1911 g.) [The Wu-ch'ang Revolt in China (1911)]. By Eric Widmer 161	H. C. BROOKFIELD. Colonialism, Development and Independence: The Case of the Melanesian Island in the South Pacific. By W. Patrick Strauss
ROGER V. DES FORGES. Hsi-liang and the Chinese National Revolution; K. S. LIEW. Struggle for De- mocracy: Sung Chiao-jen and the 1911 Chinese Revolution. By Michael Gasster 162	UNITED STATES  JOHN E. POMFRET. Colonial New Jersey: A History By Albright G. Zimmerman 178
KENNETH E. SHEWMAKER. Americans and Chinese Communists, 1927-1945: A Persuading Encounter. By Warren I. Cohen 163	BRUCE E. STEINER. Samuel Seabury, 1729-1796: A. Study in the High Church Tradition.  By Cecil B. Currey
HUNGDAH CHIU. The People's Republic of China and the Law of Treaties. By T. C. Rhee 164	DIRK HOERDER. Society and Government 1760-1780 The Power Structure in Massachusetts Townships
BERTOLD SPULER. The Mongols in History. Tr. by GEOFFREY WHEELER; J. J. SAUNDERS. The History of the Mongol Conquests; BERTOLD SPULER. History of the Mongols: Based on Eastern and Western Accounts of the Thirteenth and Fourteenth Centuries. Tr. by HELGA and STUART DRUMMOND.  By Romeyn Taylor 165	By Robert Zemsky  MARVIN KALB and ELIE ABEL. Roots of Involvement The U.S. in Asia, 1784-1971. By Theodore Friend  186  RICHARD R. BEEMAN. The Old Dominion and th
ROBERT H. C. LEE. The Manchurian Frontier in Ching History. By Michael H. Hunt 166	New Nation, 1788-1801. By Ralph Ketcham 18.  NATHAN REINCOLD et al., eds. The Papers of Joseph Hammy Vol. 1. December 1870. The
W. G. BEASLEY. The Meiji Restoration; DAVID WUR- FEL, ed. Meiji Japan's Centennial: Aspects of Po- litical Thought and Action. By John Whitney Hall	Henry. Vol. 1, December 1797-October 1832: The Albany Years. By I. Bernard Cohen 18:  FRANCIS N. STITES. Private Interest & Public Gain The Dartmouth College Case, 1819; STANLEY I KUTLER. Privilege and Creative Destruction: The Challed Privilege and Creative Destruction: The
NOBUYA BAMBA. Japanese Diplomacy in a Dilemma: New Light on Japan's China Policy, 1924–1929. By G. Ralph Falconeri	Charles River Bridge Case.  By James McClellan  18:
HAN WOO-KEUN. The History of Korea. Tr. by LEE KYUNG-SHIK. Ed. by GRAFTON K. MINTZ; WILLIAM E. HENTHORN. A History of Korea. By John K. C. Oh	RONALD P. FORMISANO. The Birth of Mass Politica Parties: Michigan, 1827-1861; MARVIN E. GETTLE MAN. The Dorr Rebellion: A Study in American Radicalism: 1833-1849. By David Curtis Skaggs
M. A. LAIRD, ed. Bishop Heber in Northern India: Selections from Heber's Journal. By R. F. Frykenberg	ROBERT J. PARKS. Democracy's Railroads: Publi Enterprise in Jacksonian Michigan.

william Gerald shade. Banks or No Banks: The Money Issue in Western Politics, 1832-1865. By Clark C. Spence	the National Interest.
ALAN DOWTY. The Limits of American Isolation: The United States and the Crimean War. By Kenneth J. Hagan	Men in the Recent South.
ANNE L. AUSTIN. The Woolsey Sisters of New York: A Family's Involvement in the Civil War and a New Profession (1860–1900).	
By Louise M. Young 187  MILTON PLESUR. America's Outward Thrust: Ap-	and Global Politics in the Missile Age.
proaches to Foreign Affairs, 1865-1890.  By O. Lawrence Burnette, Jr. 188	HARRY EDWARD GRAHAM. The Paper Rebellion: De-
ALWYN BARR. Reconstruction to Reform: Texas Politics, 1876-1906. By James A. Tinsley 188	Unionism. By Stephen J. Scheinberg 197
carl H. Chrislock. The Progressive Era in Min- nesota, 1899–1918; DAVID P. THELEN. The New Citizenship: Origins of Progressivism in Wisconsin,	CANADA
1885-1900. By Charles Forcey 189 YONATHAN SHAPIRO. Leadership of the American	IRVING M. ABELLA et al. The Influence of the United States on Canadian Development: Eleven Case
Zionist Organization, 1897–1930; SAUL S. FRIEDMAN. No Haven for the Oppressed: United States Policy toward Jewish Refugees, 1938–1945.	By Denis Smith 198
By Moses Rischin 190	LATIN AMERICA
ROBERT B. CARSON. Main Line to Oblivion: The Disintegration of New York Railroads in the Twentieth Century. By Richard C. Overton 191	Mexica Aztecs. By G. Micheal Riley 100
ARNOLD H. TAYLOR. American Diplomacy and the Narcotics Traffic, 1900–1939: A Study in Interna- tional Humanitarian Reform.	
By Calvin D. Davis	CARMEN VENEGAS RAMIREZ. Regimen hospitalario
Vol. 1, The Attitude of American Jews to World War I, the Russian Revolutions of 1917, and Com-	By Donald B. Cooper 200
munism (1914–1945). By David W. Levy 192 JOSEPH R. STAROBIN. American Communism in Crisis	antensystems in New Spanien im Rahmen der
1943–1957. By Harvey Levenstein 198	Monarchie im 18. Jahrhundert.
ernest R. MAY. "Lessons" of the Past: The Use and Misuse of History in American Foreign Policy,	
	By Woodrow Borah 201
THOMAS M. FRANCK and EDWARD WEISBAND. Word Politics: Verbal Strategy among the Superpowers By Richard L. Merritt	By Woodrow Borah 201  WINFIELD J. BURGGRAAFF. The Venezuelan Armed Forces in Politics, 1935-1959.
THOMAS M. FRANCK and EDWARD WEISBAND. Word Politics: Verbal Strategy among the Superpowers.	By Woodrow Borah 201  WINFIELD J. BURGGRAAFF. The Venezuelan Armed Forces in Politics, 1935-1959.
THOMAS M. FRANCK and EDWARD WEISBAND. Word Politics: Verbal Strategy among the Superpowers.	By Woodrow Borah  201  WINFIELD J. BURGGRAAFF. The Venezuelan Armed Forces in Politics, 1935–1959.  By Glenn E. Miller  201
THOMAS M. FRANCK and EDWARD WEISBAND. Word Politics: Verbal Strategy among the Superpowers. By Richard L. Merritt	By Woodrow Borah  winfield J. Burggraaff. The Venezuelan Armed Forces in Politics, 1935–1959. By Glenn E. Miller  Other Books Received  215
THOMAS M. FRANCK and Politics: Verbal Strategy By Richard L. Merritt  Communications  EDWARD WEISBAND. Word among the Superpowers.  194	By Woodrow Borah  WINFIELD J. BURGGRAAFF. The Venezuelan Armed Forces in Politics, 1935–1959.  By Glenn E. Miller  Other Books Received  215  Recently Published Articles  224

## The American Historical Review

THE AMERICAN HISTORICAL ASSOCIATION

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#### Article

The Limits of Quantitative Methods in History, BY ROBERT WILLIAM FOGEL

329

#### Review Article

Quantitative History, BY CHARLOTTE ERICKSON

351

WILLIAM O. AYDELOTTE. Quantification in History

LEE BENSON. Toward the Scientific Study of History: Selected Essays

WILLIAM O. AYDELOTTE et al. The Dimensions of Quantitative Research in History. Ed. by WILLIAM O. AYDELOTTE, ALLAN G. BOGUE, and ROBERT W. FOGEL

VAL R. LORWIN and JACOB M. PRICE, eds. The Dimensions of the Past: Materials, Problems, and Opportunities for Quantitative Work in History

E. A. WRIGLEY, ed. Nineteenth-Century Society: Essays in the Use of Quantitative Methods for the Study of Social Data

#### Reviews of Books

372

#### GENERAL

GÉZA ALFÖLDY et al., eds. Probleme der Geschichtswissenschaft. By Michael P. Speidel 366

EMMANUEL LE ROY LADURIE. Le territoire de l'historien. By Franklin L. Ford 366

ARTHUR RAISTRICK. Industrial Archaeology: An Historical Survey. By Robert E. Carlson . 367

MARCELLE KOOY, ed. Studies in Economics and Economic History: Essays in Honour of Professor H. M. Robertson. By J. E. Butler 368

EDUARD VAN DEN BRINK. Rooms of katholiek: De opvattingen van Christopher Dawson over kultuur en religie. By Stephen B. Baxter 369

G. J. CUMING and DEREK BAKER, eds. Popular Belief and Practice: Papers Read at the Ninth Summer Meeting and the Tenth Winter Meeting of the Ecclesiastical History Society. By H. Boone Porter, Jr. 369

ROGER BASTIDE. African Civilisation in the New World. Tr. by PETER GREEN.
By William L. Bowers 370

M. R. D. FOOT. War and Society: Historical Essays in Honour and Memory of J. R. Western, 1928-1971. By George O. Kent

RAM LAKHAN SHUKLA. Britain, India and the Turkish Empire, 1853-1882. By Gail Minault 371

R. V. SAMPSON. The Discovery of Peace. By Sandi E. Cooper

CHARLES P. KINDLEBERGER. The World in Depression, 1929–1939. By Martin Wolfe 372

DAVID W. WAINHOUSE. International Peacekeeping at the Crossroads: National Support—Experience and Prospects. By Ruhl J. Bartlett 373

ANDREW W. CORDIER and WILDER FOOTE, eds. Public Papers of the Secretaries-General of the United Nations. Vol. 3, Dag Hammarskjöld, 1956–1957.
By William Epstein 373

#### ANCIENT

v. M. MASSON and v. I. SARIANIDI. Central Asia: Turkmenia before the Achaemenids. Tr. and ed. by RUTH TRINGHAM. By Louis D. Levine 375

RALPH S. SOLECKI. Shanidar: The First Flower People. By T. Cuyler Young, Jr. 375

ANGEL CABO and MARCELO VIGIL. Condicionamientos geográficos; Edad antigua. By C. J. Bishko 376

CLAUDE MOSSÉ. Athens in Decline, 505-86 B.C. Tr. by JEAN STEWART. By Truesdell S. Brown 377

PHILIP A. STADTER, ed. The Speeches in Thucydides: A Collection of Original Studies with a Bibliography. By Chester G. Starr 377

PIERRE BRIANT. Antigone le Borgne: Les débuts de sa

carrière et les problèmes de l'assemblée macédonienne. By Glanville Downey 378

ALAN CAMERON. Porphyrius the Charioteer. By Thomas W. Africa

#### MEDIEVAL

378

ROBERT DELORT. Life in the Middle Ages. Tr. by ROBERT ALLEN. By Joseph Dahmus 379

JOHN T. MCNEILL. The Celtic Churches: A History, A.D. 200 to 1200. By Jeffrey B. Russell 379

GEOFFREY ASHE. Camelot and the Vision of Albion; RICHARD BARBER. The Figure of Arthur.

By Donald A. White

A. CAMPBELL, ed. Charters of Rochester. By Ralph V. Turner 381

HANNA VOLLRATH-REICHELT. Königsgedanke und Königtum bei den Angelsachsen: Bis zur Mitte des 9. Jahrhunderts. By William A. Chaney 381

V. PAŠUTA. Lietuvos valstybės susidarymas [The Formation of the Lithuanian State].
By Benedict V. Maciuika 982

M. H. KEEN. England in the Later Middle Ages: A Political History. By Kenneth G. Madison 383

HELENA M. CHEW and WILLIAM KELLAWAY, eds. London Assize of Nuisance, 1301-1431.

By Margaret Hastings

384

ROBERT CHAZAN. Medieval Jewry in Northern France: A Political and Social History. By Joseph R. Strayer 385

JOHN HOLLAND SMITH. Joan of Arc. By Fredric L. Cheyette

By Fredric L. Cheyette 385 LOUIS B. PASCOE, s. J. Jean Gerson: Principles of

Church Reform. By Roy M. Haines 386
PAUL JOHANSEN and HEINZ VON ZUR MÜHLEN. Deutsch

und Undeutsch im mittelalterlichen und frühneuzeitlichen Reval. By Peep Peter Rebane 386

ERNST BOCK, ed. Deutsche Reichstagsahten unter Maximilian I. Vol. 3, 1488–1490. In 2 pts. By Steven W. Rowan 387

JUAN CARRASCO PÉREZ. La población de Navarra en el siglo XIV. By Bernard F. Reilly 388

EDWARD A. ARMSTRONG. Saint Francis: Nature Mystic. The Derivation and Significance of the Nature Stories in the Franciscan Legend.

By Rosiland B. Brooke 388

v. b. Koroliuk et al., eds. Issledovaniia po istorii slavianskikh i balkanskikh narodov. Epokha srednevekov'ia. Kievskaia Rus' i ee slavianskie sosedi [Studies in the History of the Slavic and Balkan Nations. Era of the Middle Ages. Kievan Rus' and Its Slavic Neighbors]. By Imre Boba 389

APOSTOLOS ATH. GLAVINAS. Hē epi Alexiou Komnēnou (1081–1118) peri hieron, skeuon, keimēlion kai agion eikonon eris (1081–1095) [The Controversy

416

during the Reign of Alexius Comnenus (1081–1118) over Sacred Vessels, Gems, and Holy Pictures (1081–1095)]. By Pierre A. MacKay 390	ANTHONY ARMSTRONG. The Church of England, the Methodists and Society, 1700-1850.  By James Obelkevich 405
J. OTTO MAENCHEN-HELFEN. The World of the Huns: Studies in Their History and Culture. Ed. by MAX KNIGHT. By Duncan Fishwick 390	R. F. BRISSENDEN, ed. Studies in the Eighteenth Century. Vol. 2, Papers Presented at the Second David Nichol Smith Memorial Seminar, Canberra, 1970.  By John H. Middendorf
FRANZ GEORG MAIER. Byzanz. By John N. Frary 391	NEIL R. STOUT. The Royal Navy in America, 1760-
NICÉTAS MAGISTROS. Lettres d'un exilé (928-946). Ed. and tr. by L. c. WESTERINK. By Walter Emil Kaegi, Jr. 392	1775: A Study of Enforcement of British Colonial Policy in the Era of the American Revolution. By Carl Ubbelohde 406
Travaux et mémoires. Vol. 5. By George J. Marcopoulos 393	DOROTHY MARSHALL. Industrial England, 1776–1851. By Thomas Milton Kemnitz 406
	ELEANOR FLEXNER. Mary Wollstonecraft: A Biography. By Lee Holcombe 407
MODERN EUROPE	F. L. VAN HOLTHOON. The Poad to Utopia: A Study
LOUISE CUYLER. The Emperor Maximilian I and Music. By Denis Stevens 394	of John Stuart Mill's Social Thought.  By Gertrude Himmelfarb 407
HANS J. HILLERBRAND. The World of the Reforma- tion. By Harold J. Grimm 394	R. J. OLNEY. Lincolnshire Politics, 1832-1885. By David Spring 408
SALO WITTMAYER BARON. A Social and Religious History of the Jews: Late Middle Ages and Era of European Expansion, 1200–1650. Vol. 15, Resettle-	L. F. TUPOLEVA. Sotsialisticheskoe dvizhenie v Anglii v 80-e gody XIX veka [The Socialist Movement in England in the 1880s]. By Paul B. Johnson 409
ment and Exploration. By Richard H. Popkin 395  ALAN G. R. SMITH. Science and Society in the Sixteenth and Seventeenth Centuries.	FRANK MILLER TURNER. Between Science and Religion: The Reaction to Scientific Naturalism in Late Victorian England. By W. F. Cannon 409
By Glen R. Driscoll 397	EDWARD SCOBIE. Black Britannia: A History of Blacks
HOWELL A. LLOYD. The Rouen Campaign, 1590-1592: Politics, Warfare and the Early-Modern State. By Herbert H. Rowen 398	in Britain. By Robin W. Winks 410  A. K. RUSSELL. Liberal Landslide: The General Elec-
LUZI SCHUCAN. Das Nachleben von Basilius Magnus	tion of 1906. By Trevor Lloyd 410  L. P. CARPENTER. G. D. H. Cole: An Intellectual
"ad adolescentes": Ein Beitrag zur Geschichte des christlichen Humanismus. By John P. Cavarnos	Biography. By P. F. Clarke 411  A. J. SHERMAN. Island Refuge: Britain and Refugees
wolfgang müller et al. Die Kirche im Zeitalter des Absolutismus und der Aufklärung.	from the Third Reich, 1933–1939.  By George L. Mosse  412
By William J. Bouwsma 399	W. N. MEDLICOTT et al., eds. Documents on British
KARL F. HELLEINER. Free Trade and Frustration: Anglo-Austrian Negotiations, 1860-70. By Edith M. Link 400	Foreign Policy, 1919–1939. 2d ser. Vol. 13, Naval Policy and Defence Requirements, July 20, 1934– March 25, 1936. By Christopher Thorne
KEITH MALLORY and ARVID OTTAR. The Architecture of War. By B. Franklin Cooling 400	SIR LLEWELLYN WOODWARD. British Foreign Policy in the Second World War. Vol. 3. By Matthew A. Fitzsimons 414
OSWALD HAUSER. England und das Dritte Reich: Eine dokumentierte Geschichte der englisch-deutschen	F. X. MARTIN and F. J. BYRNE, eds. The Scholar Rev-
Beziehungen von 1933 bis 1939, auf Grundunveröffentlichter Akten aus dem britischen Staatsarchiv.	olutionary: Eoin MacNeill, 1867-1945, and the Making of the New Ireland. By Joseph M. Curran 414
Vol. 1. By A. J. Sherman 401  E. E. RICH, ed. St Catharine's College, Cambridge,	PIERRE ROCOLLE. 2000 ans de fortification française. Vols. 1, 2. By Horst de la Croix 415
1473-1973: A Volume of Essays to Commemorate the Quincentenary of the Foundation of the Col-	YVES CAZAUX. Jeanne d'Albret. By Raymond F. Kierstead 416
lege. By Sheldon Rothblatt 402	JULIAN DENT. Crisis in Finance: Crown, Financiers
A. P. MCGOWAN, ed. The Jacobean Commissions of Enquiry, 1608 and 1618. By Clive Holmes 402	and Society in Seventeenth-Century France.  By Leon Bernard 416

403

404

G. R. R. TREASURE. Cardinal Richelieu and the Devel-

opment of Absolutism; JEAN-LOUIS THIREAU. Les idées politiques de Louis XIV. By Orest Ranum 417

PAUL W. BAMFORD. Fighting Ships and Prisons: The

ANTONIA FRASER. Cromwell: The Lord Protector.

GERALD M. and LOIS O. STRAKA. A Certainty in the

By Maurice Lee, Jr.

Succession. By Ian R. Christie

Mediterranean Galleys of France in the Age of Louis XIV. By Robert M. Isherwood 417	Wahlenthaltung in historischer, politischer und statistischer Sicht; HERBERT KÜHR. Parteien und Wahlen im Stadt, und Landkreis Essen in der Zeit
J. Q. C. MACKRELL. The Attach on 'Feudalism' in Eighteenth-Century France. By Orville T. Murphy 418	Wahlen im Stadt- und Landkreis Essen in der Zeit der Weimarer Republik: Unter besonderer Berück- sichtigung des Verhältnisses von Sozialstruktur und politischen Wahlen. By Allan Mitchell 430
MARK POSTER. The Utopian Thought of Restif de la Bretonne. By J. Robert Vignery 419	KARL HOLL and ADOLF WILD, eds. Ein Demokrat Kommentiert Weimar: Die Berichte Hellmut von
SHIRLEY M. GRUNER. Economic Materialism and Social Moralism: A Study of the History of Ideas in France from the Latter Part of the 18th Century to the	York, 1922-1930. By George W. F. Hallgarten 431
Middle of the 19th Century. By A. Lloyd Moote 419	KLAUS HILDEBRAND. The Foreign Policy of the Third Reich. Tr. by ANTHONY FOTHERGILL. By Marshall M. Lee 432
STEVEN T. ROSS. Quest for Victory: French Military Strategy 1792-1799. By Gordon H. McNeil 420	MARTIN L. VAN CREVELD. Hitler's Strategy, 1940-1941:
ÉPHRAÎM HARPAZ, ed. Benjamin Constant et Goyet de la Sarthe: Correspondance, 1818–1822.	The Balkan Clue. By Alexander Dallin 482 WINFRIED SCHULZE. Landesdefension und Staats-
By Alexander Sedgwick 420  Histoire de l'administration; ALBERT MABILEAU, ed.	bildung: Studien zum Kriegswesen des inneröster- reichischen Territorialstaates (1564–1619). By Gordon A. Craig 433
Les facteurs locaux de la vie politique nationale.  By E. W. Fox  421	HORST BRETTNER-MESSLER, ed. Die Protokolle des
THEODORE ZELDIN. France 1848–1945. Vol. 1, Ambition, Love and Politics. By Gordon Wright 422	österreichischen Ministerrates, 1848–1867. Pt. 6. Vol. 2. By Richard B. Elrod 434
J. P. T. BURY. Gambetta and the Making of the Third Republic. By Raymond F. Betts 423	B. N. FLORIA. Russko-pol'skie otnosheniia i baltiiskii vopros v kontse XVI-nachale XVII v. [Russian-Polish Relations and the Baltic Question at the End
FERNAND GAMBIEZ. Libération de la Corse. By Jere Clemens King 423	of the 16th and the Beginning of the 17th Century]. By Robert O. Crummey 434
FINN GAD. The History of Greenland. Vol. 2. By Archibald R. Lewis 424	ATHANASIOS A. ANGELOPOULOS. Hai zenai propagandai eis tēn eparchian Poluanēs kata tēn periodon 1870—1912 [Foreign Propaganda in the Province of
A. P. LAIDINEN. Ocherki istorii Finliandii vtoroi poloviny XVIII v. [Essays on the History of Finland	Poluane, 1870-1912]. By George D. Frangos 435
in the Second Half of the 18th Century]. By John H. Hodgson  425	PATRICIA KENNEDY GRIMSTED. Archives and Manuscript Repositories in the USSR: Moscow and Leningrad. By John Keep 436
KEIJO ELIO. Otto Kaarle von Fieandt—Suomalainen upseerikouluitaja [Otto Kaarle von Fieandt—Fin- nish Officer Trainer]. By John I. Kolehmainen 425	Recent Studies in Modern Armenian History. By Vartan H. Artinian 436
MICHAEL NEUMÜLLER. Liberalismus und Revolution: Das Problem der Revolution in der deutschen lib-	J. G. CARRARD, ed. The Eighteenth Century in Russia. By John T. Alexander 437
eralen Geschichtsschreibung des 19. Jahrhunderts. By Charles E. McClelland 426	MARG VUILLEUMIER et al., eds. Autour d'Alexandre Herzen: Documents inédits. By Abbott Gleason 438
ADAM WANDRUSZKA and PETER URBANITSCH, eds. Die Habsburgermonarchie 1848-1918. Vol. 1, Die wirtschaftliche Entwicklung, ed. by ALOIS BRUSATTI. By Robert A. Kann 426	ALLEN SINEL. The Classroom and the Chancellery: State Educational Reform in Russia under Count Dmitry Tolstoi. By Linda Gerstein 439
WERNER KAEGI. Jacob Burchhardt; Eine Biographie. Vol. 5, Das neuere Europa und das Erlebnis der Gegenwart. By Felix Wassermann 428	M. G. VANDALKOVSKAIA. M. K. Lemke—Istorik russkogo revoliutsionnogo dvizheniia [M. K. Lemke—Historian of the Russian Revolutionary Movement]; v. s. VASIUKOV, ed. Kritika burzhuaznoi istoriografii
NICHOLAS MARTIN HOPE. The Alternative to German Unification: The Anti-Prussian Party, Frankfurt, Nassau, and the Two Hessen, 1859–1867.	sovetskogo obshchestva [A Critique of the Bourgeois Historiography of Soviet Society]. By Robert C. Williams 440
By John W. Cranston 428	STEPHEN F. COHEN. Bukharin and the Bolshevik Rev-
WALTER STRUVE. Elites against Democracy: Leader- ship Ideals in Bourgeois Political Thought in Ger-	olution: A Political Biography, 1888–1938. By Adam B. Ulam 440
many, 1890-1933. By Andreas Dorpalen 429	NICOLAS DE BASILY. Diplomat of Imperial Russia, 1903-1917: Memoirs. By Barbara Jelavich 441
SEBASTIAN HAFFNER. Failure of a Revolution: Germany 1918-19. Tr. by GEORG RAPP.  By Lewis D. Wurgaft 430	ALFRED LEVIN. The Third Duma, Election and Pro- file. By Stephan M. Horak 442
RALF-RAINER LAVIES. Nichtwählen als Kategorie des Wahlverhaltens: Empirische Untersuchung zur	C. VAUGHAN JAMES. Soviet Socialist Realism: Origins and Theory. By Roy D. Laird 443

P. A. ZHILIN et al., eds. Vtoraia mirovaia voina i sovremennost' [The Second World War and the Present]. By John A. Armstrong 443	3
NEAR EAST	
ARTHUR RUPPIN. Memoirs, Diaries, Letters. Ed. by ALEX BEIN. Tr. by KAREN GERSHON. By Ellis Rivkin 444	]
JACQUES THOBIE. Phares ottomans et emprunts turcs, 1904-1961: Un type de règlement financier international dans le cadre des traités.  By Roderic H. Davison 445	1
MICHAEL LLEWELLYN SMITH. Ionian Vision: Greece in Asia Minor, 1919–1922.  By John A. Nicolopoulos 445	1
AFRICA	]
DOUGLAS FRASER and HERBERT M. COLE, eds. African Art & Leadership. By K. David Patterson 446	1
VIRGINIA THOMPSON. West Africa's Council of the Entente. By Henry S. Wilson 447	ì
A second As There is the Title And A And	•

A. G. HOPKINS. An Economic History of West Africa.

By George E. Brooks

447

NEHEMIA LEVIZION. Ancient Ghana and Mali.

By Brian M. Fagan

448

FRANÇOIS RENAULT. Lavigerie, l'esclavage africain et

l'Europe, 1868-1892. Vols. 1, 2. By J. R. Hooker 449 VICTOR T. LE VINE. The Cameroon Federal Republic. By Frederick Quinn 450

MARCIA WRIGHT, German Missions in Tanganyika, 1891–1941: Lutherans and Moravians in the Southern Highlands. By Robert W. Strayer 451

SAMUEL G. AYANY. A History of Zanzibar: A Study in Constitutional Development, 1934–1964.
By Norman Robert Bennett
451

PETER M. GUKIINA. Uganda: A Case Study in African Political Development. By J. E. Lamphear 452

ROBERT C. GOOD. U.D.İ.: The International Politics of the Rhodesian Rebellion. By L. H. Gann 453

RICHARD GIBSON. African Liberation Movements: Contemporary Struggles against White Minority Rule. By Gwendolen M. Carter 454

ERIG AXELSON. Portuguese in South-East Africa, 1488-1600. By Francis M. Rogers 455

EDWARD C. TABLER. Pioneers of South West Africa and Ngamiland, 1738-1880. By Leslie Clement Duly 456

PETER WALSHE. The Rise of African Nationalism in South Africa: The African National Congress 1912–1952; MONICA WILSON and LEONARD THOMPSON, eds. The Oxford History of South Africa. Vol. 2. By Harrison M. Wright

#### ASIA AND THE EAST

KENNETH K. S. CH'EN. The Chinese Transformation of Buddhism. By E. G. Pulleyblank 457

ARTHUR F. WRIGHT and DENIS TWITCHETT, eds. Perspectives on the T'ang. By Lien-sheng Yang 458

JOSEPH NEEDHAM. Science and Civilisation in China. Vol. 4, Physics and Physical Technology. Pt. 3, Civil Engineering and Nautics. By Kan Lao 459

MICHEL CARTIER. Une réforme locale en Chine au XVI° siècle: Hai Rui à Chun'an, 1558-1562.
By James B. Parsons
461

EDWARD V. GULICK. Peter Parker and the Opening of China. By Paul A. Varg 461

RICHARD C. THORNTON. China, the Struggle for Power 1917-1972. By John L. Rawlinson 462

JOHN HUNTER BOYLE. China and Japan at War, 1937-1945: The Politics of Collaboration; GERALD E. BUNKER. The Peace Conspiracy: Wang Ching-wei and the China War, 1937-1941.

By Frank N. Trager

462

HELEN FOSTER SNOW (NYM WALES). The Chinese Communists: Shetches and Autobiographies of the Old Guard. Bks. 1, 2; JACQUES GUILLERMAZ. A History of the Chinese Communist Party, 1927-1949. Tr. by ANNE DESTENAY; PETER R. MOODY, JR. The Politics of the Eighth Central Committee of the Communist Party of China. By Pierre M. Perrolle

464

JOYCE C. LEBRA. Ōhuma Shigenobu: Statesman of Meiji Japan. By Edwin B. Lee 465

HENRY DEWITT SMITH, II. Japan's First Student Radicals. By Francis L. K. Hsu. 466

BENJAMIN C. DUKE. Japan's Militant Teachers: A History of the Left-Wing Teachers' Movement; DONALD R. THURSTON. Teachers and Politics in Japan. By Jackson H. Bailey 467

S. R. RAO. Lothal and the Indus Civilization. By Kenneth A. R. Kennedy

MAURICE HENNESSY. The Rajah from Tipperary. By Edward B. Jones 469

B. G. GAFUROV. Tadzhiki: Drevneishaia, drevniaia i srednevekovaia istoriia [Tadjiks: Prehistoric, Ancient and Medieval History].

By Richard N. Frye 469

HAFEEZ MALIK, ed. Iqbal: Poet-Philosopher of Pakistan. By Walter Hauser 470

GERD LINDE. Burma 1943 und 1944: Die Expeditionen Orde C. Wingates. By Justus M. Van Der Kroef 470

PETER BOLGER. Hobart Town. By A. Stanley Trickett

471

468

BEDE NAIRN. Civilising Capitalism: The Labor Movement in New South Wales, 1870-1900. By Samuel Clyde McCulloch 472

#### UNITED STATES

EDWARD A. PURCELL, JR. The Crisis of Democratic Theory: Scientific Naturalism & the Problem of Value. By Jay Mechling 473

WILLIAM H. CARTWRIGHT and RIGHARD L. WATSON, JR., eds. The Reinterpretation of American History and Culture; TIMOTHY PAUL DONOVAN. Historical Thought

in America: Postwar Patterns; GENE WISE. American Historical Explanations: A Strategy for Grounded Inquiry. By David A. Hollinger 474	L. H. BUTTERFIELD and MARC FRIEDLAENDER, eds.  Adams Family Correspondence. Vols. 3, 4.  By Edmund S. Morgan 490
EDWIN T. LAYTON, JR. The Revolt of the Engineers: Social Responsibility and the American Engineer- ing Profession. By Kendall Birr 476	FREDERICK W. MARKS III. Independence on Trial: Foreign Affairs and the Making of the Constitution. By Jerald A. Combs
MARY C. HENDERSON. The City and the Theatre: New York Playhouses from Bowling Green to Times Square. By Henry Hope Reed 476	ARTHUR M. SCHLESINGER, JR., gen. ed. History of U.S. Political Parties. Vols. 1-4. By J. Rogers Hollingsworth 492
WILLIAM T. GENEROUS, JR. Swords and Scales: The Development of the Uniform Code of Military Justice. By Charles A. Leonard 477	ALLAN R. PRED. Urban Growth and the Circulation of Information: The United States System of Cities, 1790-1840. By Thomas C. Cochran 494
C. ROBERT KEMBLE. The Image of the Army Officer in America: Background for Current Views. By I. B. Holley, Jr. 478	HAROLD C. SYRETT et al., eds. The Papers of Alexander Hamilton. Vols. 18, 19. By Manning J. Dauer 495
ROBERT C. NESBIT. Wisconsin: A History. By Frederick I. Olson 478	JOHN A. MUNROE. Louis McLane: Federalist and Jacksonian. By Charles M. Wiltse 496
ROBERT F. HEIZER and ALAN J. ALMQUIST. The Other Californians: Prejudice and Discrimination under Spain, Mexico, and the United States to 1920;	SARAH MCCULLOH LEMMON. Frustrated Patriots: North Carolina and the War of 1812. By Hugh F. Rankin 496
RICHARD B. CRAIG. The Bracero Program: Interest Groups and Foreign Policy; RODOLFO ACUÑA. Occu- pied America: The Chicano's Struggle toward Lib-	ROBERT W. JOHANNSEN. Stephen A. Douglas. By Frank Otto Gatell 497
eration. By J. Joseph Huthmacher 479 ALDEN T. VAUGHAN and GEORGE ATHAN BILLIAS, eds.	JULIA FLOYD SMITH. Slavery and Plantation Growth in Antebellum Florida, 1821–1860.  By William K. Scarborough 497
Perspectives on Early American History: Essays in Honor of Richard B. Morris. By Marvin R. Zahniser 481	EDWARD PESSEN. Riches, Class, and Power before the
GEORGE L. SMITH. Religion and Trade in New Netherland: Dutch Origins and American Development.  By Michael G. Hall 481	CLIFFORD M. DRURY. Marcus and Narcissa Whitman and the Opening of Old Oregon. In 2 vols.  By Robert L. Whitner 499
HUGH T. LEFLER and WILLIAM S. POWELL. Colonial North Carolina: A History. By Edward S. Perzel 482	JANE SHAFFER ELSMERE. Henry Ward Beecher: The Indiana Years, 1837–1847. By Donald M. Scott 499
ERNEST TRICE THOMPSON. Presbyterians in the South. Vols. 1-3. By Lefferts A. Loetscher 483	JOHN H. SCHROEDER, Mr. Polk's War: American Opposition and Dissent, 1846-1848. By Lewis Perry 500
MELVIN B. ENDY, JR. William Penn and Early Quakerism. By Michael McGiffert 483	STUART BRUCE KAUFMAN. Samuel Gompers and the Origins of the American Federation of Labor, 1848-
The Diary of Samuel Sewall, 1674-1729. Vols. 1, 2. Ed. by M. HALSEY THOMAS. By Sheldon S. Cohen 484	1896. By Louis L. Athey 500 WILLIAM C. WRIGHT. The Secession Movement in the
JACOB JUDD and IRWIN H. POLISHOOK, eds. Aspects of Early New York Society and Politics.	Middle Atlantic States. By George T. McJimsey 501
By John F. Roche 485 RICHARD WARCH. School of the Prophets: Yale College, 1701-1740. By George F. Frick 485	MARILYN MCADAMS SIBLEY. George W. Brackenridge: Maverick Philanthropist. By William Curtis Nunn 502
STEPHEN E. PATTERSON. Political Parties in Revolu- tionary Massachusetts. By Harry M. Ward 486	SAUL SIGELSCHIFFER. The American Conscience: The Drama of the Lincoln-Douglas Debates.
ROBERT MCCLUER CALHOON. The Loyalists in Revolutionary America, 1760-1781.	By Robert W. Johannsen 502  WILLIAM E. PARRISH. A History of Missouri. Vol. 3.
By Beatrice K. Hofstadter  K. G. DAVIES, ed. Documents of the American Revolution area and Colomical Office Series Volume	By Rodney C. Loehr 503  JOHN W. BLASSINGAME. Black New Orleans, 1860—
lution, 1770-1783 (Colonial Office Series). Vols. 1-3. By Paul H. Smith 488 NORTH CALLAHAN. George Washington: Soldier and	G. R. TREDWAY. Democratic Opposition to the Lin-
Man; RICHARD B. MORRIS. Seven Who Shaped Our Destiny: The Founding Fathers as Revolutionaries.	coln Administration in Indiana.  By David Lindsey 504
By Lawrence H. Leder 489 HUGH F. RANKIN. Francis Marion: The Swamp Fox.	JOHN NIVEN. Gideon Welles: Lincoln's Secretary of the Navy. By William James Morgan 505
By Harry L. Coles , 489	THOMAS LAWRENCE CONNELLY and ARCHER JONES. The

By Eugene S. Ferguson

Politics of Command: Factions and Ideas in Confederate Strategy. By Robert Hartje 506	JOHN L. NETHERS. Simeon D. Fess: Educator & Politician. By Lawrence L. Murray 520
JAMES A. WARD. That Man Haupt: A Biography of Herman Haupt. By Albro Martin 507	RICHARD FITZGERALD. Art and Politics: Cartoonists of the Masses and Liberator. By Louis Filler 521
ERNEST N. PAOLINO. The Foundations of the American Empire: William Henry Seward and U.S. Foreign Policy. By Harold M. Hyman 508	JERVIS ANDERSON. A. Philip Randolph: A Biographical Portrait. By Eugene Levy 522
LOUIS S. GERTEIS. From Contraband to Freedman: Federal Policy toward Southern Blacks, 1861–1865.	ROBERT G. WEISBORD. Ebony Kinship: Africa, Africans, and the Afro-American. By Michael R. Winston 523
By Alwyn Barr 509	william H. Harbaugh. Lawyer's Lawyer: The Life of John W. Davis. By Samuel B. Hand 523
MILTON LOMASK. Andrew Johnson: President on Trial. By Constance McLaughlin Green 509	ABRAHAM HOFFMAN. Unwanted Mexican Americans in the Great Depression: Repatriation Pressures,
PEGGY LAMSON. The Glorious Failure: Black Congressman Robert Brown Elliott and the Reconstruction in South Carolina.  By Joseph Boskin 510	1929-1939. By Roger Daniels 524  MARTIN DUBERMAN. Black Mountain: An Exploration in Community.
TOM E. TERRILL. The Tariff, Politics, and American	By F. Garvin Davenport, Sr. 524
Foreign Policy, 1874-1901. By David Healy 511	RIGHARD DYER MACCANN. The People's Films: A Political History of U.S. Government Motion Pictures. By Frederick D. Jackes 525
KENNETH J. HAGAN. American Gunboat Diplomacy and the Old Navy, 1877–1889. By Benjamin F. Gilbert 511	FRANK FREIDEL. Franklin D. Roosevelt: Launching the New Deal. By William E. Leuchtenburg 526
HERBERT APTHERER, ed. The Correspondence of W. E. B. Du Bois. Vol. 1. By Nathan Irvin Huggins 512	RICHARD D. MCKINZIE. The New Deal for Artists; FRANCIS V. O'CONNOR, ed. Art for the Millions: Essays from the 1930s by Artists and Administrators
CARTER E. BOREN et al. Essays on the Gilded Age. Ed. by MARGARET FRANCINE MORRIS.  Pr. J. Porry J. Langell J. J.	of the WPA Federal Art Project. By Garnett McCoy 527
By J. Perry Leavell, Jr. 513 STEPHAN THERNSTROM. The Other Bostonians: Pov-	WILLIAM STOTT. Documentary Expression and Thirties America. By Garth S. Jowett 528
erty and Progress in the American Metropolis, 1860–1970. By Kenneth T. Jackson 513	ARTHUR M. SCHLESINGER, JR. The Imperial Presidency. By Richard S. Kirkendall 529
WILLIAM W. SAVAGE, JR. The Cherokee Strip Live Stock Association: Federal Regulation and the Cat- tleman's Last Frontier. By W. Eugene Hollon 514	IRWIN F. GELLMAN. Roosevelt and Batista: Good Neighbor Diplomacy in Cuba, 1933-1945. By William Kamman 530
D. JEROME TWETON. The Marquis de Morès: Dakota Capitalist, French Nationalist. By James E. Hendrickson 515	VLADIMIR PETROV. A Study in Diplomacy: The Story of Arthur Bliss Lane; RUSSELL H. FIFIELD. Americans in Southeast Asia: The Roots of Commitment.
SALVATORE PRISCO III. John Barrett, Progressive Era Diplomat: A Study of a Commercial Expanionist,	By Armin Rappaport 530
1887-1920. By Samuel F. Wells, Jr. 515	PAUL A. VARG. The Closing of the Door: Sino-American Relations, 1936-1946; JOHN M. ALLISON.
JOHN S. GOFF. George W. P. Hunt and His Arizona. By Larry D. Ball 516	Ambassador from the Prairie: Or Allison Wonder- land. By James M. McCutcheon 532
JUNE SOCHEN. Movers and Shakers: American Women Thinkers and Activists, 1900–1970. By Janet Wilson James 516	GEORGE C. HERRING, JR. Aid to Russia, 1941–1946: Strategy, Diplomacy, the Origins of the Cold War. By Thomas G. Paterson 533
LAURENCE VEYSEY. The Communal Experience: Anarchist and Mystical Counter-Cultures in America. By Thomas R. West 517	RICHARD W. STEELE. The First Offensive, 1942: Roosevelt, Marshall and the Making of American Strategy. By D. Clayton James 534
EUGENE LEVY. James Weldon Johnson: Black Leader, Black Voice. By William M. Tuttle, Jr. 518	PETER CLECAK. Radical Paradoxes: Dilemmas of the American Left: 1945-1970. By Ann J. Lane 534
THOMAS D. CLARK. Indiana University: Midwestern Pioneer. Vol. 2, In Mid-Passage. By Merle Curti 518	ROBERT A. PACKENHAM. Liveral America and the Third World: Political Development Ideas in For- eign Aid and Social Science.
JOHN D. BUENKER. Urban Liberalism and Progressive Reform. By Bruce M. Stave 519	By John G. Sproat 535 LEONARD GREENBAUM. A Special Interest: The Atomic
LAWRENCE R. GUSTIN. Billy Durant: Creator of Gen-	Energy Commission, Argonne National Laboratory, and the Midwestern Universities.

LAWRENCE R. GUSTIN. Billy Durant: Creator of Gen-

eral Motors. By James J. Flink

RICHARD F. HAYNES. The Awesome Power: Has	rry S.	GANADA
Truman as Commander in Chief. By George E. Hopkins	537	JOHN L. FINLAY. Social Credit: The English Origins. By Richard A. Preston 543
Foreign Relations of the United States, 1948. V The Western Hemisphere.		,
By Russell H. Bostert	537	LATIN AMERICA
ELMO RICHARDSON. Dams, Parks & Politics: Reso Development & Preservation in the Truman-I hower Era. By Roderick Nash		STUART B. SCHWARTZ. Sovereignty and Society in Colonial Brazil: The High Court of Bahia and Its Judges, 1609-1751. By C. R. Boxer 544
TOWNSEND HOOPES. The Devil and John I Dulles. By Ronald W. Pruessen	Foster 539	D. C. M. PLATT. Latin America and British Trade, 1806–1914. By William Paul McGreevey 544
STEPHEN R. WEISSMAN. American Foreign Polithe Congo, 1960-1964. By Vernon McKay	cy in 540	JOHN LYNCH. The Spanish American Revolutions, 1808–1826. By John P. Hoover 545
ANATOLII ANDREIEVICH GROMYKO. Through Re Eyes: President Kennedy's 1036 Days. By E. Berkeley Tompkins	ussian 540	RONNIE C. TYLER. Santiago Vidaurri and the Southern Confederacy. By David M. Pletcher 546
G. POPE ATKINS and LARMAN C. WILSON. The U States and the Trujillo Regime; ABRAHAM F. LO THAL. The Dominican Intervention.		DAVID RONFELDT. Atencingo: The Politics of Agrarian Struggle in a Mexican Ejido. By Charles R. Berry 546
By L. Lejeune Cummins	541	Noview Design
JOSEPH M. SIRACUSA. New Left Diplomatic His and Historians: The American Revisionists.	tories	ROBERT JONES SHAFER. Mexican Business Organiza- tions: History and Analysis; ROBERT W. RANDALL. Real del Monte: A British Mining Venture in Mexico.
By Martin J. Sherwin	542	By Marvin D. Bernstein 547
Communications	549	Other Books Received 556
Recent Deaths	551	Index of Advertisers 50(a)
Festschriften and Miscellanies	554	

#### American Historians and the World Today: Responsibilities and Opportunities

#### LEWIS HANKE

NATIONS HAVE LONG HAD RELATIONS with each other and have acknowledged some responsibilities to each other in the world, but have historians? Members of the American Historical Association will increasingly ponder this question as the time approaches for the first meeting in the United States of the International Congress of Historical Sciences. Some American historians have attended the other international meetings held in Europe since 1900, but the congress is expected to bring together in San Francisco in August 1975 several thousand historians, most of them Americans. The participants will read or listen to learned papers on the "grand themes of history" as well as on a large number of smaller topics, will attend receptions, and will enjoy the still powerful attractions of northern California. One may well ask to what useful end all this movement, all this expense of time and money will be directed.

My answer is a simple one. International congresses of historians do not fully meet the needs of the times and cannot be expected to do so unless the organization that sponsors them is substantially changed and unless national organizations accept far greater international responsibilities. For the AHA this means that we need to strengthen the teaching and writing in the United States of the history of all regions of the world, to recognize the increasingly significant study abroad of our history, and to foster in all possible ways the professional relations of historians on an international scale. For the ICHS to meet its challenge, this largely Western organization must review its traditional operations in various specific ways, which will be suggested later.

This may seem a Utopian proposal to those aware of the political problems encountered by the congresses and to historians everywhere who are

This is a presidential address delivered by Mr. Hanke at the annual meeting of the American Historical Association, Chicago, December 28, 1974. The author wishes to acknowledge the valuable assistance received from many persons, beginning with the discussion held with a Spanish railroad track walker while waiting in the Escorial station for a train to Madrid in the summer of 1929.

1

often concerned principally with their own history. Conor Cruise O'Brien wrote:

Most history is *tribal* history: written . . . in terms generated by and acceptable to, a given tribe or nation. . . . Historians, like other people, tend to identify with a community—not necessarily the one in which they were born—and in the case of modern historians this identification is likely to affect, and interact with, the character of their work, their career, their geographical location, and their public. Normally they write within a convention which suggests these conditioning factors do not exist, or can be ignored. Marxist historians, indeed, emphasize such factors but only as limitations on bourgeois historians.<sup>1</sup>

If this be true, or partially true, why should Americans concern themselves with the history of other tribes and with other tribal historians?

MEMBERS OF THAT LARGE and diverse tribe which inhabits what is called the Western World can best begin to examine these questions by considering the consequences of the discovery of America on the writing of history. Herbert Butterfield has emphasized that one of the unique characteristics of the West is its "historical mindedness" and that history only in modern times has become the kind of subject it is today.<sup>2</sup> Yet he and many others ignore Iberian influences, a considerable omission because in the development of history since 1492 Spain was in the forefront, at least chronologically, of all European nations, and Portugal also made significant contributions.

Historians should be grateful for the Spaniards' keen sense of the past and for their almost unconscious though certainly widespread realization that Spanish actions overseas would one day be scrutinized by posterity. Columbus started the practice of writing about America, and many followed his example, for the conquest so stimulated their imagination that they came to look upon it as the greatest event since the coming of Christ. Even as the conquistadores roamed over vast areas of land and sea and missionaries attempted to Christianize millions of Indians, they collected historical materials and composed chronicles on a monumental scale.<sup>3</sup> This copious documentation constitutes another kind of treasure from the Indies, distinct from the gold and silver found there, a documentation that still excites historians by its richness and depresses them by its quantity, for every fleet from Spanish America carried homeward thirty or forty boxes of documents, often carefully indexed for convenient study by the council of the Indies.<sup>4</sup>

<sup>&</sup>lt;sup>1</sup> Conor Cruise O'Brien, States of Ireland (New York, 1972), 16-17.

<sup>&</sup>lt;sup>2</sup> Herbert Butterfield, Man on His Past: The Study of the History of Historical Scholarship (Boston, 1960), vii-xi.

<sup>&</sup>lt;sup>3</sup> Lewis Hanke, "The Other Treasure from the Indies during the Epoch of Emperor Charles V," in Peter Rassow and Fritz Schalk, eds., Karl V: Der Kaiser und seine Zeit (Cologne, 1960), 94-103.

<sup>&</sup>lt;sup>4</sup> Juan Manzano, ed., "Un documento inédito relativo a como funcionaba el Consejo de Indias," Hispanic American Historical Review, 15 (1935): 316.

Beginning with Viceroy Antonio de Mendoza, who arrived in Mexico in 1535, the principal Spanish officials manifested a keen interest in history. Mendoza wanted to know about the "chronicles, hieroglyphs, and pictures from Montezuma's palace which told of the migrations of the ancient Mexicans." Many other viceroys, moreover, commissioned the writings of histories or received histories voluntarily written by Spaniards on American subjects.<sup>5</sup> Sometimes there was a polemic purpose, as when Viceroy Francisco de Toledo organized in the 1570s a study of Inca history to prove Spain's contention that her conquest not only had followed just principles but in fact had liberated the Indians from a tyrannical and unjust Inca rule. But even this stern official was much impressed by what he saw in Peru, and he proposed that a museum be created in Spain where "Indian art and the products of nature" in America could be studied.6

Ecclesiastics were eager to have their missionary triumphs recorded. In 1536 the Franciscan chapter in Mexico City recommended that one of their number write an account of Indian life in pre-Spanish days as well as a history of the labors of the first group of Franciscans, known as "The Twelve Apostles," from the time of their arrival in 1524.7 The dedicated missionaries Spain sent to America were convinced that the discovery and conquest not only afforded a unique opportunity to bring the Gospel to the Indians but also, according to some, foreshadowed the rapid approach of the end of the world and the coming of the millennial kingdom. Though the traditional Church was being destroyed in Europe, or at least severely challenged by Luther, the friars were determined that a new and more powerful Church be built in America. But there was no time to be lost. Faced with an enormous diversity of native languages, which were in turn divided into hundreds of dialects, all phonetically and morphologically alien to European languages, the early friars first tried to learn Nahuatl by playing with Indian children to acquire useful phrases. Frustrated in their attempt to identify even a few words but unwilling to allow one Indian soul to suffer damnation because of their own ignorance, some of the early friars preached to the Indians in Latin or Spanish in the hope that Christian fervor would make up for linguistic deficiencies.8

As the conquest proceeded and Philip II increasingly came to dominate the administrative machinery governing the far-flung Spanish empire, a demand arose for an adequate history of Spanish accomplishments as a whole. A decisive epoch for historiography began about 1570 when the council of the Indies decided that good administration required an archive containing organized information on previous laws and past events, ma-

<sup>&</sup>lt;sup>5</sup> George P. Hammond and Agapito Rey, eds., Obregon's History of 16th Century Explorations

in Western America (Los Angeles, 1928), 10-11.

6 Marcos Jiménez de la Espada, Tres relaciones de antigüedades peruanas (Madrid, 1879), xix. Francis Borgia Steck, ed., Motolinia's History of the Indians of New Spain (Washington,

<sup>8</sup> David Haberly, "The Hieroglyphic Catechisms of Mexico" (B.A. thesis, Harvard College, 1963), 3.

chinery for obtaining current reports, and an official historian.9 A detailed questionnaire was drawn up, which every governor in America was ordered to answer with specific data on the history, people, climate, and geography of the territory he administered. Begun as a brief inquiry in 1569, this questionnaire soon grew to fifty items and—since bureaucrats never seem to have enough information—eventually became a printed volume of three hundred and fifty questions, which must have been a heavy cross for hardpressed governors in the far reaches of the empire to bear.<sup>10</sup>

The first historian was appointed in 1573, and beginning in 1578 instructions were regularly sent out requiring the principal royal representatives in America to search their archives for historical manuscripts and to dispatch the originals or authentic copies to the council of the Indies so that a true, general history of the Indies could be written. The council had a realistic view of the habits of historians, for it decreed that the appointee ` would not receive the last quarter of his salary until he had turned in some completed text. For almost two hundred and fifty years, until the eve of independence, Spain sent out a constant stream of orders for information and history.11

Controversy inevitably developed over what constituted "true" history. To set straight the record as he saw it, one foot soldier of Ferdinand Cortez, Bernal Díaz del Castillo, composed a True History of the Conquest of New Spain, now a classic on the discovery period. 12 Bitter and prolonged battles on the justice of Spanish dominion and the place of Indians in Spanish society produced an enormous amount of historical documentation, which continues to attract historians. We are particularly aware of these disputes today because 1974 witnessed the commemoration of the five-hundredth anniversary of the birth of Bartolomé de Las Casas, the best-known defender of the Indians and a persistent doubter of the justice of Spanish rule. Inasmuch as my volume on his doctrine has recently appeared, I will restrain myself, with some difficulty, from analyzing his role in the development of historical writing in America and his insistence that the American Indians should not be considered natural slaves according to the Aristotelian doctrine but instead should be persuaded by peaceful methods to accept the Christian faith.<sup>13</sup> To prove that the Indians were not semianimals whose

11 Sylvia Vilar, "Une vision indigéniste de l'Amérique en 1812," Mélanges de la Casa de

13 Hanke, All Mankind Is One: A Study of the Disputation between Bartolome de Las Casas

<sup>9</sup> Rómulo D. Carbia, La crónica oficial de las Indias Occidentales: Estudio histórico y critico acerca de la historiografía mayor de Hispano-America en los siglos XVI a XVIII, con una introducción sobre la crónica oficial en Castilla (Buenos Aires, 1940).

<sup>10</sup> Marcos Jiménez de la Espada, Relaciones geográficas de Indias, ed. José Urbano Martínez Carreras, 1 (Madrid, 1965): 5-117. For an exhaustive description and evaluation of these reports, see Howard F. Cline, "Guide to Ethnohistorical Sources: Part One," in Robert Wauchope, ed., Handbook of Middle American Indians, 12 (Austin, 1972): 183-242, 324-95.

Velazquez, 7 (Paris, 1971): 339-401.

12 Bernal Diaz del Castillo, True History of the Conquest of New Spain, ed. and tr. Alfred Percival Maudslay (London, 1908-16).

property and services could be commandeered at will by the Spaniards, Las Casas prepared a large work entitled *Apologetic History*, in which he advanced the idea that the Indians compared very favorably with both the Spaniards and the peoples of ancient times, were eminently rational beings, and in fact fulfilled every one of Aristotle's requirements for the good life.

The main argument of Las Casas against those who considered the Indians less than human beings, an argument that entitles him to be included as a principal member in that great tribe that might be called "all mankind," may best be summarized in his own words:

Thus mankind is one, and all men are alike in that which concerns their creation and all natural things, and no one is born enlightened. From this it follows that all of us must be guided and aided at first by those who were born before us. And the savage peoples of the earth may be compared to uncultivated soil that readily brings forth weeds and useless thorns but has within itself such natural virtue that by labor and cultivation it may be made to yield sound and beneficial fruit.<sup>14</sup>

We see here the beginning of the great dispute today, in which William Shockley and Arthur Jensen contend that blacks are born with genetic deficiencies that limit their intellectual growth and hinder their attempts to compete with whites.

The history of the relations between Europeans and natives in the conquest period is rich in detail. Indian men loved to wear their hair long, which offended Spaniards, whose custom was to have their hair cut short. Besides, the Spaniards said long hair was filthy and that Indian women usually slept with the men whose hair they braided, which was an offense to Christian morals. In Manila one zealous sixteenth-century bishop was so opposed to allowing Chinese converts there to keep their queues that it required an order from the council of the Indies to stop him from cutting them off. Instead, the bishop and his missionaries were ordered to treat the Chinese "with prudence and intelligence, and with the kindness and mildness required to nurture such new and tender plants." In the following century Jesuits in the Philippines denounced the drinking of chocolate; in Mexico they said it was a danger to chastity for it aroused the passions. By the end of the century, however, the nutritious drink had become a standard breakfast food on Jesuit tables in Spain and the Indies. 17

But one custom of the Indians was not accepted—human sacrifice by the

and Juan Ginés de Sepúlveda in 1550 on the Intellectual and Religious Capacity of the American Indians (De Kalb, 1974).

<sup>14</sup> Bartolomé de Las Casas, Apologética historia sumaria, ed. Edmundo O'Gorman, 1 (Mexico, 1967): 258.

<sup>15</sup> Juan de Matienzo, *Gobierno del Perú (1567)*, ed. Guillermo Lohmann Villena (Paris, 1967), 80.

<sup>18</sup> Philip II to Bishop Salazar, June 23, 1587, Archivo General de Indias, Seville, Filipinas 339, bk. DDI, pt. 2, fol. 155v.

<sup>17</sup> Horacio de la Costa, S.J., The Jesuits in the Philippines, 1581-1768 (Cambridge, Mass., 1967), 249.

Aztecs. None of the many contemporaries of Las Casas who prepared histories of the dramatic meeting of the West with Indian culture supported his view that the practice of human sacrifice, which revolted lay and ecclesiastics alike, should be understood in the light of the Indians' own history and doctrines. Las Casas discerned, underneath the horrible and bloody aspects of these rites, a commendable spirit of religious devotion that might be directed to higher ends and enlisted in the service of the only true God.<sup>18</sup>

As the conquest proceeded and as the archives of the council of the Indies in Spain began to fill, Spaniards gave more and more attention to Indians and their culture. What taxes had they paid to their rulers before the Spaniards came? What religious concepts did they have that must be rooted out to prepare them for the true faith? Did their previous habits indicate that they were capable of becoming civilized and Christian? Though ecclesiastical writers concentrated on religious aspects of the conquest, they also viewed it in the round; they wrote on art and cooking, child training, disease and death, and the many other subjects that interested them.

The greatest single figure in the study of Indian cultural history was the Franciscan, Bernardino de Sahagún. One of the earliest missionaries in Mexico, he was not satisfied with the approach involving playing with children and almost at once began to study Nahuatl and collect materials bearing on the Indian past. In 1547 his superior ordered him to work on a history, and for a decade he continued his investigations. Then in 1558 he embarked in Tepepulco near Mexico City upon a large-scale, systematic study of Aztec culture, with the aid of several of his own Spanish-speaking Indian disciples who also knew Latin. Sahagún had written down many extensive lists of items—culture elements they would be called today—on which he desired information, and he brought together about a dozen old men reputed to be wise in their own lore. Sahagún and his research assistants interrogated these informants during 1558-59; it was the first oral-history project in America. The old men illustrated their replies by preparing a series of drawings and paintings, which were explained in writing by the Indian assistants. These visual materials became an essential part of the historical documentation.

After two years of discussions with the old men and his young Indian assistants at Tepepulco, Sahagún moved to another center at Santiago Tlatelolco to test his preliminary findings, for he exhibited the fundamental skepticism of the historian who is rarely satisfied that he has complete or accurate sources. For two more years, 1560–61, he reviewed and revised all his material with the help of a new set of informants. It took him three more years to re-edit the whole manuscript, which was still in Nahuatl, and to rework it into twelve books, each one broken down into chapters and each chapter into paragraphs.

The result was a carefully organized mass of text and 1,850 illustrations

<sup>18</sup> Hanke, All Mankind Is One, 93-95.





Musicians and entertainers (bk. 1, no. 19)

Bathing the baby (bk. 6, no. 29)

on the spiritual and material aspects of the life of the ancient Mexicans as the Indians remembered them. It was decidedly not, like so much of the transatlantic literature of the period, a European view masquerading as a description of far-off peoples, but a remarkable collection of oral literature that expressed the soul and life of the Aztec people at the time of their greatness, one of the finest sources known for ethnohistory.<sup>19</sup> While some other Spaniards were fanatically destroying Indian culture, Sahagún methodically brought together documentation on the functions, ceremonies, legends, and traditions of the many gods of the Aztecs, on astronomy, astrology, the calendar, and the calculation of the recording of time, which was of great importance to them. Sahagún also included their superstitions, rhetoric, philosophy, ideas of mortality, songs to the gods, and hymns to the sun, the moon, the stars, and the wind. The ancient rulers received much attention, as did their merchants and judges. The education of the children in the home and school was treated, as well as information on botany, zoology, and the animal and plant life of Mexico, mineralogy, agriculture, the preparation and preservation of edible plants, sculpture, painting, melting of metals, the jeweler's trade, house building, the raising and care of domestic animals, road building, and temple construction. The final book described the conquest of Mexico as seen by the conquered.

Sahagún's purpose was clear: to learn all about the Indian language and culture in order to help him and the other missionaries in their conversion labor. Thus he included descriptions of the ways in which Indians got intoxicated for ceremonial reasons, for Sahagún maintained that missionaries must know all about the sins of the Indians in order to correct them, just as doctors must study disease.

<sup>19</sup> Henri Baudet, Paradise on Earth: Some Thoughts on European Images of Non-European Man, tr. Elizabeth Wentholt (New Haven, 1965), vii; J. H. Elliott, "The Discovery of America and the Discovery of Man," Proceedings of the British Academy, 63 (1972): 1-27; Benjamin Keen, The Aztec Image in Western Thought (New Brunswick, 1971).





The ruler's war array (bk. 8, no. 77)

Cleaning the teeth (bk. 10, no. 161)

As Sahagún struggled through the years against obstacles and apathy he became so immersed in the study of Indian culture that he grew interested in it for its own sake and was concerned that contact with Europeans would cause the native culture to disappear or become hybridized. Thus there was dedication and urgency in his work. At last, as the result of a royal order in 1577 instructing Viceroy Enríquez Martínez to collect all of Sahagún's manuscripts for the council of the Indies, the Nahuatl text was translated into Spanish and sent to the council.

Sahagún died in 1590 without seeing a single chapter of his monumental work published. Only in recent years have complete editions of both the Nahuatl and the Spanish texts become available, based upon the various manuscripts dispersed in libraries in Florence, Madrid, and Mexico City. The first translation into any language of the entire Nahuatl manuscript has just been completed, after thirty-five-years' labor, by Charles E. Dibble and Arthur J. O. Anderson, whose English version, General History of the Things of New Spain, imparts the spirit as well as the substance of the original. This outstanding work of American scholarship, richly footnoted and based upon extensive researches by European and Mexican scholars as well as those of the editors, will enable the English-speaking world to appreciate one of the foundation works in the history of how scholars in one culture have studied another.<sup>20</sup>

20 Bernardino de Sahagún, General History of the Things of New Spain, tr. and ed. Arthur J. O. Anderson and Charles E. Dibble (Salt Lake City, 1950-69). The history and bibliography of Sahagún's work are extraordinarily complex. For a competent guide through the maze, see Howard F. Cline and John B. Glass, "Guide to Ethnohistorical Sources: Part Two," in Wauchope, Handbook of Middle American Indians, 13 (Austin, 1973): 186-239. This rich volume contains much information on other aspects of history writing in the Indies by such authorities as Ernest J. Burrus, Charles Gibson, and others. See also Munro Edmundson, ed., Sixteenth-Century Mexico: The Work of Sahagún (Albuquerque, 1974). For a general study of the growth of Spanish studies on Indian cultures during the conquest, see Carmelo Lisón Tolosana, Antropología social en España (Madrid, 1971), 1-96. See also Alfredo Jiménez Núñez, "La antropología y la historia de América," Revista de Indias, 107-08 (1967): 59-87; and Núñez,





Treatment of spider bites (bk. 11, no. 287)

Copperworking (bk. 11, no. 796)

The illustrations are from Bernardino de Sahagún, Historia General de las Cosas de Nueva España, 5 (Madrid, 1905). Photographs reproduced from the collection of the Library of Congress.

Sahagún must be recognized as one of the most complex Spaniards in sixteenth-century America. He was a member of a powerful nation, whose people believed themselves to have been singled out by God for His purposes just as certainly as the Puritans of Massachusetts Bay Colony were convinced that they were "God's Chosen People." He was a member of one of the most militant missionary nations that the world has ever seen, yet in an age when few persons displayed a respectful interest in any culture except their own he devoted many years of effort to understanding, from their viewpoint, practically all aspects of the life of the ancient Mexicans. For a sixteenth-century European, his was a remarkable achievement particularly when we realize that no other colonizing nation produced such a figure.<sup>21</sup>

<sup>&</sup>quot;El método etnohistórico y su contribución a la antropología americana," Revista Española de Antropología Americana, 7 (1972): 163-96.

<sup>21</sup> On the lack of French writers of Sahagún's stature, see Alfred Métraux, "Les precurseurs de l'ethnologie en France du XVIº au XVIIIº siècle," Journal of World History, 7 (1962): 721-38. The early Jesuits in China had language difficulties. Donald F. Lach states that Matteo Ricci "was evidently the only one of the Europeans to learn more than a few polite expressions in Chinese." Asia in the Making of Europe, 1 (Chicago, 1965): 821. Even Ricci did not measure up to Sahagun, as will be seen from the study by George L. Harris, "The Mission of Matteo Ricci, S.J.: A Case Study of an Effort at Guided Culture Changes in the Sixteenth Century," Monumenta Serica: Journal of Oriental Studies, 25 (1960): 1-168. There was an impressive amount of information on Chinese culture available in Europe's major languages, according to Edwin J. Van Kley, "News from China: Seventeenth-Century European Notices of the Manchu Conquest," Journal of Modern History, 45 (1973): 561-82. But this information was not obtained by the rigorous methods of Sahagun. In India, according to Sir George B. Sansom, "it was not until 1606-after a hundred years of missionary effort-that the Jesuit father Roberto de Nobile, with the approval of the Society of Jesus, undertook a serious study of Hinduism in order to learn how it could best be criticized and confuted." The Western World and Japan (New York, 1950), 77. Though England had commitments in India from the beginning of the seventeenth century, Sir William Jones of the High Court in Calcutta was in 1783 the pioneer in the British study of Indian languages. The study of the Japanese language by the Jesuits Luís Frois and João

The work of Sahagún and other Spaniards who studied the history of Indian culture and the accomplishments of Spain in America have not yet sufficiently been analyzed or understood. Perhaps in 1992 when the fivehundredth anniversary of the voyage of Christopher Columbus across the Ocean Sea will be commemorated—and Spain has already appointed a commission to plan for this event-we will have an adequate examination of these works that helped to lay the basis for the modern study of history. Among the many figures who should appear in such a work, Sahagún will be seen not only as a "past glory" but as one whose work has significance for us today. As Miguel León-Portilla of the University of Mexico emphasizes, Sahagún's supreme achievement is that he found a way to discover in a different culture those elements which are common to all mankind. León-Portilla concludes that the world today, with its many distinct cultures and physically closer together than ever before because of technological advances, needs the lesson of Sahagún, for it should help us to achieve relations with other cultures through dialogue and comprehension.<sup>22</sup>

Why do some historians in the twentieth century, which bears some striking resemblances to the time when Sahagún was at work, study other cultures? Why do many more historians, though occasionally attempting to develop professional relations on an international basis, continue to work only on the history of their own tribes?

Only fragmentary accounts have been published concerning the efforts of historians to create some kind of international community, which illustrates the truth of the remark by Charles Homer Haskins, "Many historians find it easy to be historically minded respecting everything save only history." Our best single source for an understanding of the development of the international congresses from the American viewpoint is the correspondence of J. Franklin Jameson, that giant among the founders of the

Rodrigues was on a fairly low practical level, states Tadao Doi, "A Review of Jesuit Missionaries' Linguistic Studies of the Japanese in the 15th and 17th Centuries," in Japanese National Commission for UNESCO, International Symposium on the History of Eastern and Western Cultural Contacts (1957), Collection of Papers Presented (Tokyo, 1959), 215–22. Their study was a far cry from the intensive linguistic effort of Sahagún who described his work as "a sweeping net to bring to light all the terms of this language, with their regular and metaphorical meanings, and ways of saying things." Cline and Glass, "Guide to Ethnohistorical Sources: Part Two," 203. On Europe's abysmal record in studies of African cultures, see Katherine George, "The Civilized West Looks at Primitive Africa, 1400–180c: A Study in Ethnocentrism," Isis, 49 (1958): 62–72. H. J. de Graaf remarks on how little research was done by the Dutch. "Aspects of Dutch Historical Writings on Colonial Activities in South East Asia with Special Reference to the Indigenous Peoples during the Sixteenth and Seventeenth Centuries," in D. G. E. Hall, ed., Historians of South East Asia (London, 1961), 213–24. In his introduction Hall emphasizes the great strength of the "Europe-centricity of historians," especially in the period before World War II (p. 8).

22 Miguel León-Portilla, "Significado de la obra de Fray Bernardino de Sahagún," Estudios de

Historia Novohispana, 1 (1960): 27.

23 Charles Homer Haskins, "European History and American Scholarship," AHR, 28 (1922-23):

AHA, for it provides a running account of the activities of historians in the international meetings held since the first one in Paris in 1900. When Jameson attended the congress in London in 1913, the ignorance and the indifference of the European historians toward American history pained him. He reported that no one in Great Britain "was at all interested in American history." Nor did other European historians at the congress manifest the slightest curiosity in what had happened in the United States. By 1915 Jameson was fearful that World War I would create a state of mind "which for a long time will make it difficult for the students of history in various nations to come together in a spirit of harmony," and he was sufficiently realistic to see "only a restricted scope for international endeavor in history," due to "the fact that for the last four hundred years mankind has been chiefly organized in great states." He did not expect European historians to cooperate much.

Although Jameson spent most of his life outside universities, he considered them the basis for sound historical activities. In 1919 he supported plans to establish a professorship of American history at the University of London. He also applauded the proposal that the 1923 congress should include one session devoted to our history; in fact, he wrote in a burst of chauvinism, "American history, between you and me, should be the chief pursuit of mankind henceforth." The congress meeting in Brussels in 1923 was not prepared for such a radical step as a session on United States history alone but experimented with a separate session on "the history of the American continents," which may have reflected a reluctance to schedule a session on any subject that Europeans considered as parochial as United States history and on which they were not prepared to speak. The miscellaneous and scattered papers delivered at this session must have convinced the few Americans who attended that European scholars had little knowledge of or interest in our history. 26

Americans were sensitive in other ways too. Haskins devoted his presidential address in 1922 to recounting American contributions to European historiography, as if to make certain that everyone understood how much had been accomplished over here. He urged Americans not to be content with receiving European history secondhand, in packages prepared by European scholars, and insisted that American historians "participate fully and directly in all phases of the historical activity of our time." This question,

<sup>&</sup>lt;sup>24</sup> J. Franklin Jameson to Andrew C. McLaughlin, Feb. 13, 1919, in Jameson, An Historian's World: Selections from the Correspondence of John Franklin Jameson, ed. Elizabeth Donnan and Leo F. Stock (Philadelphia, 1956), 230; Jameson to Waldo G. Leland, Mar. 24, 1924, in ibid., 298. For some recent sober and detailed views on the continuing strength of nationalism, see Boyd C. Shafer, "Webs of Common Interests: Nationalism, Internationalism, and Peace," Historian, 36 (1974): 403–33.

<sup>&</sup>lt;sup>25</sup> Jameson to Leland, Nov. 26, 1922, in *An Historian's World*, 275.

<sup>26</sup> Waldo G. Leland, "The International Congress of Historical Sciences, Held at Brussels," *AHR*, 28 (1922–23): 650–51.

he declared, concerned "the future of American scholarship, its dignity, its independence, its creative power." <sup>27</sup>

Eager as Jameson was to see our history properly recognized at international congresses, he was principally determined to have the congresses produce some lasting benefit for historians and history and also bring historians together in friendly relations by working for a common purpose. The establishment of the International Committee of Historical Sciences in 1926, with a permanent bureau to provide continuity and leadership, was intended to develop projects with international support.<sup>28</sup> But only an International Bibliography of Historical Sciences received general support, and it has had a precarious existence. The statement made by Jameson still has some validity: "These congresses might have done more to promote the progress of historical science than merely to provide an opportunity for the reading of various papers and for social intercourse."<sup>29</sup>

But what can historians do, scattered around the world as they are, following different approaches to history, living under different kinds of governments, with only a few able to attend the meetings held every five years? My own view is that we should encourage the ICHS to expand its activities between sessions on the basis of a few fundamental policies, such as the following.

First, access to archives should be liberalized. The VIth International Council on Archives in 1968 passed far-reaching resolutions on this subject. It urged that archival administrations of all countries review national regulations controlling access to documents and propose to appropriate authorities the removal of all unjustified restrictions. It recommended further that "the principle of equality of treatment between national and foreign scholars be recognized and applied everywhere."<sup>30</sup> Historians surely want to have as full access to sources as possible, and international pressure might be one of the best ways to achieve it. Should not historians, therefore, join with the archivists to work toward these desirable objectives? The ICHS would be expected to devise some procedure to handle complaints, perhaps in cooperation with the archivists. The experience of the AHA with the charges against the Franklin D. Roosevelt Library proves that this might be a heavy responsibility, but no announcement of principles governing access will be

27 Haskins, "European History and American Scholarship," 215; see also Jameson to Henri Reverdin, May 24, 1923, in *An Historian's World*, 288.

20 Jameson to Alexander S. Lappo-Danilevskii, Aug. 3, 1917, in An Historian's World, 214–15. 30 For the complete text, see "Resolutions, Recommendations and Wishes of the VIth International Congress on Archives Held in Madrid, September 3–7, 1968," Archivum, 18 (1970): 213–15.

<sup>28</sup> The literature on the history of the ICHS is scanty. In 1958 the bureau authorized Halvdan Koht and Waldo G. Leland to write a history. Koht prepared an eighteen-page account, The Origin and Beginnings of the International Committee of Historical Sciences: Personal Reminiscences of Halvdan Koht (Lausanne, 1962). Boyd Shafer kindly loaned me a copy of this rare work. Leland apparently never prepared anything on the subject except an earlier paper, "L'Organisation Internationale des Études Historiques," in Historie et Historiens depuis Cinquante Ans: Méthodes, Organisation et Résultats du Travail Historique de 1876 à 1926, 2 (Paris, 1928): 741-56.

worth much unless there is some machinery for inquiry and redress of grievances.

Second, historians should be encouraged to study and teach in foreign lands. Jameson had ideas on this too. He proposed that British professors of history be invited to attend and participate in the annual meetings of the AHA, an invitation that might also involve their teaching in our universities. He once succeeded in getting support from the Carnegie Endowment for International Peace, and distinguished historians from a number of British universities attended the annual meeting in December 1924. Afterward a number of these visitors went to universities to meet with their colleagues and deliver lectures.31 Why should some similar arrangement not be developed for every session of the ICHS? How valuable it would be, for example, to our students and our faculties if fifty or more foreign historians who will attend the meeting in San Francisco could also teach for a quarter or semester before or after the meeting. The experiences of these historians on our campuses would also enlarge their understanding of life in the United States and the variety of historians to be found here. The matching of historians with appropriate institutions might require considerable managerial expertise, but it could be done.

Third, the teaching of history should receive sustained attention. Some sporadic attention has been given since World War I to the analysis of textbooks in order to eliminate gross prejudices and nationalistic bias, but the ICHS does not seem to have considered the improvement of history teaching as an essential part of its task. This is a curious fact. The modifying of national and other prejudices in the writing of textbooks should be one of the obvious and natural objectives of historians in their international organization. But attention to history teaching should not be limited to the everpresent problem of honesty and balance in textbooks. Is it not equally important for us to exchange ideas and experiences with our colleagues in other countries in order to improve the teaching of both our own national histories and the history of other cultures? This fundamental labor can probably best be undertaken at the primary- and secondary-school level, which means that we should ask the ICHS to develop some definite program for teachers in these grades to live and teach outside their own countries. Here indeed is a large and complicated enterprise in which the AHA is not yet fully equipped to participate, but our divisional committee for teaching should be very helpful in the future.

Besides these continuing activities for the improvement of accessibility to sources, travel for historians, and the teaching of history, the ICHS should re-examine the program and organization of its congresses. There must be better ways to foster understanding among historians than to mount expensive extravaganzas every five years.

<sup>31</sup> Jameson to Elihu Root, July 19, 1923, in An Historian's World, 290-91.

The beginnings of the movement for the closer association of historians on an international basis were made by a small band of European and American historians in the early decades of this century, and in our present desire for improvement we must not forget or undervalue the pioneer efforts that made possible the present system of meetings every five years. Nor must we forget that most international movements develop very slowly and often involve disappointments and frustrations. But a larger and more solid structure for the international relations of historians is long overdue, and let us hope that at least the scaffolding for a new structure will have been constructed by the time the AHA completes its first century in 1984. When this comes to pass, all historians, no matter which tribe they belong to, will benefit.

IF JAMESON COULD VISIT us today he would doubtless be gratified to see how American studies, including history, are being increasingly cultivated in universities, institutes, and special associations in Britain, continental Europe, and elsewhere. The inadequacies that lasted into the late 1950s resulted from lack of funds, faculty resistance attributed to political opposition or doubt as to the academic validity of courses on the United States, and "the absence of young scholars with sufficient academic qualifications to merit appointment to university teaching posts in American studies." Thanks in part to the Fulbright program and foundation grants to the American Council of Learned Societies to encourage these studies overseas, the situation has changed radically in recent years.

Now the shoe is on the other foot. Japanese schoolteachers who studied at the University of Massachusetts, Amherst, last summer were shocked to find so little attention to Japanese history in our textbooks, just as scholars in Japan concerned with developing Latin American studies there deplore the ignorance in Latin America of Japanese culture.<sup>33</sup> Foreign historians, in the spirit of Jameson, may now be sensitive to what they view as our indifference to their increasingly important work on United States history. C. Vann Woodward deplored the parochialism of some American historians in these words: "The fault of Americans lies largely in their habit of looking

<sup>32</sup> Gordon B. Turner, "A Decade of American Studies," ACLS Newsletter, 1970, no. 2, pp. 1-6.
33 Gustavo Andrade, "Latin American Studies in Japan," Latin American Research Review,
8 (1973): 147-56. Dr. Andrade writes, somewhat in the spirit of Jameson, "This report has analyzed the state of studies on Latin America in the country which is one of the greatest economic powers of the world and which, according to the prediction of Herman Kahn, will be the country of the twenty-first century. And now let me ask, what does Latin America know about Japan? How many research centers and university departments are there which teach that Japan is no longer the land of cherry blossoms, because the fouled air of the great cities kills them, nor the land of Mount Fuji, because the smoke of the blast furnaces wipes its stylized figure from the landscape, nor the home of the geishas, because they prefer the easier road of the nightclubs? Where are the translations into Spanish of Nobel prizewinner Kawabata? If Latin Americans want, the Japanese to understand the reality of Latin America, Latin Americans must also try to understand the reality of Japan" (pp. 155-56).

within for the significance of historical experience and assessing it narrowly according to preconceptions and legends of democracy, equality, and frontier-flavored determinants of exceptionalism." This myopia also explains why Americans have been so slow "to understand the significance of the influence they have exerted beyond their borders." David M. Potter gives substance to this charge, in examining the Civil War, for he concludes, "The significance of the Civil War for world history, and particularly for the history of nationalism, has been generally neglected by historians."<sup>34</sup>

The quantity and quality of foreign contributions to United States historiography since the end of World War II will probably surprise many of us, and the AHA might well sponsor the preparation of an annotated and organized bibliography on the subject.35 This bibliography would make clear that the increase of attention to our history abroad has not only been beneficial to the persons overseas whom Jameson worried about but would be equally useful to our own historians, for they would learn something about their own fields from foreign historians. A Dutch writer has stressed the difference between American and European scholarship: "European ideas that do not fit well into the American conception of self, that collide with the dominant official ethos of America, have long been softpedalled in American scholarly thought, while they prevail in European thinking."36 There are differences, too, between American and European conceptions of social history, for different value systems result in different views. In the light of development abroad, must we not conclude that American history is too important to be left to American historians alone?

With the ever increasing attention the AHA is giving to teaching, why could we not sponsor, in various parts of the country and on a variety of topics, a continuing series of summer seminars and colloquia that would bring together historians from other parts of the world to discuss matters of mutual interest in the teaching and interpretation of American history? The foreign participants might spend an additional month or so visiting other colleagues or working in archival or library collections. Eventually American and foreign historians might work together on some aspect of

<sup>34</sup> C. Vann Woodward, "The Test of Comparison," in Woodward, ed., The Comparative Approach to American History (New York, 1968), 352; David M. Potter, "Civil War," in ibid., 145. See also Peter Harnetty, "Cotton Exports and Indian Agriculture," Economic History Review, 2d ser., 24 (1971): 414-29.

<sup>35</sup> While gathering material for this paper, the following items came to my attention by chance: Inga Flots, Colonel House in Paris (Aarhus, 1972); A. N. J. den Hollander, ed., Contagious Conflict: The Impact of American Dissent on American Life (Leiden, 1973); "Theses on American Topics in Progress and Completed at British Universities," Journal of American Studies (published by Cambridge University Press), Apr. 1974, no. 1, pp. 131-51; Cristiano Camporesi, Il marxismo teorico negli USA, 1900-1945 (Milan, 1973); Anna Katona, "Nineteenth-Century Hungarian Travelogues on the Pre-Civil-War U.S.," Hungarian Studies in English, 5 (1971): 35-52; "Nineteenth-Century Hungarian Travelogues on the Post-Civil-War U.S.," ibid., 7 (1973): 51-94.

<sup>7 (1973): 51-94.

36</sup> A. N. J. den Hollander, "Cultural Diversity and the Mind of the Scholar," in Hollander, ed., Diverging Parallels: A Comparison of American and European Thought and Action (Leiden, 1971), 208.

our past. Would it not be refreshing to have a Brazilian scholar join with one of our historians to study the history of race relations in the United States?

Since Sahagún's fundamental work on Mexican Indians, studies of foreign cultures by scholars outside the cultures were sporadic until recently. Our institutions of higher education were parochial, for they recognized mainly the United States and Europe as proper subjects for scholarly inquiry and usually regarded other parts of the world as outposts on the periphery of civilization. The result, as Richard D. Lambert stated in his review of language and area programs, was that "generations of Americans educated before World War II were ill-equipped to live in the postwar world of newly independent nations asserting their rights to political sovereignty and to respect for their cultural identities."27 Today the situation has radically changed, due to the energetic and far-sighted support for foreign area programs of the American Council of Learned Societies and the Social Science Research Council from the 1930s onward, with financial aid from foundations.38 World War II prompted foreign language and cultural studies for strategic purposes. Since the end of the war development has been notable, for only thirty years ago "the American scholarly experts on many of the world's areas could have been assembled in a small room, and today all the world areas are represented by flourishing scholarly associations with memberships running in some cases into the thousands."39 Throughout our colleges and universities one now finds a wide variety of well-trained area specialists, ready to enrich the educational offerings for their students with their hard-won knowledge of other cultures.

Many of these area specialists are historians, and now that few students are required to take courses in United States history or Western civilization, should not all history departments use their influence to encourage undergraduates to become acquainted, through a broad "civilization" course, with the history of another culture distinctly different from their own? World history will also have a place, particularly if presented with the imagination and expertise of a William H. McNeill, but the study of a single civilization has a special value all its own. Equally important would be the encouragement of graduate students in history to select one field from non-Western history for their general examinations. Enough good material now exists in English to make this a respectable and interesting possibility for all graduate students, and such broadening of their training

<sup>37</sup> Richard D. Lambert, "Language and Area Studies Review," Items, 27 (1973): 17.
38 See Gordon B. Turner, "The Joint Committee on Slavic Studies, 1948–1971: A Summary View," ACLS Newsletter, 1972, no. 2, pp. 6–26; George E. Taylor, "The Joint Committee on Contemporary China, 1956–1969," ibid., no. 4, pp. 1–16, and 1973, no. 1, pp. 11–32. For a list of the historians who enjoyed unusual opportunities to study foreign languages and cultures in these programs, see Dorothy Sunderland and Leslie Wendell, eds., Directory: Foreign Area Fellows, 1952–1972, of the Joint Committee on the Foreign Area Program of the Social Science Research Council, 1962–1972 (3d ed.; New York, 1973).
38 Lambert, "Language and Area Studies Review," 17.

would also enlarge their possibilities as teachers. This training would, in addition, increase their ability to treat topics of comparative history. The stimulating contribution of Carl N. Degler on race relations in Brazil and the United States indicates what we may expect when practitioners in one field enter another.<sup>40</sup>

Fifty years ago Haskins felt that one of the important obstacles to American research on European history was the deficiencies of our libraries. Today it is possible to pursue meaningful research on most areas of the world without leaving the United States, and in many fields our library resources are unsurpassed. A large volume would be required to do justice to this subject. Let these illustrations indicate the depth and range of the documentation available on foreign areas: in the period 1962–67, the Library of Congress offices abroad obtained 7.5 million publications from Ceylon, India, Indonesia, Nepal, the United Arab Republic, and Yugoslavia. Through this program forty other research libraries received sets of foreign-language publications, and 310 libraries received English-language sets.<sup>41</sup> The April 1965 issue of the Library of Congress's Monthly Index of Russian Accessions contained 487 pages of triple-column pages in small type.

Another way in which American historians might improve their world view would be to hold an annual meeting in Mexico City. We have met twice in Toronto: why not follow the example of the American Association for the Advancement of Science and other American professional associations by trying out the excellent facilities in Mexico City? It would be worth the trip alone to visit the Anthropological Museum there, a remarkable testimony to the Indian cultures whose study Sahagún initiated.

One possible danger must be mentioned. As our students and professors become more acquainted with the history and conditions of other tribes, will we become more sensitive to injustices committed abroad, especially to historians, and in consequence will we attempt to influence foreign nations in ways we consider desirable? Spaniards studied Indians largely as an aid to Christianizing them. Will the AHA look upon governments and historians that do not follow our ways as laggards in civilization who must be exhorted by formal resolution and even condemnation to follow our leadership on such explosive matters as civil rights and free speech?

These are gut issues on which honest historians differ. Thus far the Soviet Union's treatment of its dissident intellectuals has received most attention, but if relations between the People's Republic of China and the United States continue to increase, there will be other problems to confront. Although ethnocentrism can be found in many places—and there may be even today some Americans who would agree with Jameson that what the world needs is a large dose of American history to save it—China

<sup>&</sup>lt;sup>40</sup> Carl N. Degler, Neither Black nor White: Slavery in Race Relations in Brazil and the United States (New York, 1971).

<sup>41</sup> Dagmar Horna Perman, ed., Bibliography and the Historian (Santa Barbara, 1968), 59.

has one of the most completely closed civilizations ever developed in the world. Until the 1840s Chinese governmental and educational elites saw little need to study foreign languages or cultures, for all non-Chinese were considered barbarians. Those few who did study these subjects were dubbed "barbarian tamers" and tolerated because they performed an "odious and distasteful job, like sewer-inspectors," an attitude that lasted in some quarters well into the nineteenth century. Today there is a different orthodoxy in China, according to which Maoist values are enshrined as the ultimate repository of truth. Apparently we will see in China a conscious and continuous ideological orientation of historical scholarship, as has been the case for some time in other countries, which will make dissidence dangerous and unlikely.

Americans living in a pluralistic society where there is legal emphasis on individual rights and free speech look upon such cultures as subject to thought control, whether in China, Saudi Arabia, the Soviet Union, or elsewhere. Can historians from widely varying cultures find common ground to stand on? If one may judge from our experience in discussing Latin American history with Soviet scholars, the possibilities of a fruitful exchange of views must not be exaggerated.<sup>44</sup> Thus far these exchanges have been limited because of financial, linguistic, and political reasons, but it is likely that the coming generation will see a more wide-ranging and intense debate than ever before as historians discuss Latin America from the standpoint of their own tribes.

A final problem must be mentioned: the function of "tribal history." Do all nations—including the United States—need parochialism, naiveté, and myths to bind together their people? Does everyone need to cultivate

history, see John K. Fairbank, "Assignment for the '70's," AHR, 74 (1968-69): 861-79.

43 Herbert A. Simon, "Mao's China in 1972," Items, 27 (1973): 1-4. On ideological aspects of history writing in the German Democratic Republic and in Poland, see the review by George G.

Iggers, Journal of Modern History, 44 (1972): 149-52.

<sup>42</sup> John King Fairbank, Trade and Diplomacy on the China Coast: The Opening of the Treaty Ports (Stanford, 1964), 176. An important recent study by Donald W. Treadgold deals in detail with both Russian and Chinese responses to the West. The West in Russia and China (Cambridge, 1973). Other valuable studies of this complicated topic are Paul A. Cohen, China and Christianity: The Missionary Movement and the Growth of Chinese Antiforeignism, 1860-1870 (Cambridge, Mass., 1963), and Joseph R. Levenson, European Expansion and the Counter-Example of Asia (Englewood Cliffs, 1962). For an example of how cautiously one scholar worked to help his countrymen comprehend the nature of the outside world, see Fred W. Drake's account of the history-geography of Hsu Chi-yu (1795-1873). "A Mid-Nineteenth-Century Discovery of the Non-Chinese World," Modern Asian Studies, 6 (1972): 205-24. For a sophisticated and forthright statement on the role American historians should play in the study of Chinese history, see John K. Fairbank, "Assignment for the '70's," AHR, 74 (1968-69): 861-79.

<sup>44</sup> I. R. Lavretskii, "A Survey of the Hispanic American Historical Review, 1956-1958," Hispanic American Historical Review, 40 (1960): 340-60. This article originally appeared in Voprosy istorii, and the survey concluded "that the official Latin Americanists of the U.S. falsify and distort the historical truth in order to benefit imperialism" (p. 360). See also Russell H. Bartley, "On Scholarly Dialogue: The Case of U.S. and Soviet Latin Americanists," Latin American Research Review, 5 (1970): 59-62. This is an introduction to the article by M. S. Al'perovich, "Soviet Historiography of the Latin American Countries," ibid., 63-70. For a Mexican perspective, see Juan A. Ortega Medina, Historia soviética iberoamericanista (Mexico City, 1961).

self-sustaining, self-satisfying, and supportive notions about the virtues and unique qualities of the tribe he belongs to? Perhaps so, and if the tribal history can be kept within decent bounds by the perspectives of historians inside and outside the tribe, it may serve a useful purpose. It must be recognized, too, that not all members of a tribe accept the dominant interpretation of its history and that divergent opinions within a tribe affect the views of historians outside. A century ago Japanese educational leaders embraced the American dogma of hard work and individualism—"Boys, be ambitious" was the watchword transmitted to Japanese youth by William Clark of Amherst. 45 Japanese Americanologists such as Yasaki Takagi, who introduced a course on the United States at Tokyo University in 1918, were convinced that America was basically "a good country of good people." The generation after 1945 was not so sure, and it aimed at viewing America objectively and dispassionately. A new school, now gathering influence, "contends that earlier American studies in Japan, following the example of American scholars themselves, have ignored the problems of America's minorities—the blacks, Indians, and immigrant groups—and is in need of fundamental reform."46 Today Japanese textbooks no longer reflect the simplistic image that summed up for previous generations the message of America: "Boys, be ambitious."

If myths are useful when held by members of a tribe concerning its own history, they are less innocent and less justifiable when they are invoked to explain another culture. For example, during the Vietnam War the United States stressed its opposition to Communist North Vietnam as a totalitarian dictatorship similar to those found elsewhere. The difficulty with this argument Frances FitzGerald has made clear: "The non-Communist Vietnamese leaders believed in intellectual freedom no more than the Communists. . . . Intellectual freedom, of course, implies intellectual diversity." Is not one of the important reasons for paying attention to the history of other peoples to make sure that our understanding of their culture is not based on untenable myths?

However we may answer these questions, I believe that historians in this country now face a watershed, just as did that small group of teachers and writers who founded the AHA in 1884, who aimed to raise the teaching of history to a higher level because they were convinced that the local and

York, 1973), 22.

<sup>45</sup> Quoted by the staff of the Asahi Shimbun in *The Pacific Rivals: A Japanese View of Japanese-American Relations* (New York, 1971), 363. It is encouraging to see that some Japanese historians, such as Masuda Yoshio, are also calling, as Lothar G. Knauth has said, for "less parochialism among Japanese historians and the removal of barriers between the Japanese historians of Japan and those of foreign countries. Only in this manner, he insists, can Japan come to grips with the problem of its place in world history and overcome her relative alienation in Asia and in the world." "Pacific Confrontation: Japan Encounters the Spanish Overseas Empire, 1542–1639" (Ph.D. dissertation, Harvard University, 1970), 530–31.

<sup>46</sup> Staff of the Asahi Shimbun, Pacific Rivals, 355-57.
47 Frances FitzGerald, Fire in the Lake: The Vietnamese and the Americans in Vietnam (New

state spirit should give way to a larger, national view.<sup>48</sup> Our problem today is to find ways of strengthening all international aspects of history teaching and writing in the United States. I am convinced—and this may be an expression of my own ethnocentrism—that no nation today has a better opportunity than our own to attempt to study other cultures without necessarily losing the necessary life-giving and life-sustaining connection with our own national roots. A minority group like the Scots or the Catalans may have some justification for giving almost exclusive attention to their own history, lest they disappear as a distinct culture. But surely the situation is different in the United States, with its many different strains of cultures, with its economic and political power, and with the need to overcome or at least diminish and channel in other directions the force of what might be called its missionary zeal.

My hope is that in the great enterprise, whose dimensions I have barely sketched, organized American historians will have an important and even indispensable part. Today the AHA has more projects, more problems, and a larger budget than ever before. Among our 17,000 members is to be found an astonishing diversity of historical interpretations, life-styles, linguistic skills, and, yes, pizzazz. Surely this remarkable aggregation of human beings will be able to influence the study and teaching of history in international as well as national ways and to strengthen the already solid beginnings made here to study seriously the history of other nations and other peoples, while continuing to help Americans understand their present and future by providing an honest and informed picture of the past. When this day arrives we shall be achieving what Jameson hoped for: recognition of the fact that the history of the modern world cannot be fully understood unless foreign historians pay more attention to our history and recognition that United States history cannot be fully comprehended if isolated from world history.

Americans will then be ready for an even more difficult step, the initiation of fundamental revisions in their own views of the world, man, and the future, which began in the century of the great discoveries and for which Bernardino de Sahagún showed the way by his studies of Aztec culture. If American historians are fully aware of their opportunities and responsibilities in the world today, they can exert a powerful influence by their teaching and research to the end that we are able to appreciate the history of other peoples without losing allegiance to our own. By studying the history of their own tribes and other tribes as well, historians should be in the forefront of all those who would seek to understand the common elements in all cultures.

<sup>48</sup> David D. Van Tassel and James A. Tinsley, "Historical Organizations as Aids to History," in William B. Hesseltine and Donald R. McNeil, eds., Essays in Memory of Herbert A. Kellar (Madison, 1958), 62.

## Immigrants from Islam: The Crusaders' Use of Muslims As Settlers in Thirteenth-Century Spain

## ROBERT I. BURNS, S.J.

DURING THE SECOND QUARTER of the thirteenth century, King James the Conqueror of Arago-Catalonia led an international body of crusaders in a series of stubborn campaigns down the eastern coast of Spain. The crusade won an extensive kingdom of Valencia, roughly the size and shape of the crusader Holy Land, whose coastal cities bustled with commerce and whose hinterland comprised an agricultural cornucopia. Valencia spelled riches and rents for the happy crusaders as long as skilled manpower remained available to maintain the complex organization of its irrigated huertas and busy ports. Having expanded too far and too fast in an age of easy opportunity, however, King James found himself unable to induce more than a trickle of Christian settlers to come south. Where he needed a minimum of 100,000 settlers to guarantee basic military security, James confessed thirty years later, he achieved an eventual total of under 30,000. Worse, the crusade and subsequent rebellions caused a hemorrhage of Muslims away from towns and countryside toward the distant havens of Islamic Granada and North Africa.1

Though the crusaders, from the king down, had deployed fierce rhetoric about expelling all Muslims beyond the boundaries of the new kingdom, they had in fact gone to extraordinary lengths to retain in place every

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¹ Background on the crusade is in Robert I. Burns, S.J., The Crusader Kingdom of Valencia: Reconstruction on a Thirteenth-Century Frontier (Cambridge, Mass., 1967). On Mudejarism see his Islam under the Crusaders: Colonial Survival in the Thirteenth-Century Kingdom of Valencia (Princeton, 1974) and its sequel, Medieval Colonialism: Postcrusade Exploitation of Islamic Valencia (Princeton, 1975). A further work, "The Crusader-Muslim Predicament: Colonial Confrontation in the Conquered Kingdom of Valencia," is currently in preparation. Each book has ample bibliographical orientation. Other monographs and articles pertinent to the Muslim-Christian situation in Valencia include Burns's "Spanish Islam in Transition: Acculturative Survival and Its Price in the Christian Kingdom of Valencia," in Speros Vryonis, Jr., ed., Islam and Cultural Change in the Middle Ages (Berkeley, 1974); "Social Riots on the Christian-Moslem Frontier: Thirteenth-Century Valencia," AHR, 66 (1960-61): 378-400; "Christian-Islamic Confrontation in the West: The Thirteenth-Century Dream of Conversion," AHR, 76 (1971): 1386-1434; "Le royaume chrétien de Valence et ses vassaux musulmans," Annales: Économies, Sociétés, Civilisations, 28 (1973): 199-225; and his series of articles in Speculum from 1960 to 1971.

Islamic community and Muslim farmer or craftsman. Such exceptions as the expulsions at Valencia city and Burriana or the extrusion of aristocratic rebel groups prove upon close examination to support this general rule. Even where the surrender treaties allowed Muslim emigration to Islamic lands the crusaders sometimes sought to evade the permission. The banners of Muslim castellans still ruling from many strongholds and the call of muezzins from the ubiquitous minarets served to underline this deliberate establishment of Islam as a component of the new kingdom. Thus the crusade that began in paradox-when a Christian king joined forces with the last Almohad ruler in Spain to suppress an intra-Islamic revolt-ended in paradox: vowed before Christendom to expel the enemy, the king found himself as disturbed over the falling level of Muslim manpower as over the equally ominous predominance of Muslims. The apparent conflict between King James's determination to preserve his Muslim aljamas intact and his boasts of having expelled them has stimulated historians to variously implausible exegeses and resolutions. My own interpretation, offered elsewhere at length, points to minimal and reluctant expulsion, to a steady policy of retaining the Muslim communities in both cities and countryside and on Crown as on baronial lands, and simultaneously to serious losses on account of voluntary departure over the decades.2

In this context the fairly startling sight of crusaders enthusiastically importing Muslim settlers takes on more meaning. Following traditional precedent all classes of landlord, ecclesiastical and lay, could prosper in direct proportion to their ability to attract onto their lands fresh supplies of Muslims. This expedient muddied the clear image of crusade, as contemporary protest proves, but the prospect of revenues overrode all objection. The Crown desperately required such revenues, to continue its transformation from suzerain to sovereign status and to raise Arago-Catalonia higher in the ranks of the maritime powers. The barons, caught in a future shock that threatened to displace them like dinosaurs, scrambled even more greedily for rents and investments. Churchmen, including the military orders, had an omnivorous appetite for funds to meet increasing challenges. And the town or rural patriciate were cannily working their several ways up a golden ladder of prosperity and status. All participated in the game. It was a game disdained by the Holy Land crusaders, though parallels might be found elsewhere in Christendom.3

<sup>2</sup> Burns, "Expelling the Muslims from Thirteenth-Century Spain: A Revisionist Interpretation," paper delivered at the seventh annual meeting of the Middle East Studies Association of North America in Milwaukee, November 9, 1973.

<sup>&</sup>lt;sup>3</sup> Bela IV of Hungary (1235-70), unable to gain a European crusade to defend the east against the Mongols, intensified a policy of his Arpad dynasty by attracting French, Italian, German, and other immigrants; he especially brought in blocs of pagans, such as the detested Cumans. In the Holy Land, despite great need, Christian repopulation was the norm. See Claude Cahen, "Le régime rural syrien au temps de la domination franque," Bulletin de la faculté des lettres de Strasbourg, 29 (1951): 286-310; see also Joshua Prawer, "Colonization Activities in the Latin Kingdom of Jerusalem," Revue belge de philologie et d'histoire, 29 (1951): 1063-1118, including the economic and political crisis threatened by large-scale Muslim emigration (pp. 1083-85); and Prawer, The Crusaders' Kingdom: European Colonialism in the Middle Ages (New York, 1972), 82-84.

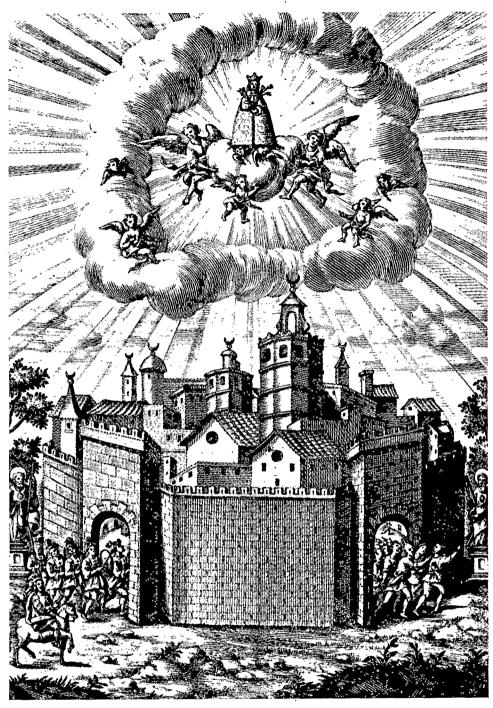


Fig. 1. A display from the celebration in 1740 of the expulsion of the Muslims from Valencia. The original, erected at St. Andrew's Church in Valencia, was a model of the Islamic city (basically a stylized eighteenth-century Valencia with crescents replacing crosses). Machinery kept the crusaders "in continual motion" entering at left and the Muslims going into exile at right, symbolizing the replacement throughout the kingdom. The Virgin of Victories, in the air, was believed to be a copy of the image King James honored in his tent during the siege. The flanking statues of Peter and Andrew each had a little relic of the respective saint in the niche inserted within its breast. Flowers and candles abounded around the model. The original picture, drawn from the huge model, is one of a number the artist drew from the many such displays in 1740. From Joseph Vicente Ortí Mayor, Fiestas centenarias con que la insigne, noble, leal, y coronada ciudad de Valencia celebró en el dia 9 de Octubre de 1738 la quinta centuria de su cristiana conquista (Valencia, 1740), facing page 125. A copy is in the Biblioteca Municipal of Valencia.

Though individual scholars have caught a glimpse of the immigration phenomenon at the periphery of one or other field of special interest, it has never been isolated for study. Consequently it has been easy to view the phenomenon as a rare expedient applied at random or as perhaps the replacement of expelled rebels at an inconsiderable number of places. But the total process, viewed whole, reveals a different scene. This merits direct confrontation, to establish its reality and universality as well as to open discussion as to its pattern and cumulative impact. To explore it fully one must combine ecclesiastical and secular documentation of varied genres; voices from the Vatican Archives must mingle with those from the manuscript collections at the king's Barcelona palace, and crusader treaties with Templar or cathedral instruments.

LONG BEFORE VALENCIA'S CONQUEST Christians had imported Muslims to augment a local working force or to extend an agricultural frontier. "Barons and other knights, as well as townsmen" under Alfonso II a century before so widely encouraged Muslim immigration, dividing their "estates and properties among Saracen owner-renters [exarici]," that "dissension was stirred up" with the Church over exemption of such tracts from Christian tithe. This quarrel, settled by royal decree in 1167, involved only land worked by Muslims under contracts drawn after the conquest—land never previously held by Mudejars and not owned by current Mudejar tenants.<sup>5</sup> A like dispute caused Innocent III to lodge a protest in 1199 with the bishop of Avila in neighboring Castile.6 The thirteenth-century Crown of Arago-Catalonia continued to allow the practice, as when King James in 1262 chartered the monastery of Piedra, near Calatayud in old Aragon, with a perpetual privilege of bringing in settlements of Christians and Muslims for three colonizing projects. Such shifts of population may have amounted only to internal immigration or movement from one point to another within the federated kingdoms of Arago-Catalonia. Surplus population at the point of origin, brighter opportunity at the point of arrival, or some

<sup>4</sup> Thus Elena Lourie, in the course of her investigation into "Free Moslems in the Balearics under Christian Rule in the Thirteenth Century," *Speculum*, 45 (1970): 624–49, speaks in passing of a colonizing movement of this kind on Majorca (p. 625), Ibiza (p. 631), and ambiguously Minorca (p. 632), and of two papal protests (p. 628).

<sup>&</sup>lt;sup>5</sup> King Alfonso II to the Church of Tarazona, Aug. 3, 1167, in Francisco Fernández y González, Estado social y político de los mudejares de Castilla, considerados en sí mismos y respecto de la civilización española (Madrid, 1866), app., doc. 8. The context makes it clear that the argument was not merely about a landlord's personal income derived from Muslim tenants but from the Muslim-held farm itself when the Muslim had been introduced after Christian ownership of the farm was an established fact.

<sup>&</sup>lt;sup>6</sup> Pope Innocent III to the bishop of Avila, May 21, 1199, Archivio Segreto Vaticano (hereafter Arch. Vat.), Rome, Reg. Vat. 4 (Innocent III), fol. 160v; also published in Demetrio Mansilla Reoyo, ed., La documentación pontificia hasta Inocencio III (965–1216) (Rome, 1955), doc. 193: "qui nolunt ecclesiis, sicut olim christiani solebant, freti potentia et favore illorum a quibus illis excolenda traduntur, decimas exhibere."

<sup>7</sup> King James I to Piedra monastery, Dec. 20, 1262; see Joaquín Miret y Sans, Itinerari de Jaume I "el Conqueridor" (Barcelona, 1918), 332.

adventitious factor such as the king's desire to thin out the Muslims of frontier zones may have been elements in a given transfer.

As a prelude to the Valencian crusade King James had been careful to conquer the flanking Islamic principalities on the Balearic Islands. His policies on this lesser stage were a dress rehearsal for management of the later Valencian kingdom. Thus colonization by transfer of Muslims was proceeding apace in the islands on the eve of the Valencian crusade. In 1231, the year before the crusaders started south, the Templars received a Crown charter to "settle and house thirty households of Saracens at whatever place you wish in your share" of Majorca. To attract the Muslims, King James exempted them and all their descendants from regalian taxes, placing them under his protection.8 This was no isolated instance. So assiduously did the Templars and Hospitallers colonize the Balearics with Muslims during the subsequent decade that Rome intervened. In 1240 Pope Gregory IX sent a brief rebuking the practice to Prince Peter of Portugal as Majorca's lord and to all landowners including the two military orders. Gregory urged that they "by no means permit the said islands to be peopled by Saracens" but only by Christians.9 Necessity overrode such appeals. Eight years later another pope, Innocent IV, found it necessary to launch a similar public directive, addressed this time directly to King James.10

Mudejarism in Valencia, though rooted in such traditional policy, had to be applied on a scale so extensive as to transmute the experience qualitatively. The importation or resettling of Muslims and the multiplication of Islamic aljamas consequently assumed unprecedented proportions. Almost immediately upon the fall of Valencia city, as the first extensive conquests underwent reorganization, the metropolitan archbishop of Tarragona considered it among his more urgent duties to excommunicate landlords importing Muslim settlers. This information has survived only in roundabout fashion. As part of maneuvers preceding an ecclesiastical trial for metropolitan control of the Valencia diocese, Toledo and Tarragona laid legal foundations by exercising such important acts of jurisdiction as giving indulgences and publishing excommunications. At the trial John Gonsalvo, from the household of the baron Peter Ferdinand, testified how "he saw

<sup>8</sup> King James I to the Templars of Majorca, July 8, 1231, excerpted in *ibid.*, 94: "populare et casare." Lourie believes these Muslims "had been imported from the Templar properties on the peninsula." "Free Moslems in the Balearics," 625–26. A. J. Forey finds her evidence for this "by no means conclusive." The Templars in the Corona de Aragón (London, 1973), 250n. Both authors miss the essential circumstance that the document refers to no specific Muslims, whatever their origins, leaving the Templars free to attract them at large; "quos habeatis proprios et francos" merely exempts from regalian control and taxes.

<sup>&</sup>lt;sup>9</sup> Pope Gregory IX to Prince Peter of Portugal, Jan. 25, 1240, Arch. Vat., Reg. Vat. 19 (Gregory IX), fol. 143v, letter 226: "dictas insulas ab eisdem sarracenis populari minime permittatis."

<sup>10</sup> Pope Innocent IV to King James, Feb. 28, 1248, Archivo de la Catedral de Palma de Mallorca, no. 13,451; also published in part by Jaime [with pseudoauthor Joaquín] Villanueva, Viage literario a las iglesias de España (Madrid, 1803-52), 21: 131.

him [the Tarragona archbishop] excommunicating all those who bring in Saracens to settle their estates." That this incident did not merely anticipate future abuses becomes apparent from the context; in the face of a captious rival, the archbishop had selected solid actions useful in a court of law to establish his ownership.<sup>11</sup>

The practice of importing Muslims also comes into view through the disapproval expressed by the first bishop of Valencia as early as 1240 in a tithe agreement with the Crown. "Saracens colonized on lands freely acquired, though we do not approve their use in populating and indeed rebuke it, are to give full tithe on everything." The text distinguishes them from Muslims who had surrendered during the past decade and kept their lands. The bishop, resigned to a continuing program of such immigration, provided for lands both "settled and to be settled from now on." 12

The decade after the fall of Valencia city saw the importation of Muslims swell until the situation got out of hand. The Tarragona metropolitan, with his suffragan bishops and King James, considered it grave enough to warrant a united appeal to Rome. In about 1245 they requested from Pope Innocent IV "a general sentence of excommunication against all in the kingdom of Valencia who, on places the Christians had taken from Saracens by armed force . . . , colonize Saracens or bring them back."18 Rome was slow in complying. Valencia meanwhile experienced a general revolt, with the subsequent expulsion of a number of Muslims. Not only the barons but religious orders and the king himself now joined the movement of introducing aliens in quantity. The Valencia bishop was particularly unhappy over this development. The newcomers got lands ideal for Christian settlement, further depriving the diocese of future tithes; the circumstance that in Valencia tithes went to diocesan authorities rather than to parishes surely sharpened his anguish. In answer to renewed complaint, this time from the bishop, Pope Innocent finally in 1251 chose to answer the king's plea for an excommunication. Embarrassingly enough, owing to these changed circumstances, the excommunication might now fall on King James himself.

The pope included a sharp reprimand. "Certain barons and exempt religious, and also others who . . . hold castles and towns as well as other possessions, have brought in and are bringing in Saracens to settle at many places of the same realm, contrary to the vow made by the said king and to the aforesaid [episcopal] excommunication, to the peril of their souls and the serious detriment of the church in Valencia—the king himself

<sup>11 &</sup>quot;Ordinatio ecclesiae valentinae," trial record edited in José Sanchis y Sivera, La diócesis valentina, nuevos estudios históricos (Valencia, 1921), 360.

<sup>12</sup> Tithe agreement between the bishop of Valencia and King James I, 1240, Archivo de la Catedral de Valencia (hereafter Arch. Cat. Val.), parchment 1,304.

<sup>13</sup> The date of the original letter is unknown, but information about it is contained in the reply. Pope Innocent IV to the bishop of Valencia, Dec. 23, 1251, Arch. Vat., Reg. Vat. 22 (Innocent IV), fol. 133v, letter 146: "popularent sarracenos vel reducerent."

doing the very same thing in some places." Innocent recognized that circumstances were abnormal. Instead of pressing for cessation he returned the situation into the hands of Valencia's bishop, stating his anxiety for the souls of the king and other malefactors and his concern over destruction of the diocese's financial underpinnings wrought by such colonization. For the good of souls and the new kingdom Innocent directed the bishop to act as seemed best "about commuting the king's vow, expelling or keeping the Saracens, relaxing or strengthening the aforesaid excommunication, as also about the other matters above." He adjoined the condition, "provided that by settling of Saracens the Church herself is in no way defrauded of the tithe and her other rights in any place." 14

Pope Innocent's decision was prudent. Given the system of Mudejarism in effect, which established all Islamic communities of Valencia as an enclave society in many ways autonomous, it must have seemed logical to expand it wherever necessary or reasonably useful. It must also have seemed inevitable. If either party—royal and burgher, or baron and knight—propped up its failing finances in this way or realized any economic advance in this era of social struggle the other party would certainly follow suit. Both parties could sidestep ecclesiastical penalties by pleading such excusing causes as the many Catalan graduates of Bologna's law faculty might hastily devise. After all, Majorcan neighbors had entered upon forbidden trade with the Muslim enemy and had recently won papal permission to continue it by their plea that "a new settlement" like Majorca could not hope for increased Christian immigration without this commerce. 15

By 1283, when a revision of the Valencian kingdom's law code restored baronial privileges that had recently suffered erosion, King Peter took care to insert a statute reaffirming that "any person of the city and realm can send Saracen workers to work his estates for a period of time or in perpetuity." The same sentence distinguished newcomers from Muslims "who already reside on the same estates." Stabilizing four decades of evolution, this statute may serve to introduce a roll call of sample cases that illustrate and clarify this fascinating story. 16

SUFFICIENT EVIDENCE OF A GENERAL NATURE remains to establish the movement's existence and scope. Particular documentation, for this as for most other facets of thirteenth-century Valencia, survives only in random samples.

<sup>14</sup> Ibid.: "quidam tam barones quam religiosi exempti ac alii etiam qui castra et villas necnon et possessiones alias possident sarracenos in plurima loca eiusdem regni, contra votum dicti regis emissum et predictam sententiam, in suarum animarum periculum et grave preiudicium ecclesie Valentine ad populandum induxerint et inducant eodem rege faciente in locis aliquibus illud idem."

<sup>&</sup>lt;sup>15</sup> [Pope Innocent IV] to the bishop of Majorca [confirming the permission of Pope Gregory IX], Mar. 21, 1248, *ibid.*, Reg. Vat. 21 (Innocent IV), fol. 516v, letter 682.

<sup>&</sup>lt;sup>16</sup> Statute in the Valencian law code, Fori regni Valentiae, ed. F. J. Pastor and P. J. de Capdevila (Monzón, 1547-48), bk. 8, rubric 8, no. 28.



Fig. 2. A thirteenth-century Mudejaresque plate from the celebrated Moorish ceramics center of Paterna, in the Valencian huerta just upriver from Valencia city. The figure represents the lord of Paterna, bearing his Aragonese Artal de Luna coat of arms as differenced for Paterna. This is the kind of local lord then settling Muslims on his estates. The Mudejaresque production recalls the symbiosis of Christian lord and Islamic subject. The source, ceramics, suggests a neglected historical source and a new methodological approach. Less than half life-size here, the plate has a characteristically greenish cast, interworked with brown. The original is in the Museo Nacional de Ceramica "González Martí" de Valencia.

They are frequent and clear enough, however, to fill in those broader outlines. The fairly well-documented military orders, for example, had many Muslim tenants throughout their estates. They were also accustomed to importing supplementary workers, particularly slaves or prisoners of war. At the surrender of Chivert in 1234 the Knights Templar took care to lure back families who had fled abroad during the crusade, inviting "any Saracen of Chivert who has taken up residence in a land of the Saracens" to return within a year and recover his "houses and estates and other possessions" without loss. If the owner wished to remain in international exile or died before being able to return, his heirs received all properties, the complexities of Islamic inheritance law prevailing. A similar proviso appears in the sur-



Fig. 3. A thirteenth-century Mudejaresque plate from Paterna. Christian settlers gradually infiltrated the Muslim ranks in this Hispano-Arab ceramics center and adapted its art. Although Muslims long dominated here, it is possible that this plate is by a Christian. Its conjunction of two characteristic zoomorphic themes, the peacock and the fish, is understood by some scholars to symbolize the coexistence in Valencia of Islam and Christendom. The plate is about a foot in diameter and is dominantly green in color, with some brown trim. The original is in the Museo Nacional de Cerámica "González Martí" de Valencia.

render charter by the Crown to the Muslims of Eslida, and by implication in the charter of Uxó. On balance it seems probable that this was a normal element in any surrender or amnesty.<sup>17</sup>

17 Joaquín Miret y Sans, Les cases de templers y hospitalers en Catalunya, ap!ech de noves y documents històrichs (Barcelona, 1910), 145, and his "Inventaris de les cases del temple de la corona d'Aragó en 1289," Boletín de la real academia de buenas letras de Barcelona, 6 (1911): 72: "quantos sarracenos habetis in honoribus vestris . . . aut illos que de aliis partibus pro amore Dei adduxeritis vel de Ispania quos Deus ibi vobis dederit"; "Spain" meant the Islamic parts; carta puebla de Chivert, Apr. 28, 1234, no. 76 in "Colección de cartas pueblas," Boletín de la sociedad castellonense de cultura, 24 (1948): 226-30; carta puebla de Eslida, May 29, 1242, in Fernández y González, Estado social y político de los mudejares de Castilla, app., doc. 17.

After the conquest and first settlement of the Valencian kingdom had rendered secure the southern border regions of the old kingdom or province of Aragon proper, the Templars introduced Muslim settlers there, at Villastar near the central Valencian frontier. Previously unsuccessful in attracting sufficient Christians, the order set up this aljama of thirty Muslim families in 1267; besides the newcomers, probably from nearby Valencia, the only other residents were four Christian farmers and the Templar garrison at the local fort. The number of Muslims, recurring in other documents, suggests that something over a hundred souls made for a stable and varied rural community. The Hospitallers, an order experienced in settlement projects, similarly imported Valencian Muslims to Aldea, just over the northern border of the conquered kingdom: "Saracens were brought up from Silla with our permission and settled by the master and brothers of the Hospital in the aforesaid place." The year was 1258, a particularly active time for rearranging population balance because of the recent revolt. In the same year the religious Knights of Calatrava colonized their Burriana estates with Muslims. During an earlier flurry of expulsion and settlement, the Hospitallers in 1248 had arranged to settle a hundred Muslim families at the Albufera lagoon in order to supply boatmen for their fleet of thirty fishing ships.18

An early bishop of Valencia, more flexible than his excommunicating predecessor, brought in Muslim settlement for diocesan lands at Garg; King Peter approved the project in 1280, adverting to two classes of colonizers. "Know that we... have granted the venerable bishop of Valencia that all the Saracens native to Garg may come, if they wish, to settle in that castle and to dwell there; likewise also those [alien] Saracens... who came to settle wherever we would want them, may come if they wish to settle, as is said above, at the above-mentioned castle of Garg." In support of the program a decree went out next day ordering Crown officials to respect this charter. By anticipation, King James had pregranted Tales to the

The Uxó charter excluded fugitives who had recently besieged the king there: "e tots aquells que isqueren de la vall de Uxo, e no[s] forem assetjats ab ells en lo dit castell." Aug. 1250, in *ibid.*, doc. 23. Both its position and its wording oppose the interpretation of Miguel Gual Camarena that it was a general prescription. "Mudéjares valencianos, aportaciones para su estudio," Saitabi, 7 (1949): 172. The Játiva charter made provision "quod si aliquis serracenus venerit ad populandum in ravallum predictum" and wished to leave after one or several years, he could do so. Carta puebla de Játiva [Jan. 23], 1251 or 1252, in Fernandez y González, Estado social y político de los mudejares de Castilla, app., doc. 24.

<sup>18</sup> Carta puebla de Villastar, July 15, 1267, in Forey, Templars in the Corona de Aragón, app., doc. 24, and see pp. 218, 221; King James I, charter of protection to Muslim settlers from Silla, Feb. 19, 1258, in Colección diplomática de Jaime I, el Conquistador, ed. Ambrosio Huici Miranda (Valencia, 1916-22), 3: 187, doc. 766; King James I to the Knights of Calatrava, Mar. 16, 1258, in Ramón de María, ed., El "Repartiment" de Burriana y Villarreal (Valencia, 1935), 71-72; King James I to Hospitallers, Feb. 5, 1248, in Francisco Diago, Anales del reyno de Valencia (Valencia, 1613), 1: 346, a transcription from the archives of Valencia kingdom of a document now lost. For background on the Hospitallers' skill and role in bringing in settlement as a general policy, see Burns, Crusader Valencia, 1: 189-90.

<sup>19</sup> King Peter III, charter in favor of the bishop of Valencia, May 10, 1280, Archivo de la Corona de Aragón (hereafter Arch. Crown), Barcelona, Peter III, Reg. Canc. 44, fol. 182. The decree of May 11, 1280, is in *ibid*.

knight Peter of Castellnou as early as 1225, to populate with thirty Muslims. After conquest and pacification of the area, Castellnou in 1260 offered the Muslims a liberal constitution.<sup>20</sup> The king also licensed Bernard of Juneda in 1258 to settle Muslims in this region:

We and ours grant permission and power to you Bernard of Juneda, resident of Onda, that you may settle Saracen men and women, whomever and in whatever number you desire, on the lands you hold in the settlements of Artesa, Tales, and Cavallera, which are in the district of Onda; which Saracen men and women you and yours are to hold forever [as tenants] . . . to do with freely as you will.

They were to move, with all their goods, under the king's special safe-guard.<sup>21</sup>

Simon Pérez of Foces received a section in Benejama for this colonial purpose. In 1258 he outlined a housing project for his Muslim pioneers. King James chartered it:

We assign as your own property frank and free, to you Simon Pérez of Foces and to yours forever, one open space for making and building houses in the village of Benejama in the district of Almizra; in which space and houses you are to settle men and women Saracens, however many and whomever you wish and to whomever they belong.

Here the king clearly overrode seignorial, ecclesiastical, or other jurisdictions, confirming the freedom of movement enjoyed by Muslim farmers (exarici) and townsmen alike. He seems to have anticipated only intra-Valencian migration here. At about the same time King James licensed Bernard of Claramunt "to populate Saracens on your estate" and took Bernard's Muslims "under our guard and protection."<sup>22</sup>

In 1257 at the height of al-Azraq's revolt, when giving to the baron Carroz of Rebollet the castles and regions of Jalón and Vall de Laguart, the king stipulated in the settlement charter "that you can give and establish temporarily or in perpetuity houses and farms in the aforesaid castles and towns, and in their villages and districts, to Saracen men and women only." Awarding the village of Alcocer, near Cocentaina, to Martin Ximénez of Soraure in 1258, King James exempted from tax there and on Martin's Alcoy estate, "forever all Saracens who have been settled and whom from now on you will settle." A final example from this flurry of settlement projects during this disturbed period of the late 1250s was the complicated transfer of an Alcira colony. The owners had lost the area by neglecting to take up residence, but they illegally sold it to William of Llauró, who then went to great expense improving it. The king first seized it as forfeited "and gave

<sup>&</sup>lt;sup>20</sup> King James, pregrant of Tales, Mar. 13, 1225, no. 22 in "Colección de cartas pueblas," Boletin de la sociedad castellonense de cultura, 11 (1930): 88–89; carta puebla de Tales, May 27, 1260, no. 84 in ibid., 28 (1952): 437–38.

<sup>21</sup> King James I to Bernard of Juneda, May 31, 1258, Arch. Crown, James I, Reg. Canc. 10, fol. 70.

<sup>&</sup>lt;sup>22</sup> King James I to Simon Pérez of Foces, June 28, 1258, *ibid.*, fol. 182; King James I to Bernard of Claramunt, Sept. 29, 1259, *ibid.*, Reg. Canc. 11, fol. 151v.

it to Saracens" but then in consideration of William's expenditures turned it back to him as landlord.<sup>23</sup>

A strange mandate went from the Crown in 1268 to two tax farmers, the knight Roderick Martin of Azagra and his partner the Crown agent Peter Diego. They received all the revenues of the Cocentaina Moors for five years, in return for a total payment of 10,000 solidi to the king in biannual installments, but on condition "that you so improve the said Moorish section that at the end of the aforesaid five years you restore to us or ours the said improved Moorish section with double the number of Saracens that reside there today." Failure to double the population would incur a penalty of 3,000 solidi. Probably most Muslim settlement was on the plan of expanding already existing aljamas or of bringing in as many individuals as feasible. Alfonso of Murcia may fit this pattern. Around 1264 he secured a permit for his modest settlement of Enova; King James allowed him "full permission . . . that you may have in your township [alcheria] . . . twelve Saracens who are to cultivate your township; and we take the aforesaid Saracens under our protection as of now."<sup>24</sup>

In 1268 King James chartered William of Rocafull, a sometime Crown lieutenant at Montpellier, to colonize his Fortaleny estates "with twenty households of Saracens." The decree given in 1274 to Raymond of Balbs from Ripoll, castellan at Calpe, had a more general effect: "While you hold the said castle for us you may... establish both Christians and Saracens on your estates." A directive of 1279 instructed the Jewish functionary Samuel to "establish Christians and Saracens, or Christians alone, or Saracens alone, as may seem best to you and for our advantage" at Alcudia, Favara, and Benibochir.<sup>25</sup>

Some of the instruments issued by the Crown related more indirectly to the immigration movement. A whole genre concerned itself with inviting rebels and fugitives to return after civil disturbances.<sup>26</sup> Property grants to

<sup>23</sup> King James I to Carroz of Rebollet, Sept. 19, 1257, ibid., Reg. Canc. 9, fol. 39v; King James I to Martin Ximénez of Soraure, July 1, 1258, ibid., Reg. Canc. 10, fols. 83–83v; King James I to William of Llauró, Oct. 4, 1257, ibid., Reg. Canc. 9, fol. 42v. In 1258 James granted to John Mur [de Muro] fourteen houses with farms "in alqueria que vocatur Nahuges que est de termino de Sexona," anticipating growth of the resident Muslim population by incoming Moors "qui... de cetero fuerint." King James I to John Mur, Feb. 13, 1258, ibid., fol. 55v.

<sup>24</sup> King James I to Roderick Martin of Azagra and Peter Diego, Feb. 10, 1268, ibid., Reg. Canc. 15, fol. 136; King James I to Alfonso of Murcia, July 22, 1264, ibid., Reg. Canc. 13, fol. 203. <sup>25</sup> King James I to William of Rocafull, Aug. 27, 1268, ibid., Reg. Canc. 15, fol. 115v: "viginti casatas"; King James I to Raymond of Balbs from Ripoll, Jan. 4, 1274, ibid., Reg. Canc. 20, fol. 204v; King Peter III to Samuel, Dec. 4, 1279, ibid., Peter III, Reg. Canc. 42, fol. 186v. See also King James I to the bailiff of Denia, June 6, 1273, ibid., James I, Reg. Canc. 21, fol. 141v: "christianis iudeis et sarracenis ad utilitatem domos et operatoria et plateas et alias terras que et quas in Denia et in Calpi et in terminis suis inveneritis ad donandum"; and King James I to Teresa Giles of Vidaure and colleagues, Mar. 27, 1261, ibid., Reg. Canc. 11, fols. 197v–98: "eas donetis christianis vel sarracenis ad laborandum, prout melius vobis videbitur expedire, qui ibidem residentiam faciant et vicinaticum, et eas laborent."

<sup>26</sup> See, for example, revenue accounts by Ade of Paterna to King James I, Apr. 27, 1266, *ibid.*, Reg. Canc. 17, fol. 67: "redditus de Pego, vi millia solidorum, de quibus dimisit ut redirent sarraceni, ccc solidos." See also Prince Peter (later Peter III) to the Muslims of Pego Valley, Dec. 5, 1272, *ibid.*, Reg. Canc. 37, fol. 56v: "universis sarracenis vallis nostre de Pego et successoribus vestris illas donationes seu concessiones quas Iacobus de Linariis baiulus noster . . . fecit usque in presentem diem"; King Peter III to Muslims of Seta, July 20, 1279, *ibid.*, Peter III,

single Mudejars or to groups, either in postcrusade distribution or by special favor, frequently fall under the rubric of colonial settlement. Moreover resettlement assumed a variety of guises. In 1274 King James threw open lands along the Alcira irrigation system to "all and single Saracen" immigrants. In 1259 he licensed the  $q\bar{a}^{\ \ \ \ \ }id$  of Montesa to "populate with Saracens" a Játiva village. Aljamas expanded, as did affluent Mudejar landholders, by substantial purchases. King Peter continued his father's work, calling in Muslims at, for example, Beniopa, Bocairente, a dozen towns in the Denia region, and Valencia city; he even ejected Christian squatters from lands "awarded to Saracens settling." Like his father, too, he often licensed settlement for mixed "Christians and Saracens" indifferently.

Many a settlement effort is revealed by a single document, but for Villarreal a small cartulary of six letters can be assembled. Here we watch the Crown check and clear previous title, offer the lure of tax relief, and even construct for each immigrant "one house roofed over and one corral enclosed by mud walls." A run of documents for the underpopulated

Reg. Canc. 44, fol. 149: "qui nunc ibi sunt et ex nunc ad populandum ibi possint adorare et legere in meçquitis eorum," at Seta, with general amnesty; King Peter III to Muslim settlers at Rafelbuñol, July 15, 1280, *ibid.*, fol. 185: "et universis sarracenis habitantibus et de cetero habitantes in Raffalbuynol . . . salvi, quieti, et securi." See also note 33 below.

27 See, for example, King James I, directive of June 30, 1269, Arch. Cat. Val., parchment 5,997, a severe order "quod alamini sarracenorum villarum et locorum regni Valencie" tithe on granted or purchased property, "alias quod distringantur" by the general bailiffs of Valencia city and Játiva; and King James I to his crossbowman Sa°d, June 25, 1272, Arch. Crown, James I, Reg. Canc. 37, fol. 48: "confirmamus vobis Çaat Abeniali ballistario sarraceno nostro de Pego domos et hereditatem quem tu [sic] tenes in valle nostra de Pego . . .; tu vero teneas ibi facere residenciam personalem." See also doc. of June 30, 1272, in Roque Chabás, ed., "Sección de documentos," El archivo, 4 (1890): doc. 37: "in quam videlicet locavimus iam sarracenis" a stretch of vineyards. The Repartimiento de Valencia records any number of such gifts. Prospero de Bofarull et al., eds. Colección de documentos inéditos del archivo general de la corona de Aragón (Barcelona, 1847-1910), 11: 177, 184, 195, 225, 248, 271-72, 285, 299, 343, 377, 483-84, 497, 508, 568, 626, 632.

<sup>28</sup> King James I, tax exemption to Muslim settlers, Apr. 16, 1274, Arch. Crown, James I, Reg. Canc. 19, fol. 124: "sarracenos qui populabunt in populacione quam facimus subtus cequiam Algezire"; King James I to  $q\bar{a}^{\bar{c}}id$  of Montesa, Sept. 29, 1259, ibid., Reg. Canc. 11, fol. 152: "populetis et detis ad populandum sarracenis."

<sup>29</sup> Prince Peter (later Peter III) to the Muslims of Pego Valley, Dec. 22, 1272, ¿bid., Reg. Canc. 37, fol. 57v: "toti universitati sarracenorum vallis de Pego hereditatum quam vos emisisti a Petro Mir cive Valencie in predicta valle."

so King Peter III to Raymond of Sant Leir, Apr. 21, 1279, ibid., Peter III, Reg. Canc. 46, fol. 6v: "sarraceni loci eiusdem et qui ibi venerint ad populandum"; King Peter III to Peter of Bolcha, June 11, 1281, ibid., Reg. Canc. 49, fol. 103: "universis sarracenis volentibus venire ad populandum apud locum nostrum de Bocayren." King Peter III to Muslim settlers from Ibi and elsewhere, Mar. 16, 1277, ibid., Reg. Canc. 40, fol. 74: "populamus vos universos et singulos sarracenos de Ibi et quoscumque alios sarracenos qui in Deniam veniatis ad populandum ad çunam sarracenorum . . . et enfranquimus similiter omnes sarracenos qui ibicem venerint ad populandum a die qua venerint usque ad unum annum continuum subsequentem." On the Denia region see also King Peter III to Muslims from Moncanet, Nov. 14, 1279, ibid., Reg. Canc. 44, fol. 160, and King Peter III to Moses of Alcira, Dec. 22, 1279, ibid., Reg. Canc. 42, fol. 193: "sarracenis populantibus in dicto loco."

31 King Peter III to Muslims from Castalla and Biar, Sept. 12, 1279, ibid., Reg. Canc. 42, fol. 137: to the Muslims "in frontaria de Castalla et de Biar." For Villarreal see the charters of King Peter III: Feb. 22, 1280, ibid., fol. 222; Jan. 10, 1281, ibid., Reg. Canc. 52, fol. 19v: "cuicumque sarraceno venienti ad populandum predictum locum de villa Regali faciatis in arraval eiusdem loci unam domum cohopertum et unum corrale clausum de tapiis"; and May 13, 1280, ibid., Reg. Canc. 48, fol. 20v; see also May 13, 1280, ibid., fol. 16gv, and Mar. 31, 1277, ibid., Reg. Canc. 39, fol. 177v.

quarters at Valencia and Játiva, capital cities for the north and south respectively, betrays similar zeal from the reign of King James through that of Alfonso.<sup>32</sup>

Correlation between the chronologies of revolt and immigration yield no firm conclusion as to causal connection, since both movements characterized each decade from the 1240s through the late 1270s. Geographical correlation of both phenomena, during the period 1277 into 1281 for example, does not encourage the hypothesis of connection as the general explanation; too many settlements like Alcira and Villarreal were outside the zones of rebellion, and too many rebel centers do not figure in the surviving documents. The particular and general amnesties do less to explain Muslim immigration than to illustrate the sustained Christian desire for it. Postrebellion activity formed part of a continuing policy, which required more zealous implementation in periods of expulsion or mistrust.

A NUMBER OF DOCUMENTS specify Islamic lands as a source of immigrants. King Peter invited Muslim fugitives back "from any kingdoms or regions" and expressly from "Saracen" countries. His successor Alfonso similarly sought not only los acoleiats but also the "foreign Saracens." Much of the documentation, on the other hand, might be understood either as truly foreign immigration or else as a shuffling of population within the realms. Which model dominated here, the intramural or the international? For a new kingdom only just conquered and comprising little more than a Christian-garrisoned land of aliens, such a distinction is more formal than real. In any case, given the scope of the movement, the diversity of Islamic Valencian locales, and the extent of the conquered area, the restrictive interpretation would still leave a remarkable migratory movement.

<sup>32</sup> King Peter III to the bailiff of Valencia, Sept. 6, 1280, *ibid.*, Reg. Canc. 44, fol. 190v: "universos et singulos sarracenos venientes ad populandum et habitandum ad moreriam Valencie"; King Peter III, charter of appointment, Sept. 6, 1280, *ibid.*, Reg. Canc. 48, fol. 190v. See also the documents in Francisco A. Roca Traver, "Un siglo de vida mudéjar en la Valencia medieval (1238-1338)," Estudios de edad media de la corona de Aragón, 5 (1952): app. doc. 7, Feb. 16, 1277: "qui venerint ab habitandum ad revallum Valencie"; doc. 8, May 5, 1278: "qui sunt et venient ad populandum in moraria Valencie"; doc. 25, Sept. 9, 1290: "ut melius dicta moraria populeter." An example of the similar set of documents for Játiva is in note 33. Alfonso also settled "viginti casatos sarracenorum" in the Peñíscola district. King Alfonso III to William of Coret, Oct. 15, 1286, Arch. Crown, Alfonso III, Reg. Canc. 64, fol. 129.

33 King Peter III to the Muslims of Cuart, July 10, 1279, Arch. Crown, Peter III, Reg. Canc. 42, fol. 149v: "si aliquis sarracenus vel sarracena de terra regis affugerit ad loca sarracenorum et de ipsis partibus reverti voluerit ad populandum ad Quart"; King Alfonso III to the Muslims of Játiva, May 3, 1287, ibid., Alfonso III, Reg. Canc. 75, fol. 5: "concedentes omnes tam sarraceni nostri . . . quam extranci qui venerint ad populandum"; this was to settle "ravallum ipsius loci Xative et alia loca nostra." An exea, usually employed for protecting foreign Muslims, guided them. See Reg. Canc. 74, fol. 26. See also King Peter III to officials throughout his realms, Jan. 1, 1277, in Ferran Soldevila, Pere el Gran (Barcelona, 1950–62), pt. 2, vol. 1, doc. 51: "quatenus si contingat aliquos sarracenos fugitivos venire ad loca nostra ab aliquibus regnis seu partibus, eosdem emparetis a quibuscumque teneantur, et eosdem salvetis, manuteneatis, et deffendatis . . . et permitatis ipsos esse in morariis salve et secure, sicut alios sarracenos." Though not without ambiguity, this seems to refer to Valencian fugitives, Crown and non-Crown, seeking to settle across the border in Aragon proper.

The restrictive interpretation also raises the question of where intramural populations came from: the immigrant direction was toward baronial as well as Crown lands, toward urban districts where Christians settled as well as toward the countryside. Towns were not left standing half empty as the result of the movement, nor did landowners raise protest that their own areas were being denuded by the invitations of competitors. Some documents concern local migration or expansion, but they do not seem to typify the general movement. It is significant that with the exception of these occasional special projects the invitations do not specify Mudejar origin or restrict immigration to recently conquered natives. Some migrants came from the older provinces of James's federated realms or from Castilian zones of reconquest, but not in sufficient numbers to evoke repercussions at the point of origin.<sup>34</sup>

After its definitive conquest in 1266, and partly owing to the very different patterns of land distribution and exploitation by the Castilians, neighboring Murcia lost a substantial portion of its Muslim population;<sup>35</sup> Valencia may well have seemed a more inviting alternative than faraway North Africa in the eyes of the average poor farmer, especially in light of the Spanish chauvinism that distinguished these Muslims. Great numbers of Murcians went to Tunis, however, and anyway, Valencia's immigrant influx was notable as early as 1240 and highly visible in the fifties. On the other hand, these were decades of despair and turmoil in many parts of Mediterranean Islam; the last Abbasid caliph of Baghdad fell in 1258 before the irresistible Mongol hordes, while the last Almohad caliph of western Islam fled from Marrakesh in 1269 after decades of civil wars and fragmented principalities in North Africa. The highly mobile Islamic society surely sent streams of refugees in all directions.36 Superficially it might seem that hardly any Muslims would make their way from North Africa up to Valencia, but in view of the extraordinary detente and then alliance between King James's realms and Hafsid Tunisia (Ifriqiya), and with the steady circular interchange of traders, diplomats, soldiers, and travelers that was making eastern

<sup>34</sup> Xavier le Cour Grandmaison found that nearly all Mudejar emigrants from the Balearics went to North Africa or Granada, but that over ten per cent each year chose Valencia, a percentage not far below that which the Granada group represented. "De Majorque au Maghrib, l'émigration des musulmans de Majorque entre 1344 y 1381" (Ph.D. dissertation, University of Algiers, 1966). See Charles Dufourcq, "Les relations de la péninsule ibérique et de l'Afrique du nord au xive siècle," Anuario de estudios medievales, 7 (1971): 57-58.

<sup>35</sup> Juan Torres Fontes, "Los mudéjares murcianos en el siglo xiii," Murgetana, 17 (1961): 16, 24, 26–27; very few Muslims were settled in those regions by the Castilian Crown (p. 20).

<sup>36 &</sup>quot;Mediterranean man in the Middle Ages was an impassioned and persevering traveler" even in normal times. For centuries the sea trip between places like Egypt and Islamic Spain had been a "humdrum" event for businessmen, and "commuting regularly" between the eastern and western ends of the Mediterranean was "nothing exceptional." Many ordinary folk emigrated in search of a livelihood, though naturalization required long residence and complications. Upheavals like the Almohad breakup "caused mass emigrations." For the Islamic and even the European scene in general, see S. D. Goitein, A Mediterranean Society: The Jewish Communities of the Arab World As Portrayed in the Documents of the Cairo Geniza (Berkeley, 1967-), 1: 42-70, 53, 57, 59, 273-81.

Barbary "a financial province of the Catalans' commercial empire," such a possibility must be seriously entertained.<sup>37</sup>

Some may have made their way by sea, in the flux of alien merchants and tourists reflected in the earliest Valencian customs lists. Poor folk or the adventurous may well have been attracted by free land and the region's fabled charms, since the rural environment remained as yet so resolutely Islamic and the web of Tunisian-Catalan understandings and interchange reinforced the privileged Mudejar state-within-a-state. The crusaders who rented armies to North African potentates and who welcomed back all alguebers or fugitives from their panicky exile on those far shores would hardly have refused immigration applicants from overseas.38 Pope Innocent's condemnation of Muslim settlement in Valencia, in fact, indifferently damns in the same breath barons "who settle Saracens or bring them back." Any number of personal names in Valencian crusader records indicate such alien connections, though without interpretative context. When an official bears the name of Muhammad of Salā or Mūsā of Morocco, or when "a certain Saracen from Murcia" owns a small town, their personal histories remain obscure.39

Paradoxically there is more evidence of increases in overseas immigration from the next century, when settlers were less welcome and therefore fenced by restrictions; thus, in the early fourteenth century an illegal entrant from North Africa was caught after residing peacefully for a year.<sup>40</sup> Postcrusade

37 Charles Dufourcq explores the whole range of interchange, with the single exception of the movement under discussion, in his magisterial L'Espagne catalane et le Maghrib aux xiiie et xive siècles, de la bataille de Las Navas de Tolosa (1212) à l'avènement du sultan mérinide Abou-l-Hasan (1331) (Paris, 1966), especially chs. 2, 3; see for example pages 69-76; the summation on pages 128-31, with its characterization of Ifriqiya as "une province financière de l'empire commercial des Catalans"; and the assessment of cordiality versus antagonism on pages 185-89. On the missionary penetration of Islam, see also Burns, "Christian-Islamic Confrontation in the West"; and see Burns, "Renegades, Adventurers, and Sharp Businessmen: The Thirteenth-Century Spaniard in the Cause of Islam," Catholic Historical Review, 58 (1972): 341-66.

38 Miguel Gual Camarena has gathered the early tariff lists of the Valencian kingdom in his Vocabulario del comercio medieval, colección de aranceles aduaneros de la corona de Aragón (siglos xiii y xiv) (Tarragona, 1968). For Muslims see doc. 3, Sept. 24, 1243, for Valencia city, no. 77: "sarracenus qui transeat per mare vel per terram," and no. 78; doc. 6, Mar. 10, 1250, for Alcira, no. 66; doc. 7, Sept. 1, 1251, for Biar, Burriana, Játiva, and Murviedro, no. 58: "sarracenus qui transeat . . causa vendendi," no. 59: "sarracenus de redemptione," and no. 67: "sarracenus alforre [liber], qui exeat de Regno Valencie"; doc. 12, 1271, for Valencia city, nos. 86, 87, 96, 145; doc. 13, 1271, for Valencia city, nos. 86, 87, 95, 151. An important element in Valencian customs duties, of course, was the "traffic in emigration" as well as in slavery and redemption; see Dufourcq, L'Espagne catalane et le Maghrib, 76-82. On this traffic and on the alguebers, see Burns, "Expelling the Muslims from Thirteenth-Century Spain."

39 King James I, tax record, Apr. 27, 1266, Arch. Crown, James I, Reg. Canc. 17, fol. 66: "de alqueria cuiusdam sarraceni de Murcia." For Muhammad and Müsä, see Burns, Islam under the Crusaders. 288, 200.

under the Crusaders, 238, 399.

40 Johannes Vincke, "Königtum und Sklaverei im aragonischen Staatenbund während des 14 Jahrhunderts," Gesammelte Aufsätze zur Kulturgeschichte Spaniens, 25 (1970): app., doc. 43. Such entrants were few in Valencia, compared to the Granadan flow, because the land approach was less guarded than the sea. Even in the sixteenth century, Muslim immigrants came from abroad, though their pseudo-Christian status made this easier. Witnesses for the inquisitorial trial of Cosme b. 'Amir (Abenamir) included small-town Valencian Moriscos who had been natives of "Barbary," Cairo, Morocco, and Tlemcen. See the trial record (1567) in Pascual

Valencia, a more stable Islamic community than either Almohad Spain in its precrusade decades or much of North Africa for some time after the crusade period, must have attracted more settlers. Ironically, one clearly North African immigrant during this postcrusade period was a returning exile. A contemporary government figure describes this incident from King Peter's diplomatic-military expedition to al-Qull on the Tunisian coast. "A Saracen who had belonged to the kingdom of Valencia" and was in fact "a native of the valley of Alfandech" volunteered to spy on the Tunisians among whom he currently resided; as a reward Peter gave him twenty gold pieces plus a grant of land "in your birthplace, among your friends." 41

The most obvious place of origin for outside Muslims, as well as point of transit, was Nasrid Granada. Here the frontier between Christendom and Islam lay startlingly open, despite all laws designed to control it, if one can judge from the easy flow of Muslim population in both directions during the better-documented and more rigidly controlled fourteenth and fifteenth centuries. A partial record of this later two-way movement is the body of petitions by returning Valencians, to the bailiff general, for waivers of prosecution. These routinely granted waivers protected illegal emigrants, upon their return to their native Valencian town, from troublesome local authorities. A recent student of this flux finds that "although nominally a frontier existed," in practice the interchange "canceled out" any political division. Returning migrants, as their numerous waivers spell out, belonged "to all the places of the Valencian kingdom," a geographical span that underlines the universal freedom of movement. In later centuries, as in the thirteenth. Crown and barons welcomed them back.<sup>42</sup>

Muslim immigration did not survive into the next century as a common expedient, owing partly to a hardening of attitude toward the domestic alien. Muslims continued to be valuable sources of revenue, however, and their expanding settlement was not altogether discontinued. The military order of Montesa, successors in Valencia to the suppressed Templars, colonized several districts of Perpunchent with Muslims in 1316, for example, leaving the actual division of homesteads to their revenue officer (amīn). A notable example was Chelva in 1370, where the widow of Jérica's lord delegated forty Muslim entrepreneurs to bring in a hundred of their fellows "to populate" there. Castellón saw an accretion of Muslims, and in 1400 the city fathers took trouble to introduce more such settlement, redoubling their efforts in 1439. At Corbera the bailiff remitted taxes for five years to lure Mudejar settlement in the town quarter and in the countryside. The fourteenth century also witnessed what Miguel Gual Camarena calls a

Boronat y Barrachina, Los moriscos españoles y su expulsión, estudio histórico-crítico (Valencia, 1901), 1: app., doc. 19, pp. 544-45.

<sup>41</sup> Ramón Muntaner, Crónica, ed. Enrique Bagué (Barcelona, 1927-52), ch. 53.
42 Leopoldo Piles Ros, Estudio documental sobre el bayle general de Valencia, su autoridad y jurisdicción (Valencia, 1970), 40-41, with illustrative documents as indicated.

"migratory flow" from seignorial lands onto the more tolerable Crown areas; this provoked protest by the lords and reflexive bettering of conditions on their estates, resulting in a contrary but smaller flow back onto baronial properties.<sup>43</sup>

MUSLIM IMMIGRATION INTO VALENCIA began from the first days of James's conquest, rapidly attained proportions making protest futile, and then continued stronger than ever under Peter. Rome named as culprits the barons, the religious, and even the king; yet not a document has survived from those early settlements that so stirred bishops and popes. It is clear that the later documents which survive cannot be studied in isolation but must be viewed as fragments of a larger mosaic whose outlines alone are visible. Of itself this conclusion would leave a fascinating footnote to crusade history, a bizarre if hardly central feature of medieval Arago-Catalonia, and a useful insight into the evolution of Valencian Mudejarism.

The movement can serve, however, as something much more valuable for the historian—a key to deeper understanding of the crusader's mind as well as of the nature of the conquered kingdom's society. Valencia had not been only a territory to conquer but a complicated, enormously valuable economic prize. To draw from it ready advantage the crusaders had to cherish the native labor force, replacing and augmenting it. Town industries like paper, ceramics, dyes, and shipbuilding demanded skilled workers, but so did the agricultural countryside. Proper exploitation of intensely irrigated areas, islands in a sea of aridity, and of the viticulture prominent in land grants called for experienced hands precisely during the postconquest generation. James and Peter devoted attention to existing irrigation systems, even expanding them. Christian workers were not available in any number, either from the realms of Aragon or from Languedoc and the wider reaches of Christendom. The Crown's active recruitment of Jewish population could not begin to meet the need.<sup>44</sup> Muslims had to do. The zealous crusader, while

43 Libro registro de poblaciones i privilegios, 1316, Archivo Histórico Nacional, Madrid, Ords. milits., Montesa, 542-C, fol. xix: "prout per alaminum nostrum sunt vobis assignate"; carta puebla de Chelva, Aug. 17, 1370, in Fernández y González, Estado social y político de los mudejares de Castilla, app., doc. 71; Vicente Traver Tomás, Antigüedades de Castellón de la Plana, estudios histórico-monográficos de la villa y su vecindario riqueza y monumentos (Castellón de la Plana, 1958), 187-88; en Corbera see Leopoldo Piles Ros, "La situación social de los moros de realengo en la Valencia del siglo xv," Estudios de historia social de España, 1 (1949): 235; Miguel Gual Camarena, "Los mudéjares valencianos en la época del Magnánimo," IV Congrés d'història de la corona d'Aragó (Palma de Mallorca, 1959-70), 1: 471; and see Gual Camarena, "Mudéjares valencianos," 173-74.

44 King James encouraged Jewish immigration; on June 11, 1247, for example, he issued a safeguard to a large family of Jewish settlers and to all Jews traveling to colonize Majorca, Barcelona, and Valencia. Described in Miret y Sans, *Itinerari*, 188. Jews in James's realms were not merely townsmen but commonly agriculturalists at various levels, differing in this from their northern brothers. For a brief overview of King James's colonization policy with the Jews in Valencia, see Yitzhak Baer, *A History of the Jews in Christian Spain*, tr. Louis Schoffman et al. (Philadelphia, 1966), 1: 139-41.

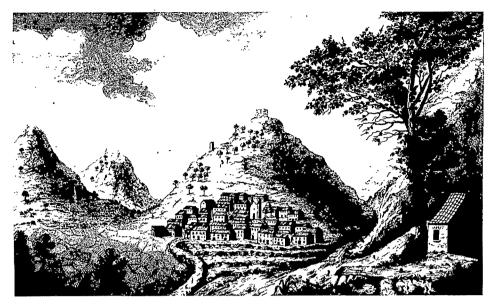


Fig. 4. A view of Eslida in the north-central portion of Valencia, inland and southwest of Castellón. This was a strong Mudejar center in the thirteenth century. Although eighteenth century, the drawing conveys the shape and nature of the Valencia farmer's landscape before radical changes altered it from any semblance of its medieval original. Note particularly the pattern of the fields. The drawing is by Antonio Josef Cavanilles (1745–1804), a noted botanist, and engraved by T. Enguidanos. From Cavanilles, Observaciones sobre la historia natural, geografía, agricultura, población y frutos del reyno de Valencia (Madrid, 1795–97), 2: 109.

elaborating his propaganda about expelling Mudejars and depaganizing the Islamic atmosphere, faced up to a quite different reality. Medieval men, like modern, contrived to live with contradiction.

Why did the Crown not meet the problem by increased importation of slaves? Since Christian and free Muslim farmers lived at a level of parity, a slave system had the advantage of subordinating the Moorish majority and reducing them in a way to nonpersons, thus contributing to the "Christianizing" or colonization of the realm. Joaquín Miret y Sans, a close student of slavery in the medieval realms of Aragon, has emphasized how the acquisition of considerable territory, in addition to the remarkable expansion of commerce and industry consequent upon the conquests of Majorca and Valencia, made some employment of slaves "little less than indispensable." This was in keeping with the times; slavery, handmaid of civilization, had already established itself in the position it was to maintain down into the eighteenth century. In mid-thirteenth-century Valencia, however, the floodgates of inexpensive slavery had just closed. Valencia continued to be an export center for the Mediterranean slave trade, and rebellions or raids occasionally provided free slaves, but the very success of the crusades by Arago-Catalonia and Castile meant an end to the stream of prisoners of war. With this main source of what Miret y Sans calls "the forced immigration of labor" shut off, peace precipitated in expansive Valencia a kind of energy crisis. Though seignorial documents are scarce, a careful review of Templar and Hospitaller documents for the Arago-Catalan realms at this time shows a decided decline in acquisition of slaves and in cash reserves available for their purchase. The triumph of the *reconquista* spelled the landlord's misfortune.<sup>45</sup>

The Valencian situation was not strictly a matter of economics overriding ideals. The foundations for Christian policy here were quite different, particularly the inherited Mediterranean tradition of enclave societies for conquered minorities as developed in Mudejarism. The profit motive had always buttressed this otherwise reasonable policy. In the dynamics of Valencia's peculiar maximum Mudejarism, however, the profit factor came to overshadow other elements. The gentle paradox inherent in the older expressions of Mudejar policy tended to become in this grosser form an intolerable contradiction. The crusader, no longer able to pose as a traditionalist enjoying his reasonable rewards, now seemed a greedy betrayer, perpetuating the overwhelmingly Islamic ambience for mere love of money. Pope Clement IV, in a philippic against this Valencian form of Mudejarism in 1266, realized that the strongest argument he must counter was: "on account of financial necessity." Against it Clement marshaled the good of the state, the purity of religion, and the preservation of King James's human fame. 46

James himself recognized the nature of the dilemma. When he finally determined to expel some portion of Valencia's Mudejars after the rebellion of al-Azraq, the prelates and cities yielded to persuasion, but the barons fought expulsion as economic catastrophe. James tells the story in a passage of his memoirs. Having appealed to the better nature of the barons, to their personal loyalty, and to their religious feelings, he anticipated acquiescence; so he tells us anyway, though James had an instinct for placing himself to the right of the angels and easily expressed consternation over an opponent's failings. To his dismay, "those who had Saracen vassals spoke with reluctance, for what I proposed did not seem to please them." James expressed sympathy with the barons, "because your revenues will be lessened, and not be so great by Christians as by Saracens." The barons' crisis was in fact more serious, as he well knew, with Christian replacements unavailable. Under pressure from Rome, and with his crusader image in Christendom in peril of tarnishing, King James may well have felt secret relief over the intran-

<sup>45</sup> Joaquín Miret y Sans, "La esclavitud en Cataluña en los últimos tiempos de la edad media," Revue hispanique, 41 (1917): 4; Dufourcq, L'Espagne catalane et le Maghrib, 71-76, 466-68. Charles Verlinden's monumental L'esclavage dans l'Europe médiévale, whose only volume to date covers Péninsule ibérique, France (Bruges, 1955), is invaluable for background but can be misleading for the general situation in Valencia. Vicenta Cortes, La esclavitud durante el reinado de los reyes católicos (1479-1516) (Valencia, 1964), has a bibliography for thirteenth-century antecedents; Vincke, "Königtum und Sklaverei," covers fourteenth-century Arago-Catalonia.

<sup>40</sup> This is not a register but the codex "Magistri Berardi [sic] de Neapoli subd. et not. ap. collectio epistolarum summorum pontificum," 1266, Arch. Vat., Reg. Vat. 29A (Clement IV, epistolae), fols. 10–11v, letter 18.

sigence of his barons. Pleading that intransigence, he saved his public image, his conscience, and his cash. Conscience revived to haunt him on his deathbed. In a codicil to his last testament King James exhorted his son and successor not to keep the Mudejar populations in Valencia, "neither ours nor theirs, for the sake of money or rent or income."<sup>47</sup>

From the Mudejars' point of view the Christian dilemma was a boon. It opened up to many Muslims opportunities for relative affluence, conferred exemptions and privileges, extended or maintained the Islamic community, formed a bond of self-interest with the conquerors, and helped balance the trend toward second-class citizenship. These blessings grew, of course, out of the misfortune of fellow Muslims. They were predicated on a substantial drift of freer spirits into permanent exile and in part on failed rebellion or on traitorous support of the crusader cause. Though the settlement projects were in no way exploitative, they strengthened the infidel regime and tightened its inhibiting frame around the Muslim community. In cases like Biar they diluted Islamic military potential at strategic points and redistributed its Muslims to the advantage of the conquerors. They probably further distorted the balance of rural over city Moors, already inaugurated by the expulsions at Valencia and Burriana and by the tendency of Christian immigrants to cluster in the cities. They removed some Moors from heavily Islamic and therefore cohesive environments into a demographic ecology that rendered these Muslims visibly a minority.

All this can hardly have been without acculturative effect—in inducing regressive traditionalism, in bonding the Mudejars more firmly into the aliens' system, in mingling the two peoples, and in introducing a more decisive element of mobility, including upward social mobility. Since Valencian Islam had been so varied a society—comprising areas of diverse ethnic origins, differing work models, idiosyncratic local political evolutions, and opposed group psychologies stretching to antipathy—the shifting of Mudejar populations even within the kingdom's framework certainly caused repercussions in the subject people as a whole.<sup>48</sup> There is food here for investigation by sociologist, anthropologist, demographer, Islamologist, and historian.

<sup>47</sup> King James I, Crònica (Llibre dels feyts), ed. J. M. de Casacuberta (Barcelona, 1926-62), chs. 366-67; King James I, will of 1276, in Charles de Tourtoulon, Don Jaime I el Conquistador, rey de Aragón, conde de Barcelona, senor de Montpeller, según las crónicas y documentos inéditos, rev. and tr. Teodoro Llorente y Olivares (Valencia, 1874), app., vol. 2, p. 458.

<sup>48</sup> On social classes and the human geography, see Burns, Islam under the Crusaders, chs. 4, 5. Some hint of the complex immigration antecedents of Islamic Valencia throughout its long previous history can be gained from representative short studies. See, for example, the innovative approach of Pierre Guichard, "Le peuplement de la région de Valence aux deux premiers siècles de la domination musulmane," Mélanges de la casa de Veldzquez, 5 (1969): 103-58; on a movement of mountain people on the eve of Valencia's reconquista, see Julian Ribera y Tarragó, "Musulmanes de Valencia originarios de Albarracín," in his Opúsculos dispersos (Tetuán, 1952), 31-33; see also Jacinto Bosch Vilá, "Establecimiento de grupos humanos norteafricanos en la península ibérica, a raíz de la invasión musulmana," Atti del I congresso internazionale di studi nord-africani (Cagliari, 1965), 147-61; and J. T. Monroe, introd. to his translation, The Shu°ūbiyya in al-Andalus: The Risāla of Ibn García and Five Refutations (Berkeley, 1970), 1-21.

The topic still lies in penumbra, awaiting deeper interpretation or even moralizing, and presenting at once the light and dark sides of the destiny awaiting the free Muslim community in crusader Spain.<sup>49</sup>

<sup>49</sup> It may also serve as remote background to the current problem posed by the considerable illegal immigration of Muslims into Spain. *La Vanguardia española* for June 7, 1972, under the heading "La emigración árabe a España: Un problema desatendido," reported "unos 30,000 norteafricanos" living clandestinely in the city and county of Barcelona alone. Mostly from Algeria and Morocco, they tend to enter Algeciras and Málaga, surviving by heavy construction labor and living in the poorest parts of older Barcelona.

## Feminism in the French Revolution

## JANE ABRAY

French feminism has a long history; its roots go back far beyond the tumult of new ideas that mark the Revolution. Since the Renaissance, indeed since the Middle Ages, French women—and men—had argued for equality of legal and political rights for the sexes. Woman's education, her economic position, and her relationship to her father and husband had all been worked over time after time.¹ In the eighteenth century intellectuals carried on a desultory debate over the status of women. The discussion slowly grew more heated until, in the early years of the Revolution, a small group of bold thinkers demanded changes that, if effected, would have altered the character of French civilization far more than did the abolition of the monarchy.

Single or married, women had few rights in the law during the last decades of the ancien régime. Their testimony could be accepted in criminal and civil courts but not for notarized acts like wills. In some parts of France a single woman could enter into contractual relationships, but for the most part her rights—reasonably extensive as late as the thirteenth century—had atrophied.<sup>2</sup> Generally speaking a single woman remained under her father's authority until she married; marriage transferred her to her husband's rule.<sup>3</sup> Once married she generally had no control over her person or her property.

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<sup>2</sup> Marcel Garaud, La Révolution et l'égalité civile (Paris, 1953), 172-74. On women's political participation and legal rights in the Middle Ages, see Maurice Bardeche, Histoire des femmes (Paris, 1968), 2: 60-61, 72-73.

<sup>3</sup> Philippe Sagnac, La législation civile de la Révolution française (Paris, 1893), 295. For an explanation of the father's power, see Jacques Godechot, Les institutions de la France sous la Révolution et l'Empire (Paris, 1951), 206-07.

¹ See Léon Abensour, Histoire générale du féminisme des origines à nos jours (Paris, 1921); Lulu McDowell Richardson, The Forerunners of Feminism in the French Literature of the Renaissance from Christine of Pisa to Marie de Gournay (Baltimore, 1929); and Georges Ascoli, "Essai sur l'histoire des idées féministes en France du XVI° siècle à la Révolution," Revue de synthèse historique, 13 (1906): 25-57, 161-84. The term "féminisme" itself did not come into use in French until the nineteenth century. Charles Fourier used it first in the second edition of his Théorie des Quatre mouvements (Paris, 1841); see Charles Turgéon, Le féminisme français (Paris, 1907), 1: 10.

Only the death of her husband could offer her some prospect of independence. As Robert Joseph Pothier, an eighteenth-century legal expert, explained, "Our customary law has put women into such a condition of dependence on their husbands that they can do nothing valid, nothing that the civil law will recognize, unless they have been specifically authorized by their husbands to do it."4 Nor was the economic position of eighteenthcentury women enviable. Although their earnings were vital to the survival of lower-class families, their wages were very low. The gild offices excluded women, and even the slight modernization of industry accomplished before the Revolution tended to worsen their condition. For the most part law and custom confined women to domestic service, heavy labor, and ill-paid labor-intensive industries like the lace trade.<sup>5</sup> Surprisingly enough, French women did have some political rights. The regency was open to women. According to the king's summons of the Estates-General women in religious orders and some noblewomen could send representatives to the Estates. A few women of the Third Estate, particularly widows, managed to participate in some of the primary assemblies.6

This was the subordinate position that the eighteenth-century intellectuals debated. The great figures of the Enlightenment-Montesquieu, Voltaire, Rousseau, Condorcet, Diderot, and other Encyclopédistes-contributed to the discussion but were not its main speakers.7 From the middle of the century on a host of now-obscure writers took up the feminist case: Abbé Joseph-Antoine Toussaint Dinouart, Philippe Florent de Puisieux, Mlle Archambault, Pierre Joseph Caffieux, Pierre Joseph Boudier de Villemert, Mme Riccobini, [Antoine-Léonard?] Thomas, and Mme de Coicy.8 As ad-

4 Robert Joseph Pothier, Traité de la puissance du mari (1769), in his Oeuvres complètes

<sup>(</sup>Paris, 1821), 10: 655.

5 Owen Hufton, "Women in Revolution, 1789-1796," Past and Present, 53 (1971): 91-92; Evelyne Sullerot, Histoire et sociologie du travail féminin (Paris, 1968), 69-74. In the weaving trade women were explicitly refused the right to become masters because if they were allowed to improve their status "la rarété des tireuses occasionnerait une augmentation ruineuse de la main d'oeuvre." Ibid., 73.

<sup>6 &</sup>quot;Règlement fait par le roi pour l'exécution des lettres de convocation du 24 janvier 1789," in Introduction historique à l'Ancien Moniteur (Paris, 1843), 559-60. On earlier meetings of the Estates, see Louis Franck, Essai sur la condition politique de la femme (Paris, 1892), 304-09. On women in the Assemblées Primaires, see the remarks of Paul-Marie Duhet about Chevanceaux, in Les femmes et la Révolution, 1789-1794 (Paris, 1971), 25.
7 On the philosophe's position, see David Williams, "The Politics of Feminism in the French

Enlightenment," in Peter Hughes and David Williams, eds., The Varied Pattern: Studies in the Eighteenth Century (Toronto, 1971), pp. 333-51; and Edwin R. Hedman, "Early French Feminism from the Eighteenth Century to 1848" (Ph.D. dissertation, New York University, 1954). Condorcet alone held strong feminist convictions. Rousseau held equally clear objections. Montesquieu and the others show some sympathy, but none of them could be called feminists.

<sup>8</sup> Abbé Joseph-Antoine Toissaint Dinouart, Le Triomphe du Sexe (Amsterdam, 1749); Philippe Florent de Puisieux, La femme n'est pas inférieure a l'homme (London, 1750) and Le Triomphe des dames (London, 1751), both supposedly translations of English works; Mlle Archambault, Dissertation sur la question: Lequel de l'homme ou de la femme est plus capable de constance? Ou la Gause des dames (Paris, 1750); Pierre Joseph Caffieux, Défense du beau sexe (Paris, 1753); Pierre Joseph Boudier de Villemert, L'Ami des femmes (n.p., 1758), with at least four more editions before the Revolution; Mme Riccobini, L'Abeille (1765); [Antoine-Léonard?] Thomas, Essai sur les femmes (Paris, 1772), with three more editions, 1772-73; Mme de Coicy, Les femmes comme il convient de les voir (London and Paris, 1785).

vocates of social revolution this group must be accounted tame. Boudier de Villemert maintained that women ought to have "a serious daily occupation" and recommended embroidery.9 Potential feminists could have found sterner stuff in the Journal des Dames, a monthly magazine. Its editor in 1774, Mme de Montenclos, was an advocate of women's rights. She staunchly proclaimed, "I am not out to draw attention to myself, but I swear I do want to shatter our conventions and guarantee women the justice that men refuse to them as if on a whim."10 Many of the opponents of these ambitions lurked in the vast literature on women's education. Restif de la Bretonne, following the path of Rousseau's Emile, ordered that all thought of equality between the sexes be suppressed. Women should be forbidden to learn reading and writing in order to limit them to useful domestic labor.11 Mme de Genlis urged that women's education be organized to prepare them "for a monotonous and dependent life." 12 While the supporters of feminism tended to exalt marriage and motherhood as a claim on society, the antifeminists used this same "natural vocation" to prove that women should be content to stay home and to obey their husbands.

BY 1789 CONVENTIONAL WISDOMS of all sorts, and even the image of the happy homemaker, had begun to quiver. For in the last years of the decade a more militant feminist theory had emerged in a spate of pamphlets. No longer content to make vague statements advocating equality, the partisans of women's emancipation got down to specific proposals about education, economics, and legal and political rights. Their brochures began to appear in 1787 and quickly multiplied. The general argument ran: human beings are naturally equal, therefore sexual discrimination is unnatural; husband and wife should be equal partners in marriage; women ought to have a better education and access to more, and higher-paid, jobs. Along with demands for marital and economic equality the new feminism laid claim to the vote.

The Marquis de Condorcet sounded the first blast of this trumpet in favor of the regiment of women. He reasoned that women, since they were not allowed to vote, were being taxed without representation and would be justified in refusing to pay their taxes. Moreover, said Condorcet, domestic authority should be shared and all positions and professions opened to both sexes. He observed that sexual inequality was a new state and not the traditional lot of women. A year later Condorcet insisted that women who met the property qualifications he proposed for the suffrage should vote. He also

<sup>9</sup> Boudier de Villemert, L'Ami des femmes, 51-54.

<sup>10</sup> Quoted in Evelyne Sullerot, *Histoire de la presse féminine des origines à 1848* (Paris, 1966), 23, italics in original.

<sup>11</sup> Restif de la Bretonne, Les Gynographes (Paris, 1777), 92, 180.

<sup>12</sup> Mme de Genlis, Adèle et Théodore ou Lettres sur l'éducation (1782), in her Oeuvres complètes (Maestricht, 1782), 10: 30.

predicted that his ideas would get little support from women, as they were all too enamored of Jean-Jacques Rousseau to listen to him.<sup>13</sup>

Most women ignored the feminists. Yet Condorcet found some allies. Two pamphlets, Requête des femmes pour leur admission aux Etats-Generaux and Remontrances, plaintes et doléances des Dames Françaises, called for political rights; the latter also criticized men for stultifying women's minds through a too-narrow education.<sup>14</sup> Not all of these pamphlets were concerned primarily with political rights. "We ask for enlightenment and jobs," said the women of the Third Estate to the king, "not to usurp men's authority, but to rise in their esteem and to have the means of living safe from misfortune." <sup>15</sup>

One of the most important of these early pamphlets was Cahier des doléances et réclamations des femmes, par Mme B... B.... The anonymous author began by revealing her astonishment that women were not seizing the opportunity to make themselves heard. She described her own conversion to feminism—she had thought women weak and incompetent but now knew better—and asked whether men could continue to make women the victims of their pride and injustice at a time when the common people were entering into their political rights and when even the blacks were to be free. She insisted that just as a noble could not represent a roturier in the Assembly, so a man could not represent a woman. Mme B... B... then lashed out at the double standard of sexual morality, at the droits d'ainesse and at those of masculinité. This pamphlet reappeared word for word as Cahier des doléances et réclamations des Femmes du département de la Charente. 17

Other pamphlets appeared along with a flurry of satires mocking the feminists' pretensions.<sup>18</sup> Condorcet contributed another major statement in which he repeated his earlier arguments on behalf of women's suffrage and

<sup>13</sup> Marquis de Condorcet, "Lettres d'un bourgeois de New Haven à un citoyen de Virginie" (1787), in his Oeuvres, ed. Frank O'Connor and M. F. Arago (Paris, 1847), 9: 15–19; Condorcet, "Essai sur la constitution et les fonctions des assemblées provinciales" (1788), in *ibid.*, 8: 141–42. Rousseau's popularity with women is an indication of how few of them held any feminist convictions.

<sup>14</sup> See Charles-Louis Chassin, Le Génie de la Révolution (Paris, 1863), 1: 477.

<sup>15</sup> Pétition des femmes du Tiers au roi, ler janvier 1789, extracts in Jeanne Bouvier, Les femmes pendant la Révolution (Paris, 1931), 249-50. Duhet compares this petition to Cahier des doléances et réclamations des femmes, par Mme B... B...." Les femmes et la Révolution, 32-90.

<sup>16</sup> Bouvier, Les femmes pendant la Révolution, 266-74.

<sup>17</sup> Duhet, Les femmes et la Révolution, 41. The pamphlet was reprinted under the second title in the Etrennes nationales des Dames. See Sullerot, Histoire de la presse féminine, 47-50.

<sup>18</sup> Les très humbles remontrances des femmes françaises (1788); De l'influence des femmes dans l'ordre civil et politique (1789); Sophie Rémi Courtenai de la Fosse Ronde, Argument des femmes aux Etats-Generaux (1789); Réponse des Femmes de Paris à l'Ordre le plus nombreux de France (1789); Discours préliminaire de la Pauvre Javotte (1790). I have found no trace of the contents of the first four. The Discours préliminaire calls for the abolition of the clergy and the nobility as orders and the constitution of women as the new second order. It castigates the Revolution for neglecting poor women to concentrate on the "aristocratie masculine." Bouvier, Les femmes pendant la Révolution, 297-303. The identification of masculinity with aristocracy was a favorite device since it permitted feminists to co-opt all the ideas and stock phrases of the Third Estate's campaign. Duhet discusses some of the satires. Les femmes et la Révolution, 30-32.

ridiculed one of the opposition's favorite arguments. "Why should people prone to pregnancy and passing indispositions be barred from the exercise of rights no one would dream of denying those who have gout or catch cold easily?" <sup>19</sup>

A very few feminist proposals appeared in the cahiers. The most common of these was the appeal for improvements in the education of women.<sup>20</sup> The Third Estate of Châtellerault (Poitou) made a unique suggestion. "Let the assemblies be constituted according to an equitable procedure; accordingly let citizens of both sexes and of all ages have equal rights in participating in the debates of the assemblies and in the appointing of deputies." Far more ordinary was their suggestion that paid midwives be provided for the countryside.<sup>21</sup> The drawing up of the cahiers also prompted comments from interested observers. One set of anonymous Observations sur la rédaction des Cahiers de Paris urged that men be forbidden to exercise "women's professions," thus assuring women the means of making their living and consequently keeping them from turning to prostitution. Henri Jabineau, a lawyer and abbé, sent thirty-two articles echoing these themes first to a Parisian electoral assembly and then to the Estates-General.<sup>22</sup>

Once the Estates had met and representative government had begun, the feminists changed their tactics. No longer did they rely on pamphlets and letters to the editor. Instead they took to sending delegations to the government and to using the political clubs as platforms. Representations to the National Assembly began very early. In November 1789 the Assembly received a series of "Motions en faveur du sexe" that attacked the economic subordination of women and the evils of convent life. This habit of addressing proposals directly to the government persisted at least until 1793. Mme Mouret went to the Assembly in 1790 to present a speech on the need for women's education. Early in 1792 several Parisians of both sexes

19 Condorcet, "Sur l'admission des femmes au droit de la cité," published in the Cercle Social's Journal de la société, 1790, in Condorcet, Oeuvres, 10: 119–30.

20 Elizabeth Racz says that thirty-three of the cahiers recommended educational opportunities for women. "The Women's Rights Movement in the French Revolution," Science and Society, 5, 16 (1051-52): 153.

21 Archives Parlementaires (hereafter AP) (Paris, 1867–1972), 1789, vol. 2, pp. 691, 696. The government considered the need for midwives. See the meetings of July 31, Aug. 6, and Sept. 4, 1790, in Procès-verbaux et rapports du comité de mendicité de la Constituante, 1790–91, ed. Camille Bloch and Alexandre Tuetey (Paris, 1911), 104, 108, 168 n.1.

<sup>22</sup> Charles-Louis Chassin, Les élections et les cahiers de Paris en 1789 (Paris, 1888), 3: 168, 384–86. The interconnection of prostitution and women's economic condition is acknowledged in some of the cahiers. Racz, "Women's Rights Movement," 152. See also Richard Cobb, The Police and the People (London, 1970), 234–39, and his Reactions to the French Revolution (London, 1972), 134. I have not found much evidence of a distinctively feminist attitude to prostitution. Attacks on its evils were common throughout the Revolution; almost no one who complained about it attached any responsibility to the customer.

23 "Motions adressées a l'Assemblée nationale en faveur du sexe," Moniteur, Nov. 29, 1789, reprinted in Réimpression de l'Ancien Moniteur (Paris, 1843-63), 2: 262-63 (hereafter cited simply as Moniteur, with volume and page numbers of the reprint). See also Duhet, Les femmes et la Révolution, 54-56.

<sup>24</sup> Moniteur, Mar. 26, 1790, vol. 3, p. 703. Mme Mouret's speech, published as Annales de l'éducation du sexe ou Journal des demoiselles (1790), is excerpted in Sullerot, Histoire de la presse féminine, 52-54.

requested the Assembly to pass a law against despotic paternal and marital power.<sup>25</sup> In April of that year Etta Palm van Aelders, a Dutch feminist, petitioned the Assembly to provide education for girls, to guarantee women's legal majority at twenty-one, to give both sexes political freedom and equal rights, and to present divorce legislation.<sup>26</sup> The following summer a woman from the Beaurepaire section addressed the Convention.

Citizen legislators, you have given men a Constitution; now they enjoy all the rights of free beings, but women are very far from sharing these glories. Women count for nothing in the political system. We ask for primary assemblies and, as the Constitution is based on the Rights of Man, we now demand the full exercise of these rights for ourselves.

The president congratulated her deputation for its zeal—and postponed discussion.<sup>27</sup>

Such a discussion might have proved to be quite excited. The assemblies had their full complement of antifeminists, but they also contained a few advocates of women's emancipation. In 1792 Aubert-Dubayet of Isère spoke on the recording of vital statistics; he called women "the victims of their fathers' despotism and of their husbands' perfidy" and warned that French law must not maintain women in a state of slavery.<sup>28</sup> In the spring of 1793 Pierre Guyomar, from the Côtes-du-Nord, presented the Convention with his reflections on political equality. To him the only differences between men and women lay in their reproductive systems, and he could not understand why such physical differences should lead to differences before the law. Like many other contemporary feminists, Guyomar compared sexual to racial discrimination. He spoke, too, of "une aristocratie formelle des hommes."<sup>29</sup>

Supporters of women's rights did not completely abandon their old platforms. Letters to newspapers continued to appear.<sup>30</sup> The founder of the *Journal des Droits de l'Homme*, a Cordelier named Labenette, defended the rights of women.<sup>31</sup> A major feminist declaration arrived on the streets of Paris in 1791. Olympe de Gouges, having had enough of the "rights of man," announced the rights of women. Her text followed closely that of the declaration of August 1789.

All women are born free and remain equal to men in rights.... The aim of all political associations is the preservation of the natural and inalienable rights of women and men.... Law is

<sup>25</sup> AP, Feb. 13, 1792, vol. 38, p. 466. Nothing came of the request at this time.

<sup>&</sup>lt;sup>26</sup> Ibid., Apr. 1, 1792, vol. 41, pp. 63-64. Louis Prudhomme ridiculed the petition in "Encore une pétition de femmes," in his *Révolutions de Paris*, Mar. 31-Apr. 7, 1792, no. 143, pp. 20-24. <sup>27</sup> AP, July 4, 1793, vol. 68, p. 254.

<sup>28</sup> *Ibid.*, Aug: 30, 1792, vol. 49, p. 117.

<sup>29</sup> Ibid., Apr. 29, 1793, vol. 63, pp. 591-99.

<sup>30</sup> Note the correspondence in the Courrier de l'hymen, Feb. 1791, given in Bouvier, Les femmes pendant la Révolution, 110.

<sup>31</sup> Sullerot, Histoire de la presse féminine, 61. Duhet adds that, pressured by other men, he ceased his propaganda. Les femmes et la Révolution, 213-15.

the expression of the general will: all female and male citizens have the right to participate personally, or through their representatives, in its formation.

De Gouges also demanded equality of opportunity in public employment, the right to paternity suits, and an end to male tyranny generally.<sup>32</sup> The following year Mary Wollstonecraft's Vindication of the Rights of Women, inspired in part by the Revolution, appeared in a French translation and created some stir.33

Women also made their presence felt in the great revolutionary journées and in the army.<sup>34</sup> While this activity was not, strictly speaking, feminist, any activity by women in a society that places a premium on female passivity has some feminist overtones. Nor were the implications of their actions lost on the women themselves. In 1789 the women of the Halles were singing:

> A Versail' comme des fanfarons, l'avions amené nos canons: [bis] Falloit voir, quoi qu' j'étions qu'des femmes Un courage qui n'faut pas qu'l'on blâme. Nous faisions voir aux homm' de coeur Que tout comme eux j'n'avions pas peur: [bis] Fusil, musquetons sur l'épaule, J'allions comme Amadis de Gaule.35

The Etrennes nationales des Dames, a feminist newspaper begun in November 1789, used the same episode to threaten "aristocratic husbands" that women could just as easily take up arms against them if they persisted in their pretensions.<sup>36</sup> In January 1794, when the back of feminism had broken under the weight of public and governmental hostility, some women still remembered their old enthusiasms. A police spy reported on groups of women eager to see Reine Chapuy, a female cavalry soldier. The idea of her daring aroused several of these women to attack male cowardice and to exalt female courage.37

Most of the people behind this agitation have left little trace. Some of their clubs can be pinned down; a few of the most flamboyant leaders survive as individuals. Of the Paris political clubs the Cercle Social was the first to advocate feminism. Its members began to hear radical ideas about women's place in society in October 1790; both Condorcet and Etta Palm

<sup>32</sup> Olympe de Gouges, Droits de la femme, Sept. 1790, complete text in Bouvier, Les femmes pendant la Révolution, 283-89.

<sup>33</sup> Sullerot, Histoire de la presse féminine, 45.

<sup>34</sup> On women in the journées, see George Rudé, The Crowd in the French Revolution (Oxford, 1967). For women soldiers, see Raoul Brice, La femme et les armées de la Révolution et de l'Empire (1792-1815) (Paris, n.d.). Léon Schwab has reprinted a satire against the women soldiers in "Les femmes aux Armées," La Révolution dans les Vosges, 6 (1912-13): 109-15. The Convention tried to remove the women soldiers, but some of them managed to evade its decree. AP, Apr. 30, 1793, vol. 63, pp. 628-29.

35 Quoted in Cornwell B. Rogers, The Spirit of Revolution in 1789 (Princeton, 1949), 182.

<sup>36</sup> Sullerot, Histoire de la presse féminine, 48-49.

<sup>37</sup> Report of Charmont, Jan. 20, 1794, in Pierre Caron, ed., Paris Pendant la Terreur: Rapports des agents secrets du ministre de l'intérieur (Paris, 1910-64), 3: 56.

used it as a forum.<sup>38</sup> Several of the Parisian sociétés populaires accepted women: the Club des Indigents, Club des Halles, Club des Nomophiles, Club des Minimes, the Société Fraternelle des Jacobins, and that of the Carmes.<sup>39</sup> Although the Société Fraternelle des Jacobins, for one, had female officers, there is no evidence that any of these clubs were directly involved in feminist activities. The same holds true of the provincial women's clubs in Besançon, Bordeaux, Dijon, Orléans, Strasbourg, and elsewhere.<sup>40</sup> The provincial women's clubs attracted middle-class women, but in Paris the rank and file were usually from the lower classes. On the other hand the male feminists of whom we have record were generally fairly substantial citizens.

The feminist leaders about whom enough is known to permit biographical sketches were rather a curious crew.<sup>41</sup> Olympe de Gouges—born Marie Gouze in 1748—was a failed playwright whose royalism and opposition to Robespierre combined to bring her to the guillotine. She relied on brochures, posters, long letters to newspapers, and very unpopular plays to spread her message. She had little enough influence. As a police spy described the reaction to one of her placards, "People stop a minute, then walk off saying, 'Oh, it's just Olympe de Gouges!' "<sup>42</sup> There can be no doubt about the ardent feminism of the author of the *Droits de la Femme*. Although her execution in 1793 had obvious political causes, it was not without its significance as a gesture of repression toward the feminists. The semiofficial *Feuille du salut public* gloated, "It seems the law has punished this conspirator for having forgotten the virtues that suit her sex."<sup>43</sup>

Etta Palm, who like de Gouges glamorized her name by declaring herself the "Baronne" d'Aelders, came to Paris from Holland in 1774. Palm urged the Constituent Assembly to form a company of amazons as "a first blow to the prejudices that have been wrapped around our lives," and she advocated "a second revolution in our customs" to overthrow sexual

<sup>38</sup> Alphonse Aulard, "Le féminisme pendant la Révolution française," Revue bleue, 4th ser., g (1898): 362-66.

<sup>39</sup> Isabelle Bourdin, Les Sociétés populaires à Paris pendant la Révolution (Paris, 1937), 139.

40 There were many more women's clubs, some of which have been described in Marc de Villiers, Histoire des clubs de femmes et des légions d'amazones (Paris, 1910). On the groups mentioned here, see Claude Brelot, "Besançon révolutionnaire" in Cahiers de l'Association interuniversitaire de l'Est, no. 10, La Révolution à Besançon et dans quelques villes de l'Est de France (Strasbourg, 1966); Henriette Perrin, "Les clubs de femmes de Besançon," Annales révolutionnaires, 9 (1917): 629-53; 10 (1918): 27-63, 505-32, 645-72; Aurelien Vivie, Histoire de la Terreur à Bordeaux (Bordeaux, 1877); Geneviève Langeron, "Le club des femmes de Dijon pendant la Révolution," La Révolution en Côte d'Or, n.s. 5 (1929): 5-71; Camille Bloch, "Les femmes d'Orléans pendant la Révolution," Révolution française, 43 (1902): 49-62; and Les Sociétés politiques de Strasbourg pendant les années 1790 à 1795: Extraits de leur procès-verbaux (Strasbourg, 1865).

<sup>41</sup> Condorcet, whose life is well known, will not be discussed here.

<sup>&</sup>lt;sup>42</sup> Report by Latour-Lamontagne, Sept. 21, 1793, in Caron, Paris pendant la Terreur, 1: 155. <sup>43</sup> Quoted in Léopold Lacour, Trois femmes de la Révolution (Paris, 1900), 2. See also Charles Monselet, "Olympe de Gouges," in his Les oubliés et les dédaignés (Paris, 1859), 139-76; and the account of de Gouges's trial in Émile Campardon, Le Tribunal révolutionnaire de Paris (Paris, 1866), 1: 164-65.

tyranny.<sup>44</sup> She addressed to the general populace an Appel aux Françaises sur la régénération des moeurs et nécessité de l'influence des femmes dans un gouvernement libre. In 1791 she tried to organize a national federation of women's groups. Failure here did not stop her. She went on to address the Assembly, demanding equal employment and education, as well as political and legal equality. Its president replied ambiguously; the legislature would avoid taking any actions that might bring the citizens to regret and tears. Like de Gouges, Palm practiced the wrong politics—she had, for example, invited the Princesse de Bourbon to be a patron to one of her charitable organizations. Unlike de Gouges she had the good sense to leave France before the government could arrest her.<sup>45</sup>

Théroigne de Méricourt, whose real name seems to have been Anne Terwagne, is perhaps the best known of these three, largely because of the attacks her contemporaries made on her. She created a sensation in the early years of the Revolution, holding a salon, trying to form a women's club, participating in the attacks on the Tuilleries, and striding about in riding clothes. Her feminism was something of a sideline, albeit sincere. In an autobiographical account she declared herself to be "humiliated by the servitude and the prejudices in which male vanity keeps our oppressed sex." She encouraged women to form a militia company because, she said, "it is time for women to break out of the shameful incompetence in which men's ignorance, pride, and injustice have so long held us captive." Her attempts to found a women's club provoked Antoine-Joseph Santerre, the commander of the National Guard, to observe that the men of his section would rather find their homes in order when they came back after a hard, day's work than be greeted by wives fresh from meetings where they did not always gain in sweetness. Like de Gouges and Palm, Théroigne was politically moderate, a friend to the Girondin deputies. The shock from a beating she received from a group of Jacobin women in the spring of 1793 seems to have turned her mind. After spending some time in an asylum she was released, only to be permanently recommitted in 1797.46

Two other women deserve mention, the chocolate maker Pauline Léon and the actress Claire Lacombe, founders and presidents of the most famous of the women's clubs, the Citoyennes Républicaines Révolutionnaires.

44 Quoted in Bouche de fer, Jan. 3, 1791, quoted in P. J. B. Buchez and P. C. Roux, Histoire parlementaire de la Révolution française (Paris, 1834–38), 8: 424–27.

<sup>&</sup>lt;sup>45</sup> There is no biographical study of Etta Palm. Episodes in her life are described in Aulard, "Le féminisme pendant la Révolution française," 364-65; in Villiers, Histoire des clubs de femmes; and in Bourdin, Les Sociétés populaires, 144-48, 151, 160, 289. See also Marie Cerati, Le club des citoyennes républicaines révolutionnaires (Paris, 1966), 19-21.

<sup>46</sup> Her life is given in Lacour, Trois femmes. Her own account of it is appended to Emma Adler, Die Berühmten Frauen der französischen Revolution (Vienna, 1906), 244-78. Her speech on the women's militia is quoted in Cerati, Le club des citoyennes, 18; that of Santerre is in Bourdin, Les Sociétés populaires, 153. One of Théroigne's doctors, Jean Esquirol, described her last years in Les maladies mentales (Brussels, 1838). The Actes des Apôtres (1789-91) is a fertile source of attacks on her; see in particular version 2, no. 38; version 4, no. 110; and version 6, no. 169. See also Marcellin Perlet, Les Actes des Apôtres, 1789-91 (Paris, 1873), 145-56.

Founded in the spring of 1793, the club contributed to the fall of the Girondins, then drifted away from the Jacobins toward the enragés, a move that had much to do with its eventual suppression. The Républicaines were sans-culottes women, and their program emphasized economic claims, notably cheap food, rather than strictly feminist demands. Nevertheless the Républicaines showed some sympathy for women's emancipation. Only two accounts of their meetings survive, and one shows the Républicaines discussing women's capacity to govern. At the first of these the citoyenne Monic concluded that women were certainly worthy to rule nations, perhaps even more so than were men.<sup>47</sup> In June 1793 the Républicaines tried to put their ideas into practice by attempting to gain entry to the Conseil Général Révolutionnaire, newly set up in Paris.<sup>48</sup> The women of the Droits de l'Homme section had high praises for their activities.

You have broken one of the links in the chain of prejudice: that one, which confined women to the narrow sphere of their households, making one half of the people into passive and isolated beings, no longer exists for you. You want to take your place in the social order; apathy offends and humiliates you.<sup>49</sup>

The feminist program for educational, economic, political, and legal change developed piecemeal. To justify their goals the feminists used three major arguments. First, women were human beings who therefore shared in the natural rights of man, a conviction often explicitly expressed but also implicit in the borrowing of political terms like "aristocracy" and "despotism" to describe the old system. Feminists saw the women's struggle as parallel to and a continuation of the war of the Third Estate against the upper classes. Second, the feminists made use of women's biological role. As the mothers of all citizens women had a special claim on the state, for they guaranteed its survival. Unlike modern feminists, they made no attempt to define women as other than mothers and potential mothers. Third, once the Revolution was under way, feminists cited women's political contributions to the struggle for liberty and pointed to their continuing patriotism. Since they were fulfilling the duties of citizens women could not logically be denied the rights of citizens. The feminists felt they had solid grounds for their proposals, but one by one the revolutionary governments reiected them.

Education was the most important feminist rallying point. It was also the subject on which feminists and their opponents had managed some agreement before the Revolution. The conviction that women's education needed improvement had been fairly general before 1789. The revolu-

<sup>47</sup> The account of the first meeting is in P.-J. Proussinale [Pierre-Joseph Alexis Roussel], Le château des Tuilleries (Paris, 1802), quoted in Cerati, Le club des citoyennes, 49-51. For the other account see note 98 below.

<sup>48</sup> Moniteur, May 31, 1793, vol. 16, p. 527.

<sup>42</sup> Quoted in Bouvier, Les femmes pendant la Révolution, 329-31. Scott Lytle, "The Second Sex (1793)," Journal of Modern History, 27 (1955): 14-26, is clearer on the Républicaines' end than is Cerati, Le club des citoyennes, 110-78.

tionary governments considered a multitude of educational projects from which some common principles can be extracted. Most projects followed Talleyrand's lead in declaring that both sexes must be educated and then sharply distinguishing between the kinds of education suitable to each. His "Projet de décret" read in September 1791 sounded a note that would recur again and again. "All the lessons taught in the public schools will aim particularly to train girls for the virtues of domestic life and to teach them the skills useful in raising a family."50 The conventionnel Alexandre Delevre dismissed secondary education for women as unnecessary.<sup>51</sup> Some of the less progressive legislators would have denied even primary education to girls, preferring to see them taught housekeeping at home.<sup>52</sup> The Convention's Committee on Public Education did in fact vote to suppress girls' schools in the summer of 1795 but then changed its mind the following year.53 Since the revolutionary governments never succeeded in running a national educational system, it is difficult to evaluate their work and dangerous simply to assume that they neglected women. Yet it is clear that the government intended to reinforce and to perpetuate sexual differences through public education.<sup>54</sup> It is also clear that the Revolution was unable to improve or even to expand women's education. On the other hand the secularization of education and the promise of an expanded primary school system held out some hope of employment to literate single women.55

This was one of the few hopes the Revolution offered to women who had to earn their own living. The Committee of Public Safety and the Con-

 $<sup>^{50}</sup>$  AP, Sept. 10, 1791, vol. 30, pp. 449, 478–79, 499.  $^{51}$  Ibid., July 3, 1793, vol. 68, p. 193. See also the opinion of Charles Gilbert Romme in the Legislative Assembly, ibid., Dec. 20, 1792, vol. 55, p. 191.

<sup>52</sup> Ibid., July 29, 1793, vol. 69, pp. 670-74.

<sup>53</sup> See the meetings of Aug. 15, 24, and Oct. 25, 1795, in Proces-verbaux du Comité d'instruction publique de la Convention nationale, ed. M. J. Guillaume (Paris, 1891-1907), 6: 546, 580, 873. 54 For further evidence of this, see the following proposals in AP: Dec. 12, 1792, vol. 55, pp. 25–27; July 2, 1793, vol. 68, pp. 113–17; July 3, 1793, vol. 68, pp. 150–52, 179–94, 194–96; July 13, 1793, vol. 68, pp. 661–75; July 29, 1793, vol. 69, pp. 670–74, 674–79. The records of the Assembly's and Convention's education committees are less rewarding. The former promised to produce a decree on women's education, but apparently never got around to it. Meeting of Apr. 21, 1792, in Procès-verbaux du Comité de l'instruction publique de l'Assemblée legislative, ed. M. J. Guillaume (Paris, 1889), 250. The latter demonstrates that women continued to be interested in education and to advise the government on how best to organize it. See the meetings of Oct. 12, Nov. 5, 1793, and June 19, 1794, in Proces-verbaux du Comité d'instruction publique de la Convention nationale, 2: 608, 746; 3: 613. One would expect Condorcet's report on education to favor feminist aspirations. After promising equal primary education, Condorcet dropped the subject, telling the Legislative Assembly that a separate report on this important subject would be presented later. AP, Apr. 21, 1792, vol. 42, p. 236. In the version given in his Oeuvres (7: 215-26) he devotes more attention to it. Here he says that women can only be taught the same truths as men and urges the opening of the sciences to them.

<sup>55</sup> At one point the government even offered equal pay for equal work, but this idea eventually disappeared. Convention decree, Moniteur, June 26, 1793, vol. 17, p. 41. Governmental authorities made pious statements on the equal importance of the work of "instituteur" and "institutrice," but there is evidence that ordinary people saw a large difference. See, for example, the case of Girard Viry, who, to his horror, was hired as an institutrice. Charles Henry Chevalier, "Le citoyen Girard Viry, 'Institutrice,' "La Révolution dans les Vorges, 18 (1930): 19-24. When it cared to do so the government could distinguish men from women. Thus in May 1796 the Council of Five Hundred ruled that "the interests of society and morality" excluded women from senior teaching positions. Moniteur, May 11, 1796, vol. 28, p. 270.

vention's education committee both flirted with the idea of training women to set type, but nothing came of it.<sup>56</sup> Nor did the revolutionary governments make any effort to help the women injured by the collapse of luxury trades like silk and lace.<sup>57</sup> The government established ateliers nationaux for men quite early in the Revolution, but it was reluctant to help women. Jean-Sylvain Bailly, the mayor of Paris, requested aid for them in January 1790, but it was almost two years before anything was done, and then action came from municipal, not national, authorities. Where women were admitted to the ateliers they were regularly paid less than men.<sup>58</sup> In the Salpêtrière the administration relied in part on the profits of the unpaid labor of young girls to make ends meet.<sup>59</sup> Small wonder women continued to complain.<sup>60</sup>

Under the Old Regime women could sometimes vote and act as regents; during the Revolution they assumed their right to form political associations. Less than five years after the calling of the Estates-General this had all disappeared. The legislators barely considered female suffrage despite the heated arguments the feminists had put forward. Abbé Emmanuel-Joseph Sievès voiced the general opinion as early as July 1789. "Women, at least as things now stand, children, foreigners, in short those who contribute nothing to the public establishment, should have no direct influence on the government."61 The systematization of French electoral law eliminated the idiosyncrasies that had permitted women to vote; for the first time in centuries women were completely barred, as a group, from this aspect of the political process. Few people protested this exclusion. The women of Droits de l'Homme in Paris and the Républicaines Révolutionnaires castigated the provisions of the Constitution of 1793, but only by making speeches in the latter's club.62 Possibly the infrequency with which elections were held took the sting out of this exclusion; certainly at the level where politics really mattered, in the clubs and sections, women continued to vote for a time. Probably exclusion from the regency also mattered little, particularly when everyone was soon excluded by the abolition of the monarchy. The regency was not an important issue in itself, but it shows the ease with which the legislators could dismiss the idea of women participating in government.63

<sup>56 &</sup>quot;Un arrêté féministe du Comité de salut public en l'an III," Révolution française, 60 (1911): 266; the meetings of Dec. 9, May 28, 30, 1793, and Aug. 20, 1794, in Procès-verbaux du Comité d'instruction publique de la Convention nationale, 3: 87; 4: 493, 503, 963.

<sup>57</sup> Hufton, "Women in Revolution," 96; Georges Duval, Souvenirs thermidoriens (Paris, 1844), 1: 53 n.2. Sullerot estimates that there were over 100,000 lacemakers in the region of Le Puy in 1789. Histoire et sociologie du travail féminin, 71.

<sup>58</sup> Bouvier, Les femmes pendant la Révolution, 156-58.

<sup>&</sup>lt;sup>59</sup> Duc de la Rochesoucauld-Liancourt, "Rapport des Visites faites dans divers hôpitaux," 1790, in *Procès-verbaux et rapports du comité de mendicité*, 618.

<sup>60</sup> Report of Latour-Lamontagne, Sept. 13, 1793, in Caron, Paris pendant la Terreur, 1: 88. 61 Sieyès's preliminary remarks on the constitution, July 20–21, 1789, Recueil des pièces authentiques approuvées par l'Assemblée nationale de France (Geneva, 1789), 1: 193–99.

<sup>62</sup> Duhet, Les femmes et la Révolution, 136-37. 63 AP, Mar. 22, 23, 1791, vol. 24, pp. 261-67, 305-07.

Far more important to ordinary women than the vote or the regency was the issue of citizenship. Were women citizens enough to take the civic v oath, one of the central means of demonstrating acceptance of the revolutionary ideals and of participating in communal life? In 1790, when the National Assembly swore the oath, the spectators, men and women, joined them.64 Within two months women's right to take the oath had become an issue. Brigent Baudouin, wife of a municipal officer in Lanion, wrote the Assembly on behalf of several women in her village. "There is not a word about women in the Constitution, and I admit that they can take no part in government; nevertheless mothers can and should be citizens." They should therefore, she continued, be permitted to swear the revolutionary oath before the municipal officers. Goupil de Prefeln, a member of the Cercle Social, moved that all married women of "respectable conduct" be granted this honor. He added that mothers undoubtedly had more right to it than did childless women. The motion was tabled.65 Swearing civic oaths became particularly important in the summer of 1790 during the Fêtes de la Fédération. In Beaune the National Guard invited eighty-four women to the ceremony, but the municipal authorities firmly refused to let them take part. 66 In Toulouse the city officials, momentarily forgetting la galanterie française, turned the fire hoses on the women present to disperse them.<sup>67</sup> Examples could be found of women who did take the oath and who were invited to sign petitions and make other symbolic gesturesfor example, at the Champs de Mars in 1791—but the whole issue of women's citizenship remained clouded. With no sure rule to which to appeal, women had to depend on the good will of local authorities. Even at this low, but symbolically vital, level women's political status continued to be a matter of privileges not rights. The Committee of Public Safety and the Directory would both find themselves dealing with the consequences. A representative on mission, J.-B. Jérôme Bô, wrote the committee in 1794 to advise exemplary punishment of troublemakers, especially of those women who claimed that the law could not touch them because they had not taken the civic oath. In 1796 some factions used women to create disorder; since the government did not take them seriously, women could get away with subversive speeches for which men could be jailed.68

The revolutionary governments had at one time taken women's activities quite seriously, but only long enough to outlaw their clubs. Apparently article 7 of the second Declaration of Rights, guaranteeing the rights of free speech and assembly, no more applied to women than did article 5, which

<sup>64</sup> *Ibid.*, Feb. 4, 1790, vol. 11, p. 432. 65 *Ibid.*, Mar. 29, 1790, vol. 12, pp. 402-03.

<sup>66</sup> Moniteur, July 28, 1790, vol. 5, p. 240.

<sup>67</sup> Révolutions de Paris, Feb. 26-Mar. 5, 1791, no. 86, pp. 385-86n.

<sup>68</sup> Report of Mar. 29, 1794, in Recueil des Actes du Comité de salut public, ed. Alphonse Aulard (Paris 1889–1951), 12: 272; on the events of Apr. 17, 1796, see the Courrier républicain, quoted in Alphonse Aulard, Paris pendant la réaction thermidorienne et sous le Directoire (Paris, 1898–1902), 3: 126.

promised equal access to public office for all citizens. The Mountain sent the women's clubs crashing down in the fall of 1793. The ostensible cause was the unrevolutionary conduct of the Républicaines Révolutionnaires, a charge that could be supported in fact by the admiration of Claire Lacombe and Pauline Léon for Jacques Roux, Théophile Leclerc, and the enragés. The campaign against the Républicaines began in the Jacobin club on a strictly political note. A member announced that the women had taken up with Leclerc; François Chabot, Claude Basire, and Taschereau spoke against Lacombe's new political line. "I do not doubt that she is a tool of the counterrevolution," said Chabot sagely. 69 At this point the campaign against the Républicaines was specifically political and focused on them alone. Yet how soon the campaign changed! A month later a deputation of women from several sections came to the Convention to protest the activities of the Républicaines; one of them requested the abolition of their club. The Convention forwarded their complaint to the Committee of General Security. Fabre d'Eglantine made good use of this opportunity to address the Convention. After the bonnet rouge, which the Républicaines wore during their meetings, comes the gun belt, then the gun, he warned. He reminded the Convention of the manner in which the women went after bread: like pigs at a trough. These were not good mothers and daughters but-significant although false characterization-"des filles émancipées, des grenadiers femelles." The members several times interrupted his speech with applause. A little later one of the women spectators came forward to demand the abolition of all women's clubs.70

The Convention must have been gratified by the report André Amar soon presented on behalf of the Committee of General Security. That committee, explained Amar, had considered two questions: should women exercise political rights and take part in government, and should women meet in political associations? From the specific case of the Républicaines Révolutionnaires the government had moved to consider the status of all French women. To both questions the committee replied in the negative. Women did not have the strength of character needed to govern; political meetings took them away from "the more important concerns to which nature calls them." Nature's imperious commands were not to be violated; women could have no political rights. Amar concluded:

There is another aspect of women's associations that seems dangerous. If we take into account the fact that the political education of men is still at its very beginnings, that all the principles are not yet developed, and that we still stammer over the word "liberty," then how much less enlightened are women, whose moral

<sup>69</sup> Meeting of Sept. 16, 1793, in La Société des Jacobins: Recueil de documents, ed. Alphonse Aulard (Paris, 1889–97), 5: 406–08. The Jacobins had earlier encouraged the Républicaines and granted them "affiliation and correspondence." Meetings of May 12 and Aug. 15, 1793, in *ibid.*, 356. There is little doubt that it was the evolution of the women's politics to the left which drew the Jacobins' fire.

<sup>70</sup> AP, Oct. 29, 1793, vol. 78, pp. 20-22.

education has been practically nonexistent. Their presence in the sociétés populaires, then, would give an active part in government to persons exposed to error and seduction even more than are men. And, let us add that women, by their constitution, are open to an exaltation which could be ominous in public life. The interests of the state would soon be sacrificed to all the kinds of disruption and disorder that hysteria can produce.

Impressed, the Convention quickly voted to outlaw all women's clubs.71

Had the government been content to close the Club des Citoyennes Républicaines Révolutionnaires without making these explanations its attitude would remain ambiguous. By expanding its target to include all women, of whatever political or apolitical stripe, the Committee of General Security and the Convention made it clear that political questions were merely a pretext. What they wanted to do was to exclude women, as a group, from public life. Anaxagoras Chaumette, the procureur of the Paris Commune, summed up the new order a fortnight later. Speaking in response to the arrival of a deputation of women at the Conseil Général of the Commune he lectured: "So! Since when have people been allowed to renounce their sex? Since when has it been acceptable to see women abandon the pious duties of their households, their children's cradles, to appear in public, to take the floor and to make speeches, to come before the senate?"72 The Committee of Public Safety drove the message home in an "Avis aux Républicaines," which appeared in the semiofficial Feuille du Salut Public. The committee began its admonition on a menacing note. It reminded women of the fate of Marie-Antoinette, de Gouges, and Mme Roland. The purpose of this reminder was strikingly clear in the choice of de Gouges. "She wished to be a politician and it seems that the law has punished this conspirator for forgetting the virtues appropriate to her sex"—that is, not for the character of her opinions but for having had opinions. This lecture concluded by spelling out the virtuous life.

Women! Do you want to be Republicans? . . . Be simple in your dress, hardworking in your homes, never go to the popular assemblies wanting to speak there. But let your occasional presence there encourage your children. Then la Patrie will bless you, for you will have done for it what it has a right to expect from you.<sup>73</sup>

Few people protested the suppression of the women's clubs. Lacombe brought a deputation of women to the Convention the following day; the deputies howled them down and hooted them out.<sup>74</sup> In the provinces the

<sup>71</sup> Moniteur, Oct. 30, 1793, vol. 18, pp. 299-300. One member alone objected. He was ignored. Note that the decree against the women's clubs was a first step to the suppression of the sociétés populaires.

<sup>&</sup>lt;sup>72</sup> Ibid., Nov. 17, 1793, vol. 18, p. 450, italics in original.

<sup>73</sup> Ibid.

<sup>74</sup> AP, Nov. 5, 1793, vol. 78, p. 364. Caron, Paris pendant la Terreur, in which one might expect to find indications of popular reaction to the Convention's decision, is particularly fragmentary for October-November 1793. Possibly there was a good deal of protest that has left no trace, but this is not probable.

clubs quietly dissolved. For a time the women in Paris could continue to participate in sectional assemblies and mixed clubs like the Société Fraternelle du Panthéon. Perhaps this softened the blow; in the capital at least women still had political outlets. Yet their status in the men's clubs was unclear. In the assembly of the Panthéon-français section a deputation from the Société des Amis de la République was warmly applauded when its spokesman asserted that the ban on women's clubs also forbade them to vote in other clubs. A member of the Paris Commune disagreed, and the matter was dropped. Vague reports of women's organizations crop up in records from later periods—a leader of a "club des femmes jacobites" was arrested in May 1795; earlier the police had flushed out a "nid des jacobines"—but these reports are too ambiguous to prove anything about women's political activities. To

The suppression of the women's clubs effectively destroyed the feminists' political aspirations. It was not, however, the clearest statement on women's rights the government made. After the *journee* of rer Prairial of the Year III (May 20, 1795), the Convention voted to exclude women from its meetings; in future they would be allowed to watch only if they were accompanied by a man carrying a citizen's card. Three days later the Convention placed all Parisian women under a kind of house arrest. "All women are to return to their domiciles until otherwise ordered. Those found on the streets in groups of more than five one hour after the posting of this order will be dispersed by force and then held under arrest until public tranquillity is restored in Paris." The progress of the Revolution had rendered the brave hopes of the feminists of 1789–91 chimeric.

Only in regard to their legal status could feminists find some gratification. The Revolution, so severe to women in public life, was kinder to them in private life. Inheritance laws were changed to guarantee male and female children equal rights. Women reached majority at twenty-one under the new laws. Moreover they could contract debts and be witnesses in civil acts. Other legislation changed the laws concerning women's property, giving them some voice in its administration, and acknowledged the mother's part in decisions affecting her children. Revolutionary divorce

<sup>75</sup> See the reports in Caron, Paris pendant la Terreur, Dec. 29, 1793, vol. 2, p. 75; Jan. 7, 1794, vol. 2, p. 227; Jan. 16, 1794, vol. 2, p. 398; Jan. 25, 1794, vol. 3, p. 149; Jan. 27, 1794, vol. 3, p. 170; Feb. 5, 1794, vol. 3, p. 342; Mar. 26, 1794, vol. 6, pp. 117–18; Mar. 30, 1794, vol. 6, p. 207. March 30 is the last date for which Caron gives reports.

<sup>&</sup>lt;sup>76</sup> Ibid., Feb. 14, 1794, vol. 4, pp. 112-14.

<sup>77</sup> Police report for May 26, 1795, and excerpt from the Courrier républicain of Jan. 23, 1795, both quoted in Aulard, Paris pendant la réaction thermidorienne, 1: 748, 421.

<sup>78</sup> Moniteur, May 20, 1795, vol. 24, p. 515.

<sup>79</sup> Meeting of May 23, 1795, in *Proces-verbal de la Convention nationale* (Paris, 1792—an IV), 62: 67. I have found no trace of the repeal of this unenforceable law.

<sup>80</sup> Felix Ponteil, Les institutions de la France de 1814 à 1870 (Paris, 1966), 170.

<sup>81</sup> AP, Sept. 20, 1792, vol. 50, p. 181; Godechot, Les institutions de la France, 48. Despite the new law the Ministry of the Interior was still in doubt as to women's rights in respect of civil acts a year and a half later. AP, Jan. 14, 1794, vol. 83, p. 338.

<sup>82</sup> Garaud, La Révolution et l'égalité civile, 178; Sagnac, La législation civile, 296; Sullerot, Histoire de la presse féminine, 65.

legislation treated both sexes equally.<sup>83</sup> Yet some inequalities remained. Women could not serve on juries; in practice they were excluded from sitting on the Tribunaux de Famille, which attempted to settle family quarrels from 1790 to 1796.<sup>84</sup> Moreover the gains were short-lived. The Napoleonic codes swept away almost every advance the women had made, returning them to the status Pothier had described in 1769.<sup>85</sup>

THE MOST IMPORTANT REASON for the almost total failure of revolutionary feminism was its narrow base. Feminism was and remained a minority interest. The majority of French women—the logical constituency to which the movement could hope to appeal—had no interest in changing their social position. For the most part French women accepted the eighteenth-century definition of femininity. Far more typical of their attitudes than any of the feminist manifestoes is this speech made by the women of Épinal to their men.

If our strength had equaled our courage we would, like you, have hastened to take up weapons and would have shared with you the glory of having won our freedom. But it took stronger arms than ours to defeat the enemies of the Constitution; our weakness has prevented us from taking part in this Revolution. We content ourselves with admiring your efforts.<sup>86</sup>

The feminist movement had been unable to reach these women. Neither its words nor its action had made any sense to ordinary women. Feminism never became part of the program of the majority of the women's clubs. Only the Besançon club considered urging the Convention to extend the suffrage to women, but faced with the mockery of local Jacobins it soon abandoned the project.<sup>87</sup> At Orléans feminism never raised its head.<sup>88</sup> One of the few lengthy series of *procès-verbaux* available from a women's club, that of Ruffec (Charente), shows not a hint of feminist attitudes in two years.<sup>89</sup> The women's clubs were content to function as auxiliaries to male societies. The mixed clubs held themselves equally aloof, except for the short-lived efforts of the Cercle Social. If the various sociétés fraiernelles des deux sexes approved of feminism, they kept the secret to themselves.

The prominent women of the Revolution are conspicuous by their ab-

<sup>83</sup> On the legislation of Sept. 20, 1792, see Moniteur, Oct. 10, 1792, vol. 14, pp. 158-60.

<sup>84</sup> J. Forcioli, Une institution révolutionnaire: Le Tribunal de Famille d'après les archives du district de Caen (Caen, 1932), especially p. 39. See also Moniteur, Aug. 1, 1791, vol. 9, p. 276.

85 The spirit of the civil code is summed up in its article 213: "La femme doit obéissance au mari." One of the few gains women preserved from the revolutionary legislation was the principle of equal inheritance.

<sup>86 &</sup>quot;Compliment fait par les citoyennes d'Epinal à MM. le Députés arrivant de la Confédération générale de Paris, le mercredi 28 juillet 1790," La Révolution dans les Vosges, 17 (1929): 47.

<sup>&</sup>lt;sup>87</sup> Meetings of Feb. 6, 1793, and after, according to Langeron, "Le club ces femmes de Dijon," 6.

<sup>88</sup> Bloch, "Les femmes d'Orléans," 67.

<sup>89</sup> Chauvet, "Registre de la Société des amies des vrais amis de la Constitution à Ruffec (Charente), 1791-1792," Révolution française, 46 (1904): 246-78.

sence. Mme Robert, coeditor of the Mercure national, belonged to the Société Fraternelle des Jacobins, but she was no feminist. She told her club, apropos of women inspectors for the public hospitals, that women could contribute greatly to the success of the inspections, but she went on to add, "Their domestic duties, sacred duties important to the public order, prohibit their taking on any administrative functions, and I do not claim to draw them from their sphere."90 Mme Roland, too, accepted the status quo. "I am often annoyed to see women arguing over privileges that do not suit them; even the title of 'author' seems ridiculous for a woman to me. However gifted they may be in these fields, they ought not to display their talents to the public."91 The Directory is often described as a womandominated regime and Mme Tallien cited as a leading example of women's power in this era. However she once wrote to the Convention, "Woe indeed to those women who, scorning the glorious destiny to which they are called, express, in order to free themselves of their duties, the absurd ambition to take over men's responsibilities."92 Mme de Staël, perhaps the most important of the revolutionary women, seems to have had some feminist leanings, but she certainly cannot be brought forward as an activist.93 The pattern is clear: the most famous women of the period were careful to give the disreputable feminists a wide berth.

Nor did the supporters of women's rights capture the backing of the leading men of the Revolution. Condorcet was a real anomaly. Far more typical was Mirabeau, who gushed over the "irresistible power of weakness," warned that women's delicate constitutions limited them to the "shy labors" of the home, and pondered whether they should ever be let out of the house. 4 Jacques-Réné Hébert, as one would expect, did not gush. Although he took some earthy shots at wife beaters—"ces bougres de tyrans"—his sympathies were limited. 5 Robespierre's attitude remains enigmatic. Jacques Godechot asserts that he spoke in favor of votes for women in the Constituent Assembly, but other commentators place him in the opposite camp. The volumes of his *Oeuvres complètes* published to date shed no light. 6 Louis-Antoine Saint-Just would go as far as agreeing that laws on adultery should be equal for both sexes but, like Mirabeau, he belonged to the "faiblesse intéressante" school and urged that girls be educated at home,

<sup>90</sup> Quoted in Villiers, Histoire des clubs de femmes, 50-51.

<sup>91</sup> Quoted in L. J. Larcher and P. J. Martin, Les femmes peintes par elles-mêmes (Brussels, 1858), 68-69.

<sup>92</sup> AP, Apr. 23, 1794, vol. 89, p. 215...

<sup>93</sup> See Madelyn Gutwirth, "Mme de Staël, Rousseau and the Woman Question," Publications of the Modern Language Association, 86 (1971): 100-09.

<sup>94</sup> AP, Sept. 10, 1791, vol. 30, pp. 518-19.

<sup>95</sup> Jacques-Réné Hébert, Le Père Duchesne, Dec. 6, 1790, no. 31, in Le Père Duchesne d'Hébert, ed. Fritz Braesch (Paris, 1938), 391.

<sup>96</sup> Godechot, Les institutions de la France, 47. I have been unable to trace this motion. Sullerot (Histoire de la presse féminine, 63), Villiers (Histoire des clubs de femmes, 248), and Lytle ("Second Sex," 23) all consider Robespierre to be an opponent of women's emancipation.

with due regard for the preservation of their chastity.<sup>97</sup> For the rest, we can find clues in their newspapers. Louis Prudhomme's Révolutions de Paris reveled in misogyny.<sup>98</sup> Jean-Paul Marat, Camille Desmoulins, and Gracchus Babeuf ignored the women's movement.<sup>99</sup> Jacques Roux, like Condorcet, was an exception, but as a defender he was hardly an unmitigated blessing.<sup>100</sup> The feminists, then, had been unable to win the backing of any of the important Revolutionary factions. Their following was confined to a few clubs and to isolated individuals, many of them political moderates whom the progress of the Revolution incidentally eliminated.

The characters of the feminist leaders were scarcely the sort to find favor with the respectable. Of those whose lives we know, only Condorcet was above reproach. The pretensions to gentility of de Gouges. Théroigne de Méricourt, and the "Baronne" d'Aelders struck contemporaries as ludicrous, and this amusement carried over to their activities. The unsavory histories of Théroigne de Méricourt and Claire Lacombe did not help the movement any more than did Lacombe's and Léon's liaisons with the enragé Leclerc. While male revolutionaries might be forgiven their sexual peccadilloes, the women could count on no such toleration. Even as they protested the existence of a double standard it was at work against them. All of the feminist leaders were further compromised by their political convictions, whether moderate or extremist. Moreover the feminists were all held guilty for the acts of all other women—the emigrés, the tricoteuses, Marie-Antoinette, Charlotte Corday. Protest as they might, the feminists could never convince the public that the principle of collective responsibility should not be applied to the whole sex.

The feminists made tactical and strategic errors. Women's groups allowed themselves to be distracted too easily. The Républicaines Révolutionnaires let themselves become embroiled in street fights over the wearing of the cocarde and the bonnet rouge. All of the women's clubs suffered from their habit of putting other people's causes before their own. The provincial

<sup>97</sup> Louis-Antoine Saint-Just, "Esprit de la révolution et de la constitution française" (1791), in Oeuvres complètes de Saint-Just, ed. Charles Vellay (Paris, 1908), 1: 291–92; Saint-Just, "Fragments sur les institutions républicaines" (1793–94), in ibid., 2: 519.

<sup>98</sup> For example, "De l'influence de la révolution sur les femmes," Révolutions de Paris, Feb. 5-12, 1791, no. 83, pp. 226-35; "Addition à l'article des femmes pétitionnaires, no. 124," ibid., Dec. 10-17, no. 127, pp. 497-500; "Club de femmes à Lyon," ibid., Jan. 19-26, 1793, no. 185, pp. 234-35; "Femmes contre-révolutionnaires en bonnet rouge," ibid., Nov. 4, 1793, no. 213, pp. 150-51. Prudhomme demonstrated his "sympathy" for the Républicaines by publishing, after their dissolution, a procès-verbal of one of their last, and chaotic, meetings, that of October 28, 1793. "Procès-verbal de ce qui est arrivé aux Citoyennes républicaines-Révolutionnaires," ibid., Nov. 13-20, 1793, no. 215, pp. 207-10.

<sup>99</sup> See their journals, respectively, L'Ami du peuple and the Journal de la République française; Révolutions de France et de Brabant and the Vieux Cordelier; and Tribun du peuple.

100 Jacques Roux, speaking to the Paris Commune, June 21, 1793, in Scripta et Acta, ed. Walter Markov (Berlin, 1960), no. 51, pp. 472-78.

Walter Markov (Berlin, 1969), no. 51, pp. 472-73.

101 The wearing of the cocarde and the brawls this caused gave the government another pretext to move against the women's clubs. The affair is complicated and is more easily followed in Duval, Souvenirs thermidoriens, 1: 52-54, than by tracing it through either the AP or the Moniteur.

clubs settled meekly into ladies' aid societies, and even the fiery Républicaines were more interested in the price of bread than in women's wages. However commendable these positions may have been as expressions of largeness of spirit, they were sorely damaging to any attempt to work specific, radical change. The feminists showed other signs of political and managerial inexperience. They acted in isolation: individual leaders had no verifiable contacts with each other; the clubs proceeded independently, and the occasional attempts to set up a national organization came to nothing.

It would seem, too, that that vague entity, the spirit of the times, ran counter to the feminist revolution. One important aspect of this countercurrent was the ideal of the nuclear family. Time and again feminists tripped over the conviction that the changes they advocated were unnatural because women belonged in the home. This was the most frequent explanation given for refusing their requests. The idea of the family as a secure nest, maintained by the wife, to which the husband retired from his toil in the outside world, was a relatively recent development. It certainly did not reflect the reality of lower-class life, for lower-class women could not afford to spend all their time keeping house. It was the wealthy who developed a hagiographic tradition around the family. 102 Once women were firmly confined to the home there was no "need" for feminism, and the majority of middle-class politicians could only gaze upon it in blank astonishment. To their way of thinking, refusing the feminists' demands ought to have been counted as so many acts of kindness toward women, who were by nature too delicate for the dirty world into which the feminists tried to thrust them.

Revolutionary feminism began in a burst of enthusiasm. Its unpopularity, its own mistakes, and the blissful incomprehension and dogmatism of its opponents combined to obliterate it. While it lasted it was a very real phenomenon with a comprehensive program for social change, perhaps the most far-reaching such program of the Revolution. This very radicalism ensured that it would remain a minority movement, almost the preserve of crackpots. Influential contemporaries turned out speech after speech, newspaper after newspaper, report after report without ever acknowledging its existence. Despite its minority nature and its abject failure, revolutionary feminism is not without significance. It illustrates, as clearly as anything can, the changing seasons of the Revolutionary calendar and stands as striking proof of the essential social conservatism of this political upheaval.

102 Bardèche, Histoire des femmes, 2: 193. For examples of worshipful attitudes toward the family, read Edmond Pilon, La vie de famille au 18° siècle (Paris, 1923) and almost any of the proposals for national education listed in note 54 above. This ideal filtered down to the sans-culottes. See Albert Soboul, The Parisian Sans-Culottes and the French Revolution, 1793-1794 (Oxford, 1964), 244-45, but note the qualifications on common-law unions on pages 245-46. See also Duhet, Les femmes et la Révolution, 139. The convention of the idle wife may have originated with the aristocracy, but the idleness of aristocratic women is not comparable to that of bourgeois women. Aristocratic men could also make a virtue of inactivity, while bourgeois men are usually seen as making a cult of activity.

## Reviews of Books

## GENERAL.

WILLIAM TODD. History as Applied Science: A Philosophical Study. Detroit: Wayne State University Press. 1972. Pp. 250. \$11.95.

WESLEY MORRIS. Toward a New Historicism. Princeton: Princeton University Press. 1972. Pp. xii, 265. \$10.00.

Is history applied science? William Todd, who teaches philosophy at the University of Cincinnati, argues that it can be. In "Types of History," the longer introductory section of his work, he presents a dozen specific historical problems drawn from as many historians, then in the brief concluding argument, "A Methodology for History," attempts to convince the reader of his thesis. Because in their treatment of a given problem historians such as Huizinga, Brinton, Granet, Bullock, Namier, and others speculate that circumstances and choices might have been different, Todd concludes that all historians employ assumptions contrary to fact (counterfactuals) in order to highlight significances and to pronounce ethical judgments on the actors of the past.

Todd's argument for history as applied science hinges on his assertion that games, models, and operations research can be devised to provide dynamic simulations of actual historical situations. Would it not be possible, he suggests, to build a model that would simulate the past, then "simply plug in the constants which represent the past situation, . . . and play the simulation out a large number of times to see if the average outcomes are the ones that actually occurred" (p. 198)?

What would be the purpose of such an enterprise? "The practical suggestion here is that historical understanding can be perfected and can be carried further than it usually is if we do resort to the setting up and playing of dynamic simulations"; moreover, the methodology "forces us to make a more complete historical investigation than would have been made without the guidance of the simulation" (pp. 237-38). Yet all this extra effort may not be worthwhile if Todd is in error in his basic assumption that scientific causality and historical causality are alike. It would appear impossible to feed into the computer-operated simulation the dreams, hopes, and fears of the neurosis that is man. Even if repeated playings with randomly selected variables offered a pattern of probabilities it is well to recall the observation of Teilhard de Chardin: "In terms of physics and chemistry, the phenomena of life are essentially characterized (in precise contrast to those of matter) by an evolution toward the least probable." Is it otherwise with human history?

Wesley Morris of Rice University takes us on an exploratory journey into the frontiers of the new historicism, a term he employs to describe a now emerging form of American literary criticism. In unrolling the map of historicism he orients the observer quickly. Historicism is essentially a search to uncover a meaningful continuity in history by seeking for principles in the product of men's minds that will provide meaningful relationships. Transcending his own present the historicist enters into the minds of the past and builds a continuity between past and present.

Traditional historicism in literary evaluation Morris classifies according to four types: "metaphysical," which intuitively perceives a transcendent universal; "naturalistic," which borrows its principles from the natural and social sciences; "nationalistic," which uses the political or racial unit as the framework for its meaning; and "aesthetic," which focuses on the creative act of the novelist or poet. The first three tend to be aesthetically blind as meaning becomes confined to the extrinsic context of historical process. Aesthetic historicism, however, tends to collapse the literary work and the observed

historical context into the mind of the creative writer.

Morris argues that true criticism must deal with both aesthetics and historical interpretation. For literature exhibits a dual character: it is at once embedded in the time period of its creation, mirroring that time, and simultaneously a monument that rises above its circumstance to command attention on its own aesthetic merit. The new historicism essays the view that literature "is at once in history and above it," in the words of Roy Harvey Pearce, whose criticism has emerged from the traditional historicist side. Morris finds the fullest development of the new historicism in the aesthetics of Murray Krieger, who sees in the unique structure and language of a poem both the timeless aesthetic world and the existential, nonpoetic world of the poet.

While Morris addresses his work to students of American literature his excursion may be profitably traced by the student of American intellectual history or indeed by any historian who wishes to renew his understanding of historicism. The study is not a chronological survey but is problem oriented, and although the preface warns that it is "narrowly philosophic in nature," the reader is never at a loss as to his whereabouts and is carried forward by the compelling excitement of discovery.

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PARDON E. TILLINGHAST. The Specious Past: Historians and Others. (Addison-Wesley Series in History.) Reading, Mass.: Addison-Wesley Publishing Company. 1972. Pp. vii, 198. \$2.75.

This book "is concerned with why some people have troubled to spend their lives writing history and why others spend their leisure . . . in reading what the historians write" (p. iii). It contains a discursive examination of the purpose of the discipline, discussions of what historians and others have said about that purpose, and a defense of the present utility of studying the past. Tillinghast is particularly interested in the general reader, sympathizing with his desire to have historians tell meaningful, true stories of broad significance.

It is a difficult book to classify. As Tillinghast says, it "has the nature of an inquiry rather than a narrative or analysis" (p. 170). After a learned, but brief, account of "the relationship between historians and their audience" (p. iv) he proceeds to a consideration of the relevance

to history of science, literature, philosophy, and theology. Next there is a chapter about "historical rebels": Nietzsche, Spengler, Toynbee, and the New Left, presented as writers who have tried to recall historians to a broader conception of their role. Historicism is examined as an attempt on the part of some historians to defend the autonomy of the discipline. A relatively lengthy discussion of the role of moral judgments in history argues strongly that historians do and ought to make them.

There are some problems with this serious and engaging book. Its presentation of historical thought is extremely condensed, but its organization leads to some repetition. Tillinghast maintains that historians can and should provide their own time with a usable past, but the strategy of his study does not serve that argument consistently. He describes present issues in the discipline largely in terms of the history of similar issues in the past, drawing our attention away from the distinctive features of the contemporary situation. Perhaps it is unfair to criticize so short a book on so large a subject for omissions, but it is surprising to find no examination of Marx in a study concerned with the utility of history, particularly one that deals with historical rebels.

I have no quarrel with Tillinghast's conclusion, a fine humanistic defense of the continuing importance of the writing of history as offering "a deeper and fuller understanding of what we can and cannot be" (p. 178).

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RADOSLAV A. TSANOFF. Civilization and Progress. [Lexington:] University Press of Kentucky. 1971. Pp. vi, 376. \$12.50.

w. WARREN WAGAR, editor. History and the Idea of Mankind. (Sponsored by the Council for the Study of Mankind.) Albuquerque: University of New Mexico Press. 1971. Pp. xv, 227. \$12.00.

To provide a framework within which to discuss these two books of 1971, it is useful to turn to Thomas Kuhn's book of 1961, The Structure of Scientific Revolutions. Here Kuhn challenged the idea that modern science represented progress away from premodern scientific communities. Modern historians of science, according to Kuhn, postulated that modern scientists were able to make progress toward understanding an ultimate reality because they approached reality as individuals while premodern scientists were kept from reality by the

General 65

perspectives of their cultures. Kuhn argued, however, that modern science, like premodern science, depended upon hypotheses that expressed a cultural definition of reality. Modern scientists, like premodern scientists, worked as members of a community built on the acceptance of what Kuhn called paradigms.

If one applies Kuhn's model to the assumptions of modern historians in other areas such as politics or economics, one finds a challenge to the whole idea of modern history as the history of liberty, as the liberation of the individual from cultural boundaries. Kuhn's model, of course, is in accord with twentieth-century anthropologists who define man as a cultural animal. Kuhn insists that one scientific culture cannot be judged as inferior or superior to a different scientific culture. This relativism is central to the logic of the twentieth-century anthropologists who can no longer define primitive as inferior to civilized, who indeed can no longer define what is primitive.

Many intellectuals, however, seem to be working with the paradigm of nineteenth-century anthropology, which believed there was an inevitable progress from the savage childhood of mankind, through barbarism to a final civilized maturity. The philosopher Radoslav Tsanoff, in his book Civilization and Progress, accepts this series of stages. His book traces the history of the idea of progress from its appearance in the seventeenth century to its challenge by nineteenth- and twentieth-century intellectuals. Tsanoff rejects this criticism because he sees it based on the assumption that progress is inevitable. But, according to Tsanoff, when man has reached the stage of civilization, he then has increased choices for both good or evil. For Tsanoff then, human progress is unambiguous when it moves from savagery to civilization because that is when the individual is liberated from group definitions. Once the individual becomes free in the stage of civilization, he can make the choice of good or evil.

History and the Idea of Mankind represents a symposium organized by the Council for the Study of Mankind, an association of scholars founded in 1952 who were concerned that "the material unity which already and irrevocably exists [in the world] must be reinforced by legal, moral, and spiritual unity, which sadly . . . still does not exist."

It was hoped that this symposium would further the historical consciousness necessary for world unity. Part 1 includes essays on the idea of mankind held by five traditional societies. Kees Bolle discusses the idea in premodern India; Helmut Callis discusses China; Eliezer Berkovits discusses Judaism; S. D. Goitein discusses Islam; and W. Warren Wagar discusses classical and medieval Europe. Part 2 has essays by Hans Kohn on "Nationism and Internationalism," Melvin Kranzberg on "Science, Technology and the Unity of Mankind," Robert Lystad on "Race: Unity in Diversity," and W. Warren Wagar, "Religion, Ideology, and the Idea of Mankind in Contemporary History."

The book is a fascinating document that illustrates the increasing competition of paradigms within the historical profession. Bolle and Callis write with great respect for the premodern Indian and Chinese theories of unity within diversity. The logic of their position is similar to the relativism of Kuhn and the twentieth-century anthropologists. In no way do they write as if these premodern perspectives are more childish or less realistic than the social theories of the modern West.

On the other hand, Melvin Kranzberg imputes progressive values to modern science, which Kuhn rejects. Kranzberg sees modern science ending the darkness of the medieval world: "The idea of inexorable law operating in the political, social, and economic sphere in order to secure human happiness was a liberating idea which freed men from the grasp of superstition," and "the demands of an advancing technology will inevitably require equality of social, educational, and economic opportunity for the Negro population of the United States."

The most interesting expression of the paradigm crisis among current historians is the concluding essay by W. Warren Wagar. Writing as a historian of ideas, he describes a crisis in the modern belief in progress: "From the middle of the 18th century down to the middle of the 20th . . . the central project of the Western spirit was to find a world-encompassing faith to replace traditional Judaism and Christianity. . . . The great fact in the spiritual life of Western man in the years just after the 2nd World War was the apparent suspension of the search for new systems of secular faith. He suddenly found himself living in a post-ideological, anti-Utopia, demythologized world in which the will to believe had withered and failed." But Wagar dissociates this modern faith in progress from modern faith in science and technology and believes that "we must look forward to highly organized societies that will probably evolve in the direction of a unified world civilization. . . . It will be a world of science and technique, of large-scale industry and government planning."

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E. J. HOBSBAWM et al. Historical Studies Today. Edited by FELIX GILBERT and STEPHEN R. GRAUBARD. New York: W. W. Norton and Company. 1972. Pp. xxi, 469. Cloth \$12.00, paper \$3.95.

This sizeable tome is a reprint of the essays in the winter and spring 1971 issues of Daedalus. Its twenty authors and two editors deal with the ramifications of two questions: Are traditional historical fields being studied in a new way, and have new fields appeared? The answer to the second question is clearly yes, and to the first, just as clearly, yes and no.

The fields covered are traditional (political, diplomatic, economic, social, military, and intellectual) and newer (quantitative, history of science, psychological, urban, prosopographical, and oral). Some essays do not fit into any particular category, such as Schlesinger's on the historian as participant rather than as memorist, Dumont and Ladurie's on the quantitative study of French military archives, and Schwartz's on the relation between political and intellectual history in non-Western countries.

None of the essays is in any way incompetent, which is hardly surprising considering the high reputation of most of the authors; some of the essays are first-rate, especially those of Hobsbawm, Manuel, Le Goff, Schlesinger, and Vansina. Several of the positions are quite predictable, for instance, bows toward computerization, demography, the Annales school, and structuralism; the normal denigration of Whig history and the more old-fashioned, impressionistic political or intellectual history, especially that in Ranke's tradition; and the denial of space to any European period before the eighteenth century.

There is a distinct pecking order: social, quantitative, urban, and oral history and prosopography do not need defense at present, and are extending and consolidating their respective turfs. On the other hand the proponents of educational and intellectual history wish to tie them more firmly to theories of society as a whole. The tone of the political and diplomatic articles is noticeably defensive, and the essay on history of science offers an all too convincing proof that most historians have given at least its modern aspects a very wide berth.

This ranking seems to correspond closely to the present interests of the profession as a whole, or at least of its more voluble practitioners; that is, it is very sound. Fortunately, new ideas as well as prevailing orthodoxies keep surfacing. Some of the best sections are excursuses inside the articles, for example, Furet on "histoire événementielle" and serial history; Stone on elitism versus statistically mass-minded history; Le Goff on the implications of "polis" and "urbs"; and Paret on the implications of psychological theory on the history of war. Vansina's whole chapter can be read as an allegory for the edification of American and European historians whose minds are fixed on immutable categories of what constitutes proper historical evidence.

The overall theme is that the best history now is "scientific." Since, in view of Kuhn's essay, this obviously does not mean scienceoriented, it must mean that it is written chiefly for the guidance of other historians (and their apprentices). This is a pity because it suggests that nonspecialists are no longer an acceptable audience, except peripherally. It is also a pity that the editors have restricted their purview so firmly. In such a large volume, with space for excursuses and technical case studies, it is surprising that no room was found for any historians not based in the United States (preferably Princeton), France, or England, and even more surprising that the relation of history to any of the humanities is not discussed at all. Had the book been entitled "History and the Social Sciences," this would have been understandable, but the implication is that either the editors felt no work was being done in, for example, philosophy of history, religious history, or the history of art or literature; or else that what there is in these fields is unworthy of attention. The former proposition is not true, so one is driven to the latter. No one objects to rigor in historical research, but does this rigor exist exclusively on the frontiers between history and social science? The issue is real, and the preface or introduction might have been a good place to have made the editors' reasoning clear.

The volume has been well edited, and it occupies a worthy place with the volumes by Higham-Krieger-Gilbert, Gottschalk on generalization, and the earlier SSRC bulletins on historiography. Like them, it is a monument to the new historical scholasticism.

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General 67

ALFRED W. CROSBY, JR. The Columbian Exchange: Biological and Cultural Consequences of 1492. Foreword by Otto von MERING. (Contributions in American Studies, number 2.) Westport, Conn.: Greenwood Publishing Company. 1972. Pp. xv, 268. \$9.50.

Alfred W. Crosby, Jr., in his short, compact book of six essays illuminates the idea that only rarely in the preindustrial age could man alter the delicate balance of nature. But when Columbus opened the Western Hemisphere to European immigration and settlement he initiated a chain of biological changes that were ultimately global in scope. The interchange of organisms that transformed New and Old World ecology is a complex subject. The author concentrates on only three important aspects of the exchange: the epidemic diseases that ravaged both hemispheres, especially smallpox, measles, and syphilis; transplantation of Old and New World food crops, particularly wheat and potatoes from the East and maize, beans, and manioc from the West; and the introduction of European farm animals into the New World. The opening chapter outlines the thesis, stressing that evolution produced specialized flora and fauna in the Western Hemisphere that differed from those of the Old World. A study of blood types points out the homogeneity of the New World's human inhabitants. Beginning with the Columbian voyages, destructive exchanges of microorganisms decimated humanity on both sides of the Atlantic. In the essay "Conquistador y Pestilencia" the well-known relationship between epidemic disease and the conquest of the Americas is briefly described, emphasizing the role of smallpox and measles. In a fast and flowing discussion of the introduction of plants and animals into the New World unintentional transfers are brought sharply into focus. An essay on syphilis opens by comparing the Columbian hypothesis with the Unitarian theory of the disease's origin. The author concludes that the Columbian thesis is still valid. Concerning the introduction of New World food products into the Old World the author demonstrates the connection between Columbus and the population explosion. In so doing he re-evaluates the Malthusian theory. In his final essay Professor Crosby points out that the Columbian exchange has continued down through the centuries to the present. He concludes that world ecology has been permanently altered as continuing combination and competition between life forms developed increased homogeneity. His final conclusion is pessimistic: "The Columbian exchange has left us with not a richer but a more impoverished genetic pool. We, all of the life on this planet, are the less for Columbus, and the impoverishment will increase."

While much of the author's evidence comes from Latin America, especially the well-documented Spanish American colonies, he ranges widely for examples and illustrations from other areas. With a preponderance of material drawn from the Latin American experience the absence of information about early botanical surveys and useful drugs is disappointing. However, Professor Crosby's data is reliable, his text well written, and his ideas stimulating. He successfully synthesized a wide assortment of medical, cultural, and historical materials of great complexity. Unfortunately, there are a number of naive generalizations that are questionable. For example, the comment that in the fifteenth century "the Bible was the source of most wisdom" is an oversimplification. Most historians will not allow that "the most important changes brought on by the Columbus voyages were biological in nature." It is doubtful if anthropologists will accept: "No civilization has ever satisfactorily solved the problem of sex." Apart from such obvious statements the book is an excellent summary of an important subject.

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RONALD H. CHILCOTE, editor. Protest and Resistance in Angola and Brazil: Comparative Studies. (Published under the auspices of the African Studies Center and the Latin American Studies Center, University of California, Los Angeles.) Berkeley and Los Angeles: University of California Press. 1972. Pp. xvi, 317. \$12.00.

The first European power to participate in transatlantic capitalistic imperialism was Portugal, and the resulting heritage of exploitation, enslavement, and oppression burdens Brazilians and Angolans still. In Africa colonial status itself is only now disappearing. In Brazil a system of personal relationships characterized by hierarchy, deference, and submission perpetuates the colonial experience. As an African journalist pointed out in 1901, the Portuguese bequeathed "an authoritarian and violent way of life."

The reaction of the victims over a period of five hundred years has been largely muted and ineffective. The essays gathered in this book—which resulted from a series of papers presented in 1968—demonstrate that the only movements of "protest" that have had any significant impact were either elitist or reactionary. Manuel

Correia de Andrade, for instance, describes the four-year revolt of the Cabanos in Brazil in the 1830s, which aimed at the restoration of an absolutist king. Similarly, Ralph della Cava narrates the collaboration of an early twentieth-century folk leader with the landed and mercantile oligarchs of the arid Brazilian Northeast.

Other movements studied here are deeply ambiguous in their significance. Two papersby Alfredo Magarido and René Ribeiro-deal with religious movements, one in Africa and one in Brazil, and several others refer to messianic, sectarian, or church-directed social manifestations. But none of them deals systematically with the question alluded to by one author: Are these movements "the only possible form of resistance . . . [making] the exploited aware of their exploiters," or have they rather diverted "the resistance of the exploited groups . . . [and] retarded the struggle?" Similarly, the rise of banditry may be seen as a form of protest in which the outlaw is a kind of Robin Hood, but he can also be interpreted, as Amaury de. Souza seems to prefer in discussing the Brazilian case, as the result of the declining ability of landed patriarchs to control the private armies they had created. The concomitant result there was that the leaders turned to banditry for personal gain, creating an "entrepreneurship of violence." For neither banditry or messianism is the social psychology of the followers explored here; concentrating attention on leaders, the essays leave the reader predominantly with the notion of the retrograde purposes of these social phenomena.

Those movements that seem to have been genuine expressions of protest and resistance were discouraging failures. Michael Samuels explores an inconsequential demand for better educational facilities in Angola in 1910. Douglas L. Wheeler details some of the protest writings of the Angolan assimilados before 1930 and comes to the sad conclusion that they discovered that if they "desired 'progress' in political rights . . . they would have to cooperate with the authorities by criticizing African rebellions." Shepard Forman narrates the unsuccessful struggles of peasant leagues in northeast Brazil during the 1950s and early 1960s. He has to limit himself to the hope-or fear-that in the future the peasants will finally unite in struggle.

Neither editor nor authors, with the exception of Marvin Harris, seems to grasp the dismal impact of their collective effort. Yet this story of failure, ambiguity, or retrogression is the only unity in the book. The juxtaposition of Brazil and Angola implies a historical commonality and it would presumably be found in their Por-

tuguese and African heritage. Yet such a commonality never emerges as an analytical or conceptual device and is specifically denied in papers by Harris and Roger Bastide. Perhaps protest, resistance, or messianism are different in these two areas from such movements elsewhere, but this meaning does not seem to be intended by the authors. Alternatively, the point of comparative study may be not to explore similarities but to detail differences despite them. Yet only Harris devotes attention to such a task, and Ronald Chilcote, the editor, seems to misunderstand his purpose. The conceptual problem is exemplified by Chilcote's attempt to classify the movements of protest and resistance. First he separates Brazilian and Angolan experiences and then he is forced to lump Catholic Action, millenarian movements, banditry, military revolt in the countryside, and communist peasant leagues into the same subcategory. Perhaps the conclusion is that we must abandon the practice, of which I too have been guilty, of publishing multiauthored books that spring from the mind of the editor and not from the collaborative attempt of authors to deal with the same problem. When, in addition to the lack of unity, some of the contributions are disjointed, others badly translated, still others are summaries of findings better presented elsewhere, and the whole is published four years late, the editor may justifiably feel that his energy could have been more profitably directed.

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RONDO CAMERON, editor. Banking and Economic Development: Some Lessons of History. New York: Oxford University Press. 1972. Pp. ix, 267. \$8.95.

There are two theoretically polar positions concerning banking and economic development: (a) a fully developed banking system in an otherwise backward economy and (b) a fully developed modern economy that has no banking system. Examples of either do not exist. Reality lies between. This book examines, with eight case studies, the middle ground between the two extremes using Alexander Gerschenkron's hypothesis as a motivating paradigm. Gerschenkron believes that as economies are (have been) more relatively backward, the greater is (was) the motivating power of banking's entrepreneurial function in engendering those social changes that coalesce(ed) into the fabric of economic progress. In the present volume industrialization is at early stages, too,

General 69

in each country examined. The object of these essays is to examine specific economies during periods of economic change and modernization to determine how adequate the Gerschenkron thesis is as a guide to these histories. The authors, economies, and dates are: Richard Rudolph (Austria, 1800-1914), Jon S. Cohen (Italy, 1861-1914), Gabriel Tortilla (Spain, 1829-1914), John R. Lampe (Serbia, 1878-1912), Kozo Yamamura (Japan, 1868-1930), George D. Green (Louisiana, 1804-61), and Richard Sylla (United States, 1863-1913). There is in addition a substantial introduction by the editor, Rondo Cameron, treating the outlines of the theoretical argument, summarizing these empirical results against the theory.

Not surprisingly the Gerschenkron thesis is more useful in some cases than in others. The divergences are surprising and suggestive. What is most surprising though, at least to me, is that all of the authors have managed to weave their narrow concerns into larger questions of economic history in a way that is interesting, even fascinating. All of the essays are sharp, economically written, and supported by quantitative analyses that are the results of hard thinking and scholarly ingenuity. This is a most interesting little book. One might argue that Germany, rather than Austria, or England, rather than Serbia, would have been more useful case studies. I disagree. The question at issue needs to be illuminated by studies of detail, and no amount of theoretical acrobatics at the aggregate level is a substitute. Hence the justification for such studies as these. Testing the Gerschenkron thesis requires detailed analysis at the levels of disaggregation these authors explored. Serbia is as germane a subject as England for this purpose, just as is Louisiana, rather than the whole United States economy.

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JOSEPH HABERER. Politics and the Community of Science. New York: Van Nostrand Reinhold Company. 1969. Pp. vi, 337. \$6.50.

It is the thesis of this book that the politics of the modern community of science were established by the contrasting models of two of its founders, Descartes and Francis Bacon. For Bacon "science is designed to be applied—as a method, a body of knowledge, a social enterprise." For Descartes science is "basic," and the scientist's motive is "to create a universe, as if he were God, and to be able to understand it." Although differently organized as communities, and with different goals, both models of

modern science advocate a relationship to the larger world that Haberer characterizes as "prudential acquiescence." To use his words: "in any serious confrontation with State and Church, that is, with potential enemies who could inflict serious sanctions, they both advocated a tactic of prudential acquiescence: their theory and their conduct posited retreat or an apparent acquiescence as the appropriate response to danger." By way of illustration Haberer recounts the behavior and fate of German science under the Weimar and Nazi regimes, and of American science during the Oppenheimer affair.

Historians of science will be uncomfortable with the fact that Haberer's easy generalizations concerning Descartes and Bacon come from a less than thorough study of the rise of modern science, and political historians will no doubt quarrel with his characterization (or lack of it) of Germany in the 1920s and 1930s, and America in the 1950s. His considerable contribution is not to the traditional history of science, however, but to precisely those important questions to which that discipline seldom addresses itself. His insistence that "politics is an inherent component of the scientific enterprise" clashes directly with the official ideology of modern science—which has been, unfortunately, too often internalized as well by historians of the subject.

It is true, as Haberer points out, that "science still tends to speak with the voice of Descartes and act with the hand of Bacon." It is significant that many working scientists, ignorant of traditional histories of science, have read and worried about Haberer's book. Such charges as "scientists have almost always been pliant partners, willing under almost any condition to accommodate to a given political order," may not arise completely from his two case studies, but they apparently speak to some deep perceptions in the contemporary scientific community. Historians of science would do well to take seriously both Haberer's insights and the response he has found among scientists.

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HERMAN H. GOLDSTINE. The Computer: From Pascal to von Neumann. [Princeton:] Princeton University Press. 1972. Pp. x, 378. \$12.50.

The computer has become the metaphor and the representative technological device of our age. Dr. Herman H. Goldstine was a participant in the opening stages of the computer revolution that began only three decades ago. His narrative, although difficult and unnecessarily complicated at times, tells about the invention of the electronic digital computer for the calculation of ballistic tables. But, regrettably, he devotes the first third of the book to pre-World War II computers with the implication that they are progenitors of the modern machine. The computer in use today is no more in a historical line with Charles Babbage's analytical engine than the radio can be called a lineal descendant of the flag semiphor.

Dr. Goldstine's personal account, as he rightly claims, is an essential source for the historian, but that historian will have to be wary of being drawn into the trap of the author's too simple interpretations. His own experience contradicts his belief that new calculating machines came about with the convergence of new technology gears, relays, or vacuum tubes-and the recognition of the necessity for the advance. The Ballistic Research Laboratory sought a faster means of calculating tables, it is true, but their break-through came when they analyzed the problem into its simplest components, that is, components capable of being handled by electronic circuits doing simple-minded iterative operations. Stating the ballistic tables in the simplest mathematical terms and using electronic circuits was the first stage of the computer revolution.

John von Neumann, with Dr. Goldstine's help, prepared the way for the second stage of modern computer development. By generalizing the computer operation they showed that the key to its success was the two-symbol language. The original specialized computer became the model for all computers that handle large quantities of information. In this second stage the technological break-through became a socially determined innovation employed in an endless number of places, not only in computation of higher mathematics, but in the collection, storage, and retrieval of information. It is in this last function that university students find themselves identified by a number with little personal regard for their idiosyncracies.

Dr. Goldstine and those who worked with him deserve credit for the major technological break-throughs—the first stage. The rest of society bears responsibility for the many uses and abuses of the miracle of modern electronics—the second, socially determined, stage of the computer revolution.

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GEORGE W. CORNER. Doctor Kane of the Arctic Seas. Philadelphia: Temple University Press. 1972. Pp. xiii, 306. \$10.00.

Elisha Kent Kane (1820-57), naval surgeon and eldest son of Judge John Kane, a prominent Philadelphia Democrat, was the commander of the second Grinnell expedition of 1853-55 to the Arctic in search of the lost English explorer, Sir John Franklin. With little experience in naval command and only one previous adventure in Arctic exploration, in a series of unwise judgments Elisha Kane subjected his small ship's crew to two terrifying winters of ice-locked semistarvation off northern Greenland. The company suffered scurvy, madness, threat of mutiny, and the secession of a frightened group of the seamen who fled south only to return defeated to their captain. Kane finally led a desperate sledge journey across five hundred miles of ice desert and floe-choked ocean. Befriended by Eskimos, the party reached a whaling village in Baffin Bay, was intercepted by a United States rescue squadron, and was taken to New York for a tumultuous welcome.

The instigator of this expedition was an unlikely hero. Elisha Kane stood five feet four and suffered from recurrent rheumatic fever. He studied medicine after friends and family convinced him that engineering was too rigorous for the life of periodic invalidism he had ahead. He was driven by the contradictory needs of escaping his father's domination and of earning his esteem by a publicly acclaimed deed. The emotional double-bind prolonged his adolescent immaturity, apparently brought neurosis, and fostered the recklessness that would endanger his expedition. Not unexpectedly he was unable to free himself from family opposition to marry Margaret Fox, the famous spiritualist, with whom he had exchanged pledges before his fateful voyage.

George Corner portrays Kane with a minimum of psychological analysis, even though this treatment simplifies a man whom he admires. Rather, Corner emphasizes Kane's scientific pursuits (his medical thesis was published), which were genuine though superficial. The biography is well researched, drawing on family papers, and plainly written, but interest in it will be limited largely to a nonprofessional interest in the rash exploits of its hero.

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w. WARREN WAGAR, Good Tidings: The Belief in Progress from Darwin to Marcuse. BloomGeneral 71

ington: Indiana University Press. 1972. Pp. viii, 398. \$11.50.

RICHARD KING. The Party of Eros: Radical Social Thought and the Realm of Freedom. Chapel Hill: University of North Carolina Press. 1972. Pp. 227. \$7.50.

From the Enlightenment until the First World War the idea of progress pervaded most aspects of Western thought. Since 1914 it has fallen on evil times; its detractors have been numerous and its adherents both fewer in number and more qualified in their enthusiasm. W. Warren Wagar's Good Tidings traces the vicissitudes of the belief in progress from the Golden Age of Herbert Spencer to the Freudian radicalism of Marcuse. For Wagar belief in progress is simply a faith, "a thought form" rather than a doctrine with specific ideological content; its salient features are a view of history that traces general improvement in the temporal life of man into the predictable or possible future measured by some standard of good. After wisely establishing his own definition of progress, he gives a brief sketch of its history until la belle époque (i.e., 1880-1914), reiterating his previously published contentions as to the modernity of the notion. The book is then divided into three sections: the first deals with belief in progress in la belle époque; the second with critics of progress from the nineteenth century until the 1950s; and the third with the survival and subsequent resurrection of the idea from 1914 until 1970. Consequently two separate dialogues are established between the believers and the skeptics on the one hand and between a belief in a process of history and the harsh facts of history itself on the other. The net is cast wide and most aspects of human thought are incorporated—political thought, philosophy, theology, sociology, anthropology, psychology, natural science, literature. Those discussed range from Bergson and Dewey to R. H. S. Crossman and Kurt Vonnegut. Indeed, over one hundred and fifty individuals' views on the notion of progress are discussed. Herein lies the basic problem that confronts the author.

Until the 1880s Western thought had a unity, partially based on the belief in progress; thereafter it has disintegrated under the impact of its own fecundity into numerous "glorious fragments." Had each fragment taken a stand for or against progress, Wagar's task would have been easier. Many major figures' thought, however, is at best ambivalent or almost irrelevant when viewed from the point of view of a belief in progress. Thus Nietzsche appears as both

believer and skeptic; Karl Jaspers, indeed most existentialists, becomes very hard to assign finally to one camp or the other, and often, as in the case of Bonhoeffer, Robinson, and the "secular theologians," optimism and meliorist aspirations are equated to a belief in progress. Figures are often discussed, therefore, because of their stature rather than because they are prophets or critics of progress per se. As a result the book becomes virtually a history of Western thought since 1880, with the idea of progress serving as a compass to guide the way through the morass of its disintegration. As a result some interesting patterns and relationships emerge, and the tendency of the book to become an encyclopedia of modern thought is mitigated.

To cover so many figures is to invite criticism as to inclusions or omissions; but a few individuals this way or that would make no difference. The merely fleeting reference to literary naturalism and total omission of Einstein and relativity theory are slightly more puzzling, although symptomatic of the relative weakness in the treatment of both literature and natural science. Wagar is himself a firm believer in the concept of progress and shows that, understood in terms of an increasing knowledge and control of the natural world, it is still far from a historical curiosity.

One of the many virtues of Wagar's book is that it treats American and European thinkers together and Western thought as a whole. The dilemma facing Richard King in his The Party of Eros is maintaining a distinction and balance between European ideas and their American context and transfiguration. King solves his dilemma well. His immediate purpose is to study the way Sigmund Freud's ideas have been used by three radical social thinkers-Paul Goodman, Herbert Marcuse, and Norman O. Brown—in the twenty-five years since 1945. The broader purpose is to reveal the roots of the current counterculture. In an excellent chapter on the Progressives he shows not only how Progressivism helped create the society against which the counterculture is in revolt, but how the Progressives themselves, particularly Dewey and Randolph Bourne by their stress on youth, education, the primitive, and the generational conflict, helped create a vessel wherein Freudian ideas could flow. Given the sterility of American political thought since the thirties and the failure of orthodox Marxism in America, social radicals like Goodman turned to psychology, not economics or politics, to seek the cause of and solution to man's predicament.

King shows, too, that Freudian influence was complex; different people drew on different strands in his thought. It was the early Freud that influenced the "Beat Generation" via Wilhelm Reich and the later Freud that influenced Marcuse, Indeed, Brown, Goodman, and Marcuse are post-Freudians because Freud could not offer any consolation or liberation from repression. King also rather plays down the influence of Marx on Marcuse, tending to see Max Weber as more important. As an exploration of the ideology of the counterculture in the round, it has much to recommend it, but there are problems. Partly these stem from the material itself. Goodman, Marcuse, and Brown have much in common but are also much at odds; it is questionable if they do form a party, of "Eros" or of anything else. Furthermore, King is too defensive. He does not merely criticize the subject matter, he tends to argue with it. Yet this is a useful book not only in showing "where it's at" but, more important, how "it" got there.

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JAY MONAGHAN. Chile, Peru, and the California Gold Rush of 1849. Berkeley and Los Angeles: University of California Press. 1973. Pp. viii, 312. \$11.95.

The book under review is not a scholarly historical work. Rather it is in the genre of popular history of the American West, with the area of coverage simply extended to include Chile and Peru. A companion volume to Mr. Monaghan's earlier Australians and the Gold Rush: California and Down Under, 1849–1854, the present work explores the impact of the Gold Rush on Chileans and Peruvians as well as their roles in California. This is an interesting theme, probably meriting serious and systematic treatment. Unfortunately the topic does not receive such treatment here.

Mr. Monaghan has written more with the intent to entertain than to inform. He can never resist a good yarn (or even a not so good one), no matter how irrelevant to his quest. Thus for no clear reason the reader is treated to a four-page sketch of the conquest of Peru, complete with details on the execution of Atahualpa. And his account is sprinkled with equally irrelevant tales taken from nineteenth-century travel accounts.

Even accepting this fascination with the merely picturesque, one is offended by the author's flip tone toward Latin American culture, most flagrantly and foolishly demonstrated in supposedly literal translations from Spanish that preserve the Spanish linguistic structure for intended comic effect. ("'Like to me is said, las malaguas . . . bait irresistible make for fish. Si, Senor?'") As it turns out Mr. Monaghan's own grasp of Spanish is as "funny" as the structure of the Spanish language. ("Plaza des Armes," "Plaza des Gallos," etc.)

As to the basic content the narrative is most interesting in its treatment of matters in California. Mr. Monaghan thinks that anti-Chilean riots in the Gold Rush occurred because the Chileans got to some of the best claims first and were more effective and experienced miners than the other groups. Unfortunately his evidence for Chilean experience in gold panning is weak and specious. On Chile and Peru, Mr. Monaghan's treatment, based primarily upon standard Chilean works, travellers' accounts, and a few contemporary newspapers, is much less competent. He discusses but explains poorly why Chileans participated more actively in the Gold Rush than Peruvians. He is disappointed that the Gold Rush experience had little political impact upon Chile and Peru and only sloppy analysis enables him to suggest any at all.

I suppose that the University of California Press has issued this book in the hope that it will make money to pay for more serious works. Even if one accepts this motive the press ought to have its editorial wrist slapped for publishing a work that, in intellectual terms, is stunningly sloppy.

FRANK SAFFORD

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JACK M. SCHICK. The Berlin Crisis, 1958–1962. Philadelphia: University of Pennsylvania Press. 1971. Pp. xix, 266. \$9.50.

The theme of Jack M. Schick's book is Soviet-American diplomacy. Berlin is his vehicle for displaying it. For a generation the city was a barometer registering fluctuations in the Soviet-American relationship. Whether Berlin remains the place where "the United States is more dependent on nuclear retaliation to ensure a national commitment" is another matter. But even if the commitment is no longer clear this study dramatizes the importance of that divided city at a time when it was the sorest issue between the superpowers.

The anomaly of West Berlin isolated from the Federal Republic by over one hundred miles of East German territory yielded a series General 73

of crises between 1958 and 1962. In reviewing them Schick has brought into focus Dulles's insensitivity toward Russian fears of nuclear weapons in Germany, Khrushchev's reckless threats of nuclear destruction, Eisenhower's embarrassment over the U-2 affair, the meaning of the Berlin Wall, and finally, the intimate connection between Berlin and the Cuban missile crisis of 1962. His conceptualization is impressive, and he presents his analyses clearly. Occasionally his eye-catching subtitles go awry. For some reason he casts the Dulles-Khrushchev contest in the language of cricket, which seems an inappropriate sport to associate with these particular Russian and American statesmen.

The author's detachment is attractive. He is neither a cold warrior nor a New Leftist, although his approach may give more comfort to the former than to the latter. Although he ascribes Soviet aggressiveness to understandable concerns about German membership in NATO he also points out Khrushchev's succumbing to the dangerous temptation to exploit the West at its most vulnerable point on the mistaken assumption that the missile gap favored the Soviet Union. The record itself is one of repeated failure of statecraft on both sides in which fear of a nuclear holocaust stayed war, but not before they grappled at the brink. The worst cases of brinkmanship occurred under Kennedy over the Wall and Cuban missiles. Schick concludes that the Wall was no overnight coup, but a gradual development that accelerated when the United States failed to respond, and that the Soviet-Cuban adventure was intended to resolve the Berlin question. Soviet rashness was matched by American lack of clarity over objectives, although he applauds Kennedy's firmness and restraint over Cuba.

Sources, or lack of them, provide some difficulties for the author. He is forced at times into conjectures about Khrushchev's behavior that may not be correct. The European view of the conflict is relatively neglected. The German reaction is undeveloped, and German source materials are meager. Nevertheless, this is a contribution historians should turn to in untangling the interlocking crises of this period.

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ROBERT M. SLUSSER. The Berlin Crisis of 1961: Soviet-American Relations and the Struggle for Power in the Kremlin, June-November 1961. Baltimore: Johns Hopkins University Press. 1973. Pp. xvi, 509. Cloth \$17.50, paper \$8.50. ANATOL RAPOPORT. The Big Two: Soviet-American Perceptions of Foreign Policy. (American Involvement in the World, volume 1.) New York: Pegasus. 1971. Pp. 249. \$6.95.

The authors of these two books attempt to bring new perspectives to American-Russian relations during the cold war. Anatol Rapoport, a mathematical biologist ventures boldly into historians' terrain with an incisive interpretation of the confrontation between the United States and the Soviet Union in the past generation. His interpretation of the cold war is unmistakably revisionist. He emphasizes the Soviet Union's weak and essentially defensive posture after the Second World War, Truman's reversal of Roosevelt's cautious hopes for postwar Soviet-American cooperation, the Americans' half-hearted efforts to share atomic secrets and control atomic weapons with the Russians, and their deep-seated desire to impose their nation's liberal ideals and institutions on foreign peoples. He also calls the American political establishment "arrogantly antidemocratic" and criticizes traditional and "realist" scholars and American policymakers alike for their celebration of military strength and power politics.

Although Rapoport stresses America's offensive and aggressive foreign policies, he is less interested in assessing blame than in trying to persuade Americans to adopt new perspectives on international relations. Like many leftliberal and radical scholars, he focuses on the economic and social "substructure" or "roots" of American foreign policy, though he gives this approach the name "systemic." "The prime mover" of American diplomatic history, he asserts, "has been the need to expand . . . ; the so-called 'national interest' of the United States has been shaped to preserving and extending global capitalism." The Russians, on the other hand, had "no practical program for bringing the Communist world order into being." "In. short," Rapoport concludes, "while the American program of economic penetration was completely in accord with American political ideology (liberalism), there was no concrete program that could be geared to the supposed long-term goals of Communist ideology. . . . The Soviets lack the powerful instruments of economic penetration that are at the disposal of the United States."

He also uses social psychology to link the dynamic aspects of expansive capitalism to the national character. "People have an urge to engage in exciting collective activity," he suggests, and because "the extreme individualistic ideology" in the United States has progressively eroded community life and a sense of national purpose, Americans invented an anticommunist menace after 1945 to continue against the newly-proclaimed adversary USSR the sense of national purpose that had begun to fade with the defeat of the Axis powers. Moreover, the American civilian elite tolerates "the self-propelled burgeoning of the American war machine" because it appeals to their capitalistic faith in business and growth.

Despite his numerous provocative insights Rapoport is not entirely persuasive. He has used almost entirely secondary sources, and even these he often introduces as whipping boys for his own criticisms. Rapoport is most convincing in the negative stance of devil's advocate, but his own unconventional hypotheses lack the breadth of research and sustained theoretical musings of leading revisionists like Gabriel Kolko and Richard J. Barnet. Unlike many critics of American foreign policy, Rapoport attempts to evaluate the dynamics of Soviet foreign policy as well, but he is not very successful. His characterization of Nikita Khrushchev as a good-hearted man genuinely interested in détente with the United States is intriguing and plausible but is too thinly researched and speculative to be taken seriously.

Although Slusser, a historian of Russia, shares Rapoport's interest in recent Soviet diplomacy, in other respects his approach to cold-war tensions is markedly different. Rapoport covers twenty-five years of Soviet-American relations in 219 pages of text, while Slusser requires more than twice as much space to write a conventional narrative history of a six-month period during the Berlin crisis of 1961. True, Slusser's detailed analysis of Soviet policy is revisionist. He argues convincingly that deep fissures in the Soviet leadership between Premier Khrushchev's moderate faction and a hard-line group led by Frol R. Kozlov surfaced during the Berlin crisis and seriously threatened its peaceful resolution with the West. His detailed description of the volatile political situation adds an important dimension to recent Soviet-American relations.

But Slusser's long quotations, needless repetition, and labored prose, however, often obscure the significance of the events for an understanding either of Russian or cold-war history. Presumably the author believed his controversial thesis required exhaustive discussion of the entire story so that he could effectively parry all possible objections of Russian specialists. This preoccupation with minutiae may be defensible in elaborating the complexities of

Soviet politics, and the last portion of the book that meticulously chronicles the climax of the internal power struggle at the Twenty-second Party Congress is a valuable contribution to recent Soviet history. But the Soviet-American confrontation over Berlin is of minor significance by comparison, especially because the positions of the two powers had essentially stabilized very early in the crisis. Although Slusser's volume is a model of intensive research, intelligent speculation, and patient exposition, the tedious recapitulation of the moves and countermoves of the great powers is likely to disappoint many readers before they get to his analysis of the internal party strife in the Kremlin.

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International Bibliography of Historical Sciences. Volumes 37–38, 1968–1969, including some publications of previous years. Edited with the contributions of the national committees by MICHEL FRANÇOIS and NICOLAS TOLU for the International Committee of Historical Sciences, Lausanne. (Published with the assistance of UNESCO and under the patronage of the International Council for Philosophy and Humanistic Studies.) Paris: Librairie Armand Colin. 1971. Pp. xxviii, 654.

These two-volumes-in-one of the International Bibliography of Historical Sciences represent an effort by the International Committee of Historical Sciences to bring the bibliography up to date. The last previous volume (vol. 36, 1967) was published in 1970, three years after the date of publication of the items entered in the bibliography. Volumes 37–38 (1968–69) appeared only two years after the publication of the entries for 1969. The same plan will be followed for volumes 39–40 (1970–71). Thereafter the pattern of one volume per year is expected to be resumed.

Unfortunately the quickened publication rate has resulted in a reduction in coverage. Prior to the appearance of this double volume, the number of entries in the annual bibliography had grown steadily—from 4,908 in volume 1 to 8,390 in volume 36. But volumes 37–38 together have only 10,945 entries, and a comparable limitation will apparently be placed on volumes 39–40.

Dismal predictions for the bibliography's future emanate from responsible officials. Funds are increasingly inadequate. Julien Cain, president of the Bibliographical Commission of the International Committee on Historical Sciences,

Ancient 75

which supervises preparation of the bibliography, pleads for help from the 14th International Congress of Historical Sciences that will meet in San Francisco in 1975.

The American Historical Association, which represents the historians of the United States on the International Committee, sponsor of the Congress, should initiate action to increase the support provided for the *International Bibliography*. The AHA's current annual contribution to the International Committee is 500 Swiss francs, approximately \$150. Surely the foremost historical organization in the world's richest nation can afford to contribute a significantly larger amount than that.

Increased support now by the AHA for the International Bibliography would help to offset several decades of indifference toward that publication. It would also be in harmony with the renewed interest at the AHA in historical bibliography. Although the organization's commitment to a new annual listing of periodical articles on American history probably precludes its soon undertaking large-scale projects for history in general, the AHA could reassert the leadership it once held in the field of international historical bibliography and by example and persuasion make certain that the 14th Congress takes steps not only to assure the survival of the International Bibliography but also to foster its growth.

Because of changes taking place in the researcher's environment in the United States, historical bibliographies are likely to be of greater service in the future than in the past. As college campuses grow, history professors cannot rely on having offices and classrooms a few strides away from the library. Those who once could walk quickly to the library to use its large reference sets and its card catalog, long the historian's subject bibliography, have already in some instances been moved blocks away. Students have known this handicap for years, and many of them are being steadily housed at still greater distances. Similarly, the large, well-equipped public libraries are becoming more difficult to reach in the burgeoning cities. Even more serious is the questionable future of card catalogs. In some libraries, personnel who seldom use the tools of research have been placed in charge of making decisions about catalogs and are attracted to space-saving book catalogs, which are cumbersome and frustrating to the regular researcher. If the card catalogs cannot be saved, historians will be especially grateful to have bibliographies on their own shelves.

A fully developed International Bibliography of Historical Sciences could be the pinnacle of a pyramid of historical bibliography. At the base, in the United States, would be the book reviews, book lists, and article lists in historical journals; the new annual list of articles on American history undertaken by the AHA; and the abstracts of articles in ABC-Clio's Historical Abstracts and America: History and Life. In the middle could be a revived and expanded Writings on American History, for example, or the "American History Index" proposed by ABC-Clio; but either alternative could omit articles and restrict its coverage to such items as books, dissertations, proceedings, and pamphlets. At the top could be the International Bibliography, a compilation—possibly selective rather than all-inclusive-of items already identified at the other levels. If all the nations participating in the International Committee built such a bibliographical structure and thus enriched their contributions to the International Bibliography, it would be a special treasure for historians and libraries all over the world.

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## ANCIENT

DEREK ROE. Prehistory: An Introduction. Berkeley and Los Angeles: University of California Press. 1970. Pp. 288. \$6.50.

GRAHAME CLARK. Aspects of Prehistory. Berkeley and Los Angeles: University of California Press. 1970. Pp. xiii, 161. \$5.95.

Prehistory is today a popular subject. As a result sometimes a book on prehistoric archeology is published whose raison d'être is unclear. Prehistory: An Introduction is perhaps an example of this.

I am not sure to whom this book is addressed; the title suggests one audience, the content another. Certainly it is not a general introduction to prehistory or to what prehistorians are doing and thinking. The author admits this in his preface, but not loudly enough. The dust jacket almost says the opposite. Actually the book is a traditional tour guide to the high spots of British prehistory with barely enough on Europe and other parts of the Old World thrown in to set the British story in perspective. The chapters are arranged chronologically, with the many "cultures" marching along like soldiers on parade. More dynamic issues such as the na-

ture of culture and the processes and causes of cultural change are not discussed, and the reader is left thinking that the prehistorian is still little more than a cataloger of ancient pots and pans. All this makes some sense if one is writing an introductory grammar of British prehistory for the beginner, yet the University of California Press was certainly hoping to catch a larger audience. On the other hand, readable introductions to British prehistory are already available that are perhaps better because they are more sharply focused. One wonders if the author was led astray by his publisher.

These strictures aside, the book is well written, the illustrations are adequate, the bibliography is excellent, and the notes add much to the value of the whole. Indeed in the notes the author gives us a glimpse of the book he could have and perhaps should have written. Had the whole been pitched to the level of the notes, and had it dealt in some detail with many of the issues only touched upon in those notes, then the author would have caught a wider audience and we would have a better and more useful book.

Aspects of Prehistory is a slightly expanded and documented version of three lectures given by Professor Clark at Berkeley in 1969. Here it is clear what audience is addressed; both lectures and book are for the interested, educated general public. Though the book contains little the practicing prehistorian will find new, some professionals will benefit from the straightforward and organized way in which Clark puts his case.

The author is concerned with certain broad, general concepts that may be said to emerge from the study of prehistory. The first chapter, "The Relevance of Prehistory," explores the proposition that "world prehistory makes it sufficiently plain that both literacy and urban living are extremely recent. From the perspective allowed by prehistory we are all on much the same level" (p. 50). The second chapter outlines man's material progress since becoming a tool user some two million years ago. It is here that Clark most clearly articulates his underlying assumption, which is that our social and material development from the Lower Paleolithic to the present is simply the working out, in a different matrix, of processes of change similar to those of biological evolution. The outstanding aspect of this pattern of social and technological evolution is a progressive growth of man's self-awareness. The final chapter, the best in the book, is a history of that growing self-awareness and a discussion of its implications for the past, present, and future of man. To become self-aware is to become human, and "societies of human character have been better fitted for survival in a cultural milieu precisely to the extent that they are human; and the record of archaeology testifies over the last few tens of thousands of generations to an increasingly human pattern of behavior" (p. 146). Though I do not entirely agree with the argument, as a prehistorian I appreciate the point and would suggest that these ideas are worth thinking about.

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KATHLEEN KENYON. Royal Cities of the Old Testament. New York: Schocken Books. 1971. Pp. xi, 164. \$10.00.

The title of this book is taken from Dr. Kathleen Kenyon's lecture delivered in 1965 before the Palestine Exploration Fund in connection with its centenary celebrations.

The volume, which is primarily archeological, is up to high professional standards. Numerous photographs enable the reader to understand the material concretely. Maps and charts help him locate the places, visualize the terrain, and view ground plans and elevations.

Constant attention is paid to historic setting based on primary sources, which are mainly but not entirely Biblical. The chronological statement on page x and the table on page xi will aid the reader in sorting out the details and arranging them in sequence.

Technical terms are often defined for general readers. For example, though every student of Old Testament problems knows what "high-places" are, Miss Kenyon takes the trouble to define them as the generic designation of heather shrines (p. 117).

While we could include more sites among "the royal cities of the Old Testament" than the author does, no one should quarrel with her because she states exactly what she means and gives her reasons. Thus 1 Kings 9:15 groups Jerusalem, Hazor, Megiddo, and Gezer as special royal cities of Solomon. The only addition the author makes is Samaria, the main capital of the northern Kingdom of Israel.

The book includes the author's presentation of her own excavations in Jerusalem before the 1967 war. Her account of archeological work in Jerusalem provides the background for the Ancient 77

later monumental expeditions of Benjamin Mazar, which are not mentioned. The bibliography records no item published after 1966.

R. A. S. Macalister's publication of his diggings at Gezer was long revered as "the Bible of Palestinian archeology" and still retains considerable importance. Yet his work at Gezer was so lacking in stratigraphic evidence that Miss Kenyon feels obliged virtually to omit a discussion of that site. The book before us therefore deals only with Jerusalem, Megiddo, Hazor, and Samaria.

Miss Kenyon formulates her methods scientifically and clearly (for example, her careful "assessment of pottery evidence" on p. 122). Yet with complete integrity she points out again and again that even the most methodical archeological work often leads only to very tentative conclusions.

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CARL ANDRESEN. Einführung in die christliche Archäologie. (Die Kirche in ihrer Geschichte, volume 1, series B, part 1.) Göttingen: Vandenhoeck & Ruprecht. 1971. Pp. 175. DM 32.50.

Few scholars could match the broad erudition of the distinguished Church historian, Carl Andresen, and few would be better qualified to write a handbook on the art and archeology of the Church. The present work is not an introduction to Christian archeology for laymen. The Einführung is instead a valuable reference work for scholars who wish to begin the investigation of a particular aspect of Christian art and archeology.

Andresen's monograph is divided into two major parts. The first (pp. 11-62) consists of detailed bibliographies with brief discussions of painting, sculpture, architecture, etc. The second section (pp. 62-142) is a survey of special themes from the pre-Constantinian period to the Age of Justinian.

The scope of the book is staggering and the number of works cited is stupefying. The author has attempted to survey fields of study covering areas from England to Iraq and periods from the first to the ninth century A.D. Scholars will be indebted to Andresen for sharing with them the accumulated notes of a lifetime of scholarship.

In view of the inherently impossible task of mastering all fields and the exciting but frustrating accretion of data provided by current excavations, there are some striking deficiencies in Andresen's work. Some of these lacunae may be understood in view of the limitation of space. There is, for example, a listing of works on the excavations at St. Peter's in Rome (pp. 68–69), but no discussion of this significant and controversial subject.

Some deficiencies, however, seem related to the author's personal interests and competence. He is most instructive in dealing with the later periods of Church history but he is not a helpful guide for the archeological developments in Palestine at the dawn of Christianity. In a discussion of the pre-Constantinian period (p. 63) his earliest archeological citation is a catacomb at Rome from the second century A.D.!

Though Andresen acknowledges the importance of Jewish prototypes for Christian art and archeology, his references to Jewish monuments leave much to be desired. His bibliographical list on Jewish mosaics (p. 14) contains no reference to important mosaics excavated by Yadin at Masada from 1963 to 1965. In the bibliography on synagogues (p. 3c) it is surprising to find no references to the important first-century synagogue at Masada or to the enormous synagogue discovered at Sardis.

Important archeological evidences for early Christianity in Palestine are also neglected. Though the recent excavations at the Church of the Holy Sepulchre are noted, no notice is taken of Kenyon's excavations at the nearby Muristan that help to confirm the authenticity of the site as the scene of Christ's crucifixion and burial. References to ossuaries and their symbols, which may possibly provide evidence for early Christianity, are lacking. Nor is note taken of such important discoveries in Israel as the first epigraphical documentation of Pilate found in 1961 and of Felix the procurator found in 1966. As Andresen completed his manuscript in the fall of 1970 he may not have known of the first archeological evidence for crucifixion, discovered in Jerusalem in 1968 and published in the Israel Exploration Journal in 1970.

It seems that the surprising omission of the latter journal in the list of recommended periodicals (p. 34) and the listing of the Bulletin of the American Schools of Oriental Research inexplicably only up to the 1957 issue indicate a blind spot in the author's almost all-encompassing vision. This is most unfortunate inasmuch as some of the most significant archeological developments for the early history of Christianity, such as the excavation at Pella and the discovery of Byzantine churches near Amman,

are being made in the areas of Israel and Jordan—the birthplace of Christianity.

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M. I. FINLEY. Early Greece: The Bronze and Archaic Ages. (Ancient Culture and Society.) New York: W. W. Norton and Company. 1970. Pp. 155. \$5.00.

Moses Finley, the professor of ancient history at Cambridge University, sketches here the history of Greece from earliest times to ca. 500 B.C. in a book of 150 pages. Such a scope does not make for an expansive treatment, but he has made good use of his space and it is clear that Finley, the general editor of the series to which this book belongs, has written the book he wanted to write. This work is not the result of boiling down a long manuscript to manageable size, nor is it a potted history of Greece.

What Finley does is to give the reader his own interpretation of the historical development of Greece prior to the Persian Wars, consciously setting the stage for what is to follow. He concentrates upon what he considers to be the essential characteristics of each period and makes no attempt at narrative history. In sixty-eight pages Finley summarizes the history of Greece before 1200 B.C. emphasizing the palaces of Minoan Crete, the shaft graves of Mycenae, and the writing systems of the Bronze Age Aegean. His account is balanced and up to date, including a discussion of the new-cult objects from Mycenae (p. 57).

Finley is very skeptical about mass migrations and total displacement of populations, stressing the importance of commerce in the foreign expansion of Greece. The Greeks were forced to go abroad for many of their basic needs, especially metals. Finley's explanation of the Minotaur legend (p. 40f.) is not very convincing, and I certainly would not agree with the statement (p. 45) that "Late Minoan II saw Cnossus at the height of its power" (and why Cnossus, when Knossos has become standard and is so preferable?). Any discussion of the Greek Neolithic must now include the material from the Franchthi Cave in the Argolid (compare, T. W. Jacobsen, Hesperia, 42 [1373]: 45-88). The most significant recent publication dealing with the Bronze Age Aegean is Colin Renfrew's The Emergence of Civilisation. The Cyclades and the Aegean in the Third Millen-. nium B.C. (London, 1972).

As in a number of other publications, Finley

is very critical of those who are trying to turn Greek legends and myths into history. Finley himself has recently been taken to task by one of his favorite targets, George Mylonas-in Mylonas's Mycenae's Last Century of Greatness (Sydney, 1968). I am in full agreement with Finley's position and endorse his remark (p. 10) that: "One is free to believe if one wishes that King Minos of Cnossus, Agamemnon of Mycenae and Priam of Troy were historical personages, not figures of myth; no one has found them on the spot in any shape whatsoever, not even as a name on a slab or a seal-stone." As for the "Age of Homer," attention should be given to the discussion by A. M. Snodgrass (Gnomon, 42 [1970]: 157-66).

The "Dark Age" of Greek history has been treated in recent books by A. M. Snodgrass and V. R. d'A. Desborough, both published in 1972. The fall of Mycenaean civilization, the Trojan War, the Dorian Invasion, and the transition from Bronze Age to Iron Age all still present the historian with many problems. Finley follows recent interpretations, especially that of C. G. Starr, in emphasizing the formative nature of the obscure centuries between 1200 and 800 B.C. Protogeometric pottery, the introduction of cremation in burials, and the spread of the use of iron are the age. The significance of possible contacts with Europe is still an unresolved question. Desborough emphasizes the presence of northern invaders, while Snodgrass denies their very existence. Finley is ambiguous: northern invaders would provide a welcome explanation, if only their existence could be substantiated.

The Archaic period (800-500 B.C.) is dominated by the introduction of hoplite warfare and the rise of the tyrant. Hoplites fighting in phalanx formation meant an increase in the number of men bearing arms; fighting for the polis led to demands for a voice in the affairs of that polis. Finley stresses the popularity of the Greek tyrants and the paradox that, by breaking the habit of aristocratic rule, the tyrants actually helped to bring about democratic government (p. 107). The fairly abrupt transitions from monarchy to oligarchy to tyranny to democracy are all related to the social upheavals resulting from overpopulation amid new social and economic pressures. The most characteristic response was colonization, a movement that cannot be explained in terms of trade and the search for new sources of metal (p. 97).

Early Greece concludes with separate chapters on Sparta and Athens. While this emphasis Ancient 79

provides a convenient introduction to the Classical period, when Greek history was dominated by those two states, it does give a misleading conception of the Archaic period itself, a time when states such as Argos and Corinth were as important, if not more important, than either Athens or Sparta. The interested reader would do well to consult W. G. Forrest's Emergence of Greek Democracy, 800-400 B.C. (1966), an excellent account not even listed in Finley's brief bibliography.

In these days of lavish books, thin on text but rich in gorgeous color illustrations, it is a pleasure to find a book that has the text as its central concern. Early Greece is not a handsome book, not a "tribute to the skill of the modern art of printing," but the printing is adequate and the book provides an excellent introduction to the history of pre-Classical Greece at a very modest price. It only remains to add that the book is now available in a French translation, as Les premiers temps de la Grece, l'âge de bronze et l'époque archaïque (Paris [François Maspero], 1973). This includes a few minor corrections and additions to the text as well as a revised bibliography.

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CHIARA PECORELLA LONGO. "Eterie" e gruppi politici nell'Atene del IV sec. a. C. (Università di Padova, Pubblicazioni della Facoltà di Lettere e Filosofia, volume 48.) Florence: Leo S. Olschki Editore. 1971. Pp. 162. L. 3,500.

In 1913 George Miller Calhoun had his doctoral dissertation, Athenian Clubs in Politics and Litigation, reprinted from the University of Texas Bulletin, number 262, and that monograph remains today a starting point for investigations of this elusive phenomenon at Athens. The earlier clubs were societies made up of men whose backgrounds, interests, and age were about the same. Sometimes the clubs took the form of sworn associations and were so named. In Athens during the sixth and fifth centuries B.C. they were important units of social and political action. From Thucydides especially, we know the part they played in the oligarchic revolution of 411 B.C. Then in the fourth century B.C., although their presence can be detected at publicly and privately motivated legal trials, they seem to have lost the sort of coherence and power that changes governments.

Now Chiara Pecorella Longo in her own doctoral dissertation reviews the comparatively abundant evidence for political life in fourth-

century B.C. Athens to test the traditional view that the clubs (hetaireiai) were active and influential only in restricted spheres after ca. 403 B.C. She accordingly surveys possible references to such associations in Plato, Isocrates, Xenophon, and Aristotle (ch. 1); argues that the impeachment law preserved in an oration of Hyperides was re-edited in 403-02 B.C. after its initial formulation in 411-10 B.C. (ch. 2); discusses the political motivation of Andocides's trial in 399 B.C. (ch. 3); enumerates various groups associated with various Athenian political leaders (chs. 4-7); and concludes that the traditional view is correct. Clubs lost much political power in the fourth century and were most active in law courts. Dr. Longo adds that certain words or phrases that in earlier times were used to denote clubs, in the fourth century sometimes referred instead merely to groups formed for financial or political reasons and identified with a particular man.

Dr. Longo's acquaintance with the literature, both ancient and modern, is wide ranging if not exhaustive. She could, for instance, have speculated on the contents of the defixionum tabellae from Athens that are relevant to her theme. Her assessments of modern scholarship are, however, generally sensible. Her conclusions have the sanction of long usage and reputable approval, and her study can be cited as a responsible confirmation of a useful hypothesis.

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K. D. WHITE. Roman Farming. (Aspects of Greek and Roman Life.) Ithaca: Cornell University Press. 1970. Pp. 536. \$12.50.

In the Roman imperial period, as throughout the history of the ancient Mediterranean world, agriculture formed the economic basis of society. The vast administrative structure of the Empire relied upon an agricultural surplus, and agriculture was of similar importance in the private sector of the economy. As under the Republic, although farm management was inliberalis labor, land was the primary form of investment. By the Lex Claudia of 218 B.C. senators were forbidden to own ships other than small ones to transport the produce of their estates; and throughout its existence the senatorial class remained a landed aristocracy. The pervasive importance of agriculture in the economic life of ancient Italy secures for it a historical significance far above other aspects of ancient industry and technology. Agrarian reform is a leitmotiv in the social struggles of the early Republic and in the century following the Gracchi, while the extent and causes of agricultural decline in late antiquity is essential to any study of the decline of Roman power in the West.

In this context ancient political historians, as well as social and economic historians, will find a valuable tool in K. D. White's encyclopedic survey of agricultural techniques and organization in Roman Italy. After a brief critical exposition of the literary and archeological sources and a summary of the natural resources of the various regions of Italy, White proceeds to a detailed treatment of the variety of soils available for cultivation and methods used in antiquity to maintain soil fertility, including crop rotation, fertilizers and manures, and land drainage and irrigation. A major portion of the text is devoted to the theory and practice of field crop husbandry, arboriculture, horticulture, and animal husbandry. The work concludes with discussions of agricultural personnel and management, systems of farming, size of farm units, agricultural architecture, and the level of technical achievement obtained in Roman agriculture. The absence of a bibliography is regrettable, for all may not have access to White's full and critical Bibliography of Roman Agriculture (University of Reading, 1970). In the same way the reader must supplement White's disappointingly brief remarks on farm equipment by reference to the same author's Agricultural Implements of the Roman World (Cambridge, 1968). The text is complemented by a series of appendixes, treating such topics as methods of protecting stored grain, comparative estimates of labor productivity, and soil conditions appropriate to particular crops.

White's Roman Farming is a masterly survey of a complex topic, and it is of enormous value as a reference work for the present. Nonetheless its treatment of many problems, some of considerable historical interest, must be regarded not as exhaustive and authoritative expositions but rather as preliminary sketches and guides to future research. Although generally judicious in his handling of the sources, White is perhaps overly enthusiastic in his acceptance of the precepts of Roman agricultural writers as evidence for contemporary agricultural practice (compare, Columella 1. praef. 4-5, 7, 13). At points White is not sufficiently critical in his use of nontechnical sources, for example, accepting without reservation (p. 320) the testimony of the Historia Augusta concerning Aurelian's free distribution of pork. White underestimates the enormous cost of land transportation and the role it played in marketing (compare, A. H. Jones, The Later Roman Empire, Two Eighty-Four to Six Hundred Two [Cambridge, 1964], pp. 841-55). Surprisingly White gives almost no attention to deforestation, and there is no discussion of exciting new material derived from the analysis of sedimentary deposits (compare, J. B. Ward-Perkins, Landscape and History in Central Italy [1964], pp. 2-11). The evidence for free contract labor is far more extensive than White will allow (p. 368). The explicit remarks of Varro (De re rustica 1.17.2) and others (Pliny 14.10; Suetonius Vespasian 1.14 etc.) can be supplemented by epigraphical testimony (CIL VIII.11824).

These strictures, however, should not detract from the fundamental importance of White's work of synthesis and the value of this study as a major contribution to ancient economic history and more particularly to the study of ancient technology.

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R. M. OGILVIE. The Romans and Their Gods in the Age of Augustus. (Ancient Culture and Society.) New York: W. W. Norton and Company. 1969. Pp. 135. \$5.00.

In the conclusion of his useful monograph Ogilvie observes that "the smoke no longer curls up from the sacrifices in the Forum; the augur no longer takes his seat on the Capitol to watch the birds wheeling overhead. Yet its ultimate failure should not tempt us to underestimate the validity of Roman religion." Certainly no one will be so tempted after reading this book.

Although the title of the work is The Romans and Their Gods in the Age of Augustus it should be quickly pointed out that the book is broader in scope than its title indicates. While the author's express goal is to illustrate how Roman religion could claim the faith of the Augustan age, it is necessary in dealing with a subject so complex as Roman religion to range far afield in time and topic. In eight chapters, which cover the gods, prayer, sacrifice, divination, the religious year, private religion, the priests, and religion in Augustus's time, Ogilvie moves easily across the whole area of the Roman religious experience to provide a literate survey that, though clearly designed for the layman, may be read with profit by the more casual reader and specialist alike.

Ancient 81

Thus, for example, Ogilvie ranges from the elder Pliny's remark that "trees were the temples of the spirits and . . . simple farming communities even now dedicate an outstanding tree to a god," to the observation that "the greater gods had their favorite haunts many of which were overseas as a result of the identification of Greek and Roman gods and the resulting attachment of Greek myths to the old, native beliefs."

That Augustus undertook, as part of his general program to heal the wounds of a hundred years of civil war, a broad range of activities designed to revitalize Roman religion is well known. It is Ogilvie's belief, which he carefully documents in the chapter that he devotes specifically to the Augustan period, that the Augustan religious revival was not simply something arbitrarily imposed on the Roman people, but, rather, something that had deep emotional appeal to a tired and confused people who had lost much of their self-confidence. Surely he is right in taking this view, and surely he has performed a good service in providing us with this unpretentious reappraisal of an area a knowledge of which is essential to the understanding of the recovery of confidence by the Romans under Augustus.

JAMES G. HARRISON, JR. Converse College

O. A. W. DILKE. The Roman Land Surveyors: An Introduction to the Agrimensores. New York: Barnes and Noble. 1971. Pp. 260. \$12.50.

Land surveying was very important for the Romans, with their strong practicality and sense of property. Dilke's appendix A lists thirty-five works in the corpus of ancient treatises on the subject. Their authors, agrimensores, run from Frontinus in the late first to Boethius and Cassiodorus in the first half of the fifth century A.D. The survival of such technical and specialized pieces when so much of Latin literature is lost attests to the concern for land surveying on the part of both the Romans and the medieval scholars who decided what to copy from the materials that they inherited from the late Empire. Modern historians of Rome may, however, find the works of the agrimensores of little significance except insofar as centuriation, the laying out of allotments, was a regular feature of the founding of colonies. To this topic Dilke devotes three chapters, 6, 10, and 14.

For those who are interested in Roman surveying, Dilke begins with a study of the back-

ground in Near Eastern and Hellenistic procedures. He then describes the training of Roman surveyors, the instruments that they used, the actual measurement of land and the establishment of boundaries, mapping, and finally the archeological and epigraphical evidence. The archeological evidence consists chiefly of traces of boundaries (limites) of centuriation plots as revealed either on the ground or from the air. The epigraphical evidence comprises actual boundary stones and several inscribed surveys (cadasters) from Orange in France. It should be noted that the technical chapters are not easy for a layman to follow, particularly that on the instruments used (ch. 5). The general reader will find more comprehensible and informative the chapters on land measurement and centuriation in relation to colonies and state domains.

The book is fully illustrated. It offers reproductions of miniatures and drawings from the medieval manuscripts of the agrimensores, illustrations whose originals appear to date from the late Empire. There are also photographs and drawings of surviving instruments, of traces of centuriation as viewed from the air, and of relevant stones and inscriptions. At the end of the book is a list of the locations of surviving sources. On this it should be observed that the fragments of the Forma Urbis Romae, the well-known Severan plan of Rome, are not on the wall of a garden of the Museo dei Conservatori; what is there is a copy of Lanciani's reconstruction (1893-1901). The original fragments of the Forma were studied by G. Carettoni and others for a new publication in 1960 and are still in the Palazzo Braschi (the Museo di Roma). The book concludes with a glossary, notes and references, two appendixes (on the contents of the corpus, and on the meaning of kardo and decumanus), a bibliography by chapters, acknowledgments, and an index.

MASON HAMMOND Harvard University

DAVID P. JORDAN. Gibbon and His Roman Empire. Urbana: University of Illinois Press. 1971. Pp. xv, 245. \$8.95.

In the seven essays that make up this book, Jordan discusses Gibbon's life, his use of contemporary scholarship, his relation to the writers and ideas of the Enlightenment, the influence upon him of five earlier writers (Tillemont, Pascal, Bayle, Tacitus, and Montesquieu), and Gibbon's own treatment of Constantine. A brief summary chapter surveys Gibbon's explanations

of the fall of Rome, and discusses the extent of Gibbon's identification with his subject.

It should be said at once that this is an interesting and useful book: it is well written and organized (despite a tendency to repetition); accurate (checking has revealed no mistaken references, and, other than words in Latin, there are very few misprints); and Jordan is both judicious in his use of sources and careful in his acknowledgment of debts.

The book, nevertheless, serves rather as an introduction to several aspects of Gibbon's life and work than as a major contribution to our understanding of Gibbon. A number of the chapters are based, at least in part, on previous studies, as Jordan himself generously acknowledges. Thus, the study of contemporary scholarship owes much to Momigliano; Gibbon's gradual development of an ironic narrative mode has been noted by Braudy; and Jordan himself has previously dealt with Gibbon's treatment of Constantine. Again, most of the essays are left as rather general treatments, and Jordan is not overly concerned with either detail or documentation. Thus, Pascal's Jansenism and Gibbon's reaction to it are described in five pages without a single reference to Pascal's works and only one to Gibbon's (pp. 149-54); intellectual influences on Gibbon are discussed here in a manner that is neither so detailed nor so incisive as Dawson's earlier work; occasionally topics are not specifically discussed at all, as with the sources of Gibbon's picture of Constantine (surely, for example, Constantine's hypocrisy in hiding his true character as a young man is much influenced by Tacitus's picture of Tiberius); finally, a major theme, such as Gibbon's irony, may be treated only very briefly by the inclusion of some ten examples with very little discussion (pp. 155-57).

This same general and introductory quality of the book is indicated by Jordan's tendency to treat everything from Gibbon's point of view, rather than moving outside Gibbon to establish a context, and by the frequent use of short quotations with no indication of the source. Ultimately, one feels that Gibbon's work and study as a young man have still not been assessed adequately, and it remains difficult not to share at least some of Horace Walpole's surprise at the unexpected appearance of the Decline and Fall of the Roman Empire.

GEORGE W. HOUSTON
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G. M. DURANT. Britain, Rome's Most Northerly Province: A History of Roman Britain, A.D.

43-A.D. 450. New York: St. Martin's Press. 1972. Pp. 179. \$6.95.

This book is based on the premise that Roman Britain is a "closed book to many if not most of the inhabitants of the British Isles." What is needed to rectify this situation, the author implies, is a book that is "neither intimidating nor darkly obscure." Whether the first statement is true or not I am unable to judge. But it is certainly true that Mr. Durant has written a frankly popular history of Roman Britain, I should add here that there is nothing wrong with popular history, provided that it is not bad history, any more than there is anything wrong with talented amateurs writing popular or any other kind of history. Britain, more than any other country, has produced a remarkable breed of free-lance historians. Whether Mr. Durant is one of them I do not know since he is not identified on the dust jacket. I doubt it for his book embodies two major faults. In the first place, although his book was copyrighted in Britain in 1969, his perfunctory bibliography contains nothing printed since 1963 and contains some curious items like Sir Charles Oman's England before the Norman Conquest (1938) and Charles W. Previte-Orton's Shorter Cambridge Medieval History (1952). The contents of the book also bear out the fact that he did not delve very deeply nor very recently into his subject. Second, Mr. Durant frequently indulges in statements that are needlessly speculative when they are not sheer guesswork. For example, Maximian's "unskilful, untrained sailors were no match for the seamanship of Carausius and his crews." Or after the capture of Fullofaudes in 367 (Durant says death thus confusing him with Nectaridius) the remnants of the sixth and twentieth legions "disbanded themselves in panic, or fought isolated ineffective actions in bands scattered here, there and everywhere." The above observations, however, have little to do with the audience intended by Mr. Durant. He has written a lively, readable introduction to Roman Britain that, despite its intellectual deficiencies, might well serve the purpose for which it is intended.

DONALD A. WHITE
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EVANGELOS K. CHRYSOS. To Vyzantion kai hoi Gotthoi: Symbolē eis tēn exōterikēn politikēn tou Vyzantiou kata ton tetartov aiōna [Byzantium and the Goths: A Study of Byzantiue Foreign Policy in the 4th Century]. (Idryma Meletōn Chersonēsou tou aimou, 130.) Thessaloniki: Hetaireia Makedonikōn Spoudōn. 1972. Pp. 195.

Medieval 83

As the title suggests, the bulk of this book is indeed a study of the whole span of relations between the Roman Empire and the Visigoths in the fourth century. Scholarly treatments of all or parts of this span already exist, but Chrysos's approach differs in being not only comprehensive but also in that it presents, from an essentially diplomatic and institutional point of view, an analysis of the application to the Visigoths of the status of foederati.

Chrysos scrutinizes systematically all the primary sources and the interpretations of modern scholars, sifting through the various phases of his topic and reassessing thoroughly all of their details. Predictably, he divides the topic into four main episodes, with a distinct chapter for each: Constantine the Great's dealings with the Goths and his treaty of 332; the Christianization of the Goths and the career of Ulfila (whose personal role is given much weight); the dilution of the relationship and its crisis under Valens; and the pacification under Theodosius the Great, with his treaties of 380 and 382, to the final stages of the Gothic position as foederati and its collapse with the career of Alaric

There are fine points that could be disputed in Chrysos's study, but he is quite circumspect and fair in his presentation. Moreover, what gives his work its point is the set of transcendent perspectives into which he attempts to set his topic. The first of these perspectives consists of the institutional implications and development in the application of the foederatus status. The second is the argument of Stauffenberg and others (which Chrysos rejects) that the treaty of 332 marks a sharp new departure in imperial policy, replacing an "imperial" concept with a "universal" concept of relationships between the Empire and its barbarian neighbors. The third is the need that Chrysos perceives for relating the initiatives in Christianizing the barbarians to the broader aims of the Empire's external policy.

As a result, and above all, Chrysos seeks to present his topic as a case study in Byzantine diplomatic history and methodology. In opposing Stauffenberg's position he sees the fourth century as a transitional period of adaptation, rather than as an era of radical innovation. On the other hand, he sees this period as formative in generating the first elements of subsequent Byzantine theory concerning the "family of rulers" as presided over by the Byzantine emperor, a theory examined extensively by Dölger and other recent scholars. He concludes: "The Eastern Empire's experience with the institution of the Visigoths as foederati was not

forgotten. It was applied successfully in the fifth century and later it constituted the basis for the establishment of the Empire's relationships with the peoples that came to be settled in the Balkan Peninsula" (pp. 173-74).

Chrysos has produced a useful treatment of aspects of fourth-century history and of Romano-barbarian relations. At the same time he has provided a stimulating explication of the origins of what would later become basic principles of mature Byzantine diplomatic theory and practice. In so doing he has made a worth-while contribution to illuminating that notorious, fascinating, but sadly neglected time zone in which the late antique and the early Byzantine overlap. The only regret is that the book's language will tend to limit the circulation and accessibility it deserves.

JOHN W. BARKER
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## **MEDIEVAL**

HELMUT MAURER. Konstanz als ottonischer Bischofssitz: Zum selbstverständnis geistlichen Fürstentums im 10. Jahrhundert. (Veröffentlichungen des Max-Planck-Instituts für Geschichte, 39. Studien zur Germania Sacra, 12.) Göttingen: Vandenhoeck & Ruprecht. 1973. Pp. 99, 9 plates, 1 map. DM 18.

In contrast to much current work on medieval town origins, this essay ascribes the tenth-century growth of the city of Constance entirely to its bishops' desire to glorify the Church and their Ottonian masters by reproducing the sacral and ecclesiastical topography of Rome. The careers of the three most active bishops are sketched to show how a minor bishopric, contained within the walls of a small Roman fort, had grown by the beginning of the eleventh century into a major trading town, attracting Italian merchants to its linen market, and comparable as a branch of the imperial Church to Liège or Hildesheim.

A pupil of Notker the Stammerer, Bishop Salomo III, started the program, rebuilding the minster to house a relic of the martyr Pelagius he had brought from Rome. The resulting influx of pilgrims led to the establishment of a market and a mint. Next, Bishop Konrad built an almshouse and two new churches, one of which, St. Paul's, was constructed deliberately away from the city's center to duplicate Rome's St. Paul Without the Walls. He also presented the church of St. Lawrence with a relic of its martyr, which he had obtained in Rome, and

and the second

founded a church in honor of St. Maurice. Since Otto I had ascribed his victory over the Magyars in 955 to the aid of Saints Lawrence and Maurice, Konrad was clearly expressing his loyalty to the emperor as well as accommodating a growing population. The most recherché reduplication of the Roman map was made toward the end of the century when Bishop Gerhard founded St. Peter's monastery in the swampy land on the north bank of the Rhine (providing it with a relic of Gregory the Great) to imitate St. Peter's situation in Rome, where the church is located across the Tiber from the main part of the city. Between them the three bishops had built a "new Rome" as a microcosm of the renovated Roman Empire, with five station churches strung out along a processional route, and had secured the patronage of the Ottonian house by the most judicious flattery.

BERNARD S. SMITH
Swarthmore College

PAUL DE VOOGHT. Jacobellus de Střibro (†1429): Premier théologien du hussitisme. (Bibliothèque de la Revue d'Histoire Ecclésiastique, number 54.) Louvain: Bureaux de la R.H.E., Bibliothèque de l'Université. 1972. Pp. xv, 413. 600 fr. B.

With this study Dom De Vooght has filed a brief in defense of a successor of Hus written in much the same spirit as the author's earlier defense of Hus himself (L'Hérésie de Jean Huss [Louvain, 1960]). Jacobellus, who was a friend and colleague of Hus and a theological leader of the Hussites for more than a decade after Hus's death, is described by De Vooght as having remained within the boundaries of "Catholic tradition" on most points of his teaching. Like the defense of Hus, this brief for Jacobellus stays exclusively on a theological level. For De Vooght the question is not whether Jacobellus exuded a reformist or revolutionary spirit. He asks only if Jacobellus asserted theological positions that a fair-minded, congenially ecumenical judgment could call heretical.

The reader can only wonder if Jacobellus would have retained such a one as De Vooght for his advocate. That zealous spirit Kaminsky ascribed to Jacobellus, the spirit that insisted that Communion in both kinds was not merely tolerable but necessary to salvation, that Communion should be given to children, that the Roman Church was the embodiment of the anti-Christ, that immoral behavior by priests threatened the efficacy of their ministry—that spirit is not emphasized by De Vooght. In every

case the author focuses on the limits of Jacobellus's critique not on his radical thrust. In order to save Jacobellus, De Vooght postulates radical evil and heresy in other persons and then denies that Jacobellus was in league with them. Thus Wyclif is the archheretic and the Taborites the intolerable radicals. Jacobellus's distance from them is emphasized.

The splendid isolation of the theological approach becomes particularly difficult as De Vooght explicates Jacobellus's essay against usury. A discussion of merchant classes in Bohemian cities and their reaction to this issue as it reflected on their attitude toward the Hussite revolt would have been most helpful.

Even granting De Vooght his methodological presuppositions, some points are unclear. Jacobellus is lauded for adhering to "Catholic tradition," but as a matter of fact, quotations from his own writings pay obeisance to the early Church. Scripture and the early Church are used by Jacobellus to criticize that which "Catholic tradition" has handed over to the early fifteenth-century Bohemian Church. De Vooght paints an exceedingly negative picture of Gerson and his colleagues at Constance who opposed Utraquism. In fact, De Vooght seems to share Jacobellus's criticism of what had become of "Catholic tradition" by 1415. When "Catholic tradition" lost its way is unclear. Scholastics as late as Thomas are cited as respected authorities. The tradition after Thomas is more criticized than respected. Had De Vooght filed his brief in the fifteenth century rather than in the twentieth, Gerson and his circle might well have found him to be in contempt of court.

The study is done with the thoroughness and care De Vooght lavished on his earlier defense of Hus. Readers who might quarrel with the method employed by the author are rewarded with long quotations from Jacobellus's writing including critical editions of three essays in the appendix. The work is another landmark venture, offered from the Catholic side, to aid the search for ecumenical rapprochement. No student of the development of the Hussite movement can neglect it.

PAUL L. NYHUS
Bowdoin College

DAVID JACOBY. La féodalité en Grèce médiévale. Les "Assises de Romanie": Sources, application et diffusion. (École Pratique des Hautes Études, VI° Section. Documents et recherches sur l'économie des pays byzantins, islamiques et slaves et leur relations commerciales au Moyen Âge, Medieval 85

number 10.) Paris: Mouton & Co. 1971. Pp. 358. 72 fr.

With the occupation of Constantinople in 1204 by the armies of the Fourth Crusade and the establishment of the Latin Empire, vast territories of the Greek mainland and the Aegean archipelago—the area called Romania—came under Western, mainly French and Venetian, influence. An immediate impact of the Latin domination was the introduction of Westerntype feudal institutions and the development of a feudal law in the area.

Professor Jacoby, from the Hebraic University of Jerusalem, in his exhaustive and richly detailed study examines the origins, formative elements, and evolution of the feudal law of Frankish Morea, as compiled in the Assizes of Romania, giving full and detailed account of the application and recognition of the assizes in various other parts of Frankish Greece and in the Venetian possessions of the Levant. The book is essentially legal history, and in this field it will certainly remain the most comprehensive and indispensable work of reference for a long time. Citing numerous individual legal cases and analyzing the relevant articles of the assizes, the author demonstrates the complex nature of the law of Romania that blended Western feudal concepts with local Greek customs and Byzantine legal elements. Through adaptation in the Venetian possessions, it was also influenced by the more centralistic judiciary system of the Republic of Venice.

On the basis of certain historical facts and names mentioned in the assizes, the author places the time of the compilation of the first complete French text of the Assizes of Romania in the time period between 1333 and 1346, when the principality of Morea already passed under the suzerainty of the House of the Angevins of Naples. The original French text was lost; an Italian translation in Venetian dialect was prepared and approved in 1421 by the chancery of the doge of Venice as authentic text (autenticum cancellariae). Eventually, in 1459 and after the addition of thirty-seven new articles, the Senate of Venice declared this official text of the assizes as a law in force in all Venetian possessions of the area.

The Venetian version survived in twelve manuscript copies dating from various periods between 1423 and the second half of the eighteenth century. Professor Jacoby analyzes each manuscript separately, and in discussing the application and recognition of the assizes in the Peloponnesus and in the Venetian posses-

sions of the Aegean, he includes a great number of individual case histories. They refer to the personal status of noblemen, litigations over proprietary and feudal rights, questions of inheritance, and, besides their legal aspects, offer interesting insights into the complex social structure and the political and economic conditions of medieval Greece. As appendix, twenty unpublished documents in Latin from the State Archives of Venice complete the book.

Professor Jacoby's work takes full account of the voluminous primary and secondary sources and reveals a fascinating and lesser-frequented area and period of medieval history. His technique of accumulating a great amount of detail material makes it sometimes difficult for the uninitiated reader to piece together a coherent picture of the feudal society of medieval Greece; the academic scholar, who is interested in the study of the period, will find in the book an invaluable source of information.

ANDREW URBANSKY
University of Bridgeport

CONSTANCE HEAD. Justinian II of Byzantium. Madison: University of Wisconsin Press. 1972. Pp. xi, 181. \$10.00.

Professor Head has two primary aims in her recent book on Justinian II: to reinterpret his reign in a way that stresses its accomplishments, and to write a work that will be readable by nonspecialists.

In the second aim she succeeds admirably. This is a well-written book, easily accessible to readers who have an interest in Byzantine history without being specialists in the field. This is a virtue that is rare enough in historical works, and perhaps rarer still in the Byzantine field. It does entail certain problems. For example, the treatment of the thorny problems of the themes, the village communities, and fiscal reforms (chapters 11 and 12) is so summary as to be almost superficial. The Slavic settlements in the Byzantine Empire are never discussed sufficiently; Professor Head speaks of "Sklavinia" in such general terms that the nonspecialist may well wonder whether this was a defined area and if so where it was situated and what its extent was. These problems stem from the fact that the book tries to rehabilitate Justinian II, and so the primary interest is with the man rather than with the issues.

The first aim, that is, the reinterpretation of the reign of Justinian II, is the one that presents the most interest for the specialist. In fact, Justinian II has already been partially rehabilitated. The first part of his reign (685-95) is generally considered to have been a productive and successful one. In what concerns this decade. Professor Head discusses achievements about which there is no longer much debate; of course, a service is rendered by the fact that the record is here presented in a unified and detailed fashion. Much more debatable are Justinian's activities after his return to the throne in 705. Professor Head tries to show his "constructive statesmanship and genuine concern for his empire" (p. 115) and contrasts it to the general impression that Justinian's second reign was marked primarily by acts of revenge against his enemies. But the only area where it could even be argued that Justinian was engaged in statesmanship was the area of foreign policy. Here one can hardly be persuaded that his fruitless quest for foreign alliances is proof of any great statesmanship. The discussion of Justinian's expedition against Ravenna is convincing in its claim that the campaign was an issue of policy and not merely an act of vengeance. But on the whole, the reader will be justified in considering Justinian's second reign as a series of unmitigated disasters.

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DONALD M. NICOL. The Last Genturies of Byzantium, 1261-1453. New York: St. Martin's Press. 1972. Pp. xii, 481. \$14.95.

With the rapid advance of Byzantine scholarship, detailed monographs have in the last decade or so been written on the reigns of all the Paleologan emperors except for John V (which was partially dealt with by Halecki). Nevertheless, since the general histories of Vasiliev and Ostrogorsky, no one has attempted to write what Nicol himself refers to as "a more extended work of synthesis covering the period 1261-1453." Nicol now offers us such a narrative of this fascinating period of Byzantium's last two centuries when the Empire was in full decline, suffering the longest death throes of any state in historical annals. And yet during this period, Byzantium underwent some of the most remarkable social, intellectual, religious, and even political changes or modifications in her long history. In this vast panorama of development, the author has chosen to limit himself to the political sphere and, secondarily, the ecclesiastical. Perforce, therefore, he can treat only cursorily such problems as the causes and effects of the Paleologan Renaissance, discussing other cultural and social problems only as they impinge on the political and ecclesiastical.

Nicol's work is constructed in straight chronological fashion. As may be seen from the documentation, he draws heavily upon secondary sources, very few indeed escaping his sharp eye. Although on occasion he cites primary sources to emphasize or elaborate a point (especially for the reigns of Andronicus II and John V), there is no startlingly original research entailed. The extremely complex series of events is kept in focus by the attractiveness of the narrative style and the skillful organization of the vast material. A reader might take issue with certain emphases or interpretations: despite the avowed emphasis on politics and religion, one could argue that a fuller understanding of the vital cultural and ideological differences separating Greeks and Latins in this period-when the West, after centuries of cultural lag, had finally caught up with and even, in some ways, surpassed the East-would have enhanced the usefulness of the work even more. The unionist councils of Lyons and Florence in particular might have received fuller treatment, especially as to the reasons most Greeks were so fearful, almost paranoid, regarding religious union with Rome.

Nicol is probably right in affirming that Bessarion and Cydones were "in love with Italy before they went there." One might equally stress their preoccupation with the concept of the restoration of the ancient unity of Christendom (compare Loenertz on this). Nicol also perceptively emphasizes the paradox that, while the Turkish peril affected the Byzantines to the point of their becoming "a nervous state," Byzantium, nevertheless, in its wretched condition, was thereby rendered "more fertile in ideas than in the previous two centuries."

A few points or events the treatment of which the reviewer might take issue with: the account of Michael VIII's capture of Constantinople in 1261 might take into greater account the disagreement of the sources and the factor of a possible ruse on Michael's part. Scholars now accept that the Directorium was written rather by Guillaume d'Adam. Greater use might have been made of Weiss's (recent). work on John Cantacuzene's reign, of Vacalopoulos's recent book, of articles of Anastasiou on Athos and Michael VIII, and of Cirac on religious union. More information on Alexander V, the Greek little-known pope, could have come from Syropoulos and other sources. But these are minor points that do not detract from the overall value of the work. Particularly well discussed are the fluctuating fortunes of

Medieval 87

Epirus and Thessaly, the vital point that not all Palamites were pro-Cantacuzene, the surprising increase in the Orthodox world of the patriarch's authority in sharp contrast to the declining power of the emperor, and the author's (partial) rehabilitation of Andronicus II from a virtual "do nothing" historical status. In sum this volume, though offering no really new conclusions, with certain minor qualifications may be said to provide the first synthetic, unified, and well-written narrative of the long and complex Paleologan era. As such it constitutes a highly useful, learned, and admirably detailed delineation of the political and ecclesiastical events of the epoch-especially for those who, unable to read foreign languages, cannot plow through the maze of events in the minutely detailed monographs on the reigns of the many Paleologan emperors.

DENO GEANAKOPLOS
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W. MONTGOMERY WATT. The Influence of Islam on Medieval Europe. (Islamic Surveys 9.) Edinburgh: University Press; distrib. by Aldine-Atherton, Chicago. 1972. Pp. viii, 125. \$5.50.

w. Montgomery watt. The Formative Period of Islamic Thought. Edinburgh: University Press; distrib. by Aldine Publishing Company, Chicago. 1973. Pp. vi, 424. \$12.50.

The Influence of Islam on Medieval Europe comprises a series of lectures delivered at the Collège de France in December 1970. There are eighty-four pages of text, a glossary of English words derived from Arabic, notes, and an index. The lectures make no pretense of turning over new ground but simply review the evidence from the point of view of an Islamicist. Watt is a skillful and accomplished lecturer and so the results are exactly what the series of Islamic surveys was designed to give, an enlightening presentation for the educated reader.

This has been done before, notably in the original edition (Oxford, 1931) of The Legacy of Islam, but that work was the product of many hands, as will be its soon-to-appear second edition. The patient reader might well choose to wait upon that latter work, but in the meantime any student of the subject will be clearly and succinctly instructed and guided by Watt's notes directly into the scholarly foundations upon which these lectures so gracefully rest.

The Formative Period of Islamic Thought is a work of primary scholarship; it is an important study of an important and not much

understood period in the intellectual history of Islam. The book is based, as Watt says, upon a "radical critique" of the sources of Islamic theology up to about 950 A.D. and attempts to present the results of that critique in the form of an evolutionary history of the period. Watt is generally kind to his readers, and here he provides (pp. 1-6) a forthright set of principles upon which he intends to proceed. The need for such flows from the fact that the finished version of the history of Islamic theology was written by theologians whose self-constituted "orthodoxy" had won general acceptance and whose treatment of their predecessors, the thinkers of Watt's "formative period," was chiefly in the form of heresiography.

The results of Watt's "radical critique" is a rich and provocative book, which is the first attempt to explain early Islamic theology on such a broad scale. But one must conclude almost immediately that the heresiographers have not entirely yielded up their secrets. Information still holds its pre-eminence over sense, and many of the heresiographers' lists of names and positions continue to clog the pages of Watt's work without yielding much understanding. Theology still stands isolated from the political, social, and economic evolution of Islam.

Watt is a careful and conservative scholar, and much of his analysis is convincing. Where he fails, I think, is in his scanting of the metaphysics of Jahm ibn Ṣafwān, his ignoring of the ghulāt or "extremist" elements in the early history of Shî'ism, his silence on early Shî'ite kalām, and a totally inadequate account of the origins of Ismā'îlism.

But there is an issue of somewhat larger substance between us. Watt is forthright in conceding that the "Islamic thought" of his title is really Islamic theology. But the exclusion, with simply a passing notice, of other intellectual currents is difficult to defend. The period covered by this book is precisely the time when Muslim life was exposed to great drafts of Hellenic occultism, scholasticism, and science, all of which deeply affected Muslim thought. Watt does speak from time to time of "Greek ideas" or "Greek methods of argument," but he never says exactly what they were, where they came from, or what effect they had on Islam. The omission here is serious, and Watt's pages on "the attraction of reasoning" are among the least successful in the book.

These are simply preliminary reflections. Watt's Formative Period occupies new ground, and only after it has been thoroughly studied

and tested against its own sources will one be able to say how much of it deserves to stand there. For others less specialized, it may well be an interesting example of a discipline emerging from almost utter incoherence into the first outlines of a communis opinio.

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DEREK BAKER, editor. Relations between East and West in the Middle Ages. Edinburgh: Edinburgh University Press; distrib. by Aldine Publishing Company, Chicago. 1973. Pp. 156. \$8.00.

This book, consisting of papers presented at a colloquium at the University of Edinburgh in March 1969, is an excellent accomplishment. This is true despite a somewhat misleading title. The East is in fact the "eastern part of the Graeco-Roman world," with heavy concentration on the Byzantine Empire and with emphasis almost entirely on the dynastic, ecclesiastical, and cultural. The one paper that deals at all with the economic and institutional is limited to Latin Syria. The book is really a collection of monographs (the second section by Karl Leyser, "The Tenth Century in Byzantine-Western Relationships," runs to 19 pages and is accompanied by 12 pages of notes in fine print).

This reviewer is not competent to assess the value of the papers by W. H. C. Frend, "Old and New Rome in the Age of Justinian," and that by Karl Leyser, but I find new material and new interpretations in the papers by R. H. C. Davis, "William of Tyre"; by Anthony Bryer, "Cultural Relations of East and West in the Twelfth Century"; and by Joseph Gill, "Innocent III and the Greeks: Aggressor or Apostle?" The paper by Jonathan Riley-Smith, "Government in Latin Syria and the Commercial Privileges of Foreign Merchants," is full of new approaches to the subject and a mass of information on tolls, taxes, and the funda, information not available elsewhere except in the relevant sources, which he seems to have examined with remarkable thoroughness. The general scholarly reader will perhaps find the papers on "William of Tyre" and the concluding paper by R. W. Southern, "Dante and Islam," especially interesting and certainly more readable. The former presents the thesis that William of Tyre's History Overseas was a "message" his readers never recognized; they read it as an adventure story and he had meant to warn them that Jerusalem could be saved only if the Muslims were

divided and the Christians united. Southern has an interesting hypothesis concerning the place in the Divine Comedy of Mohammed and his son-in-law Ali. He concludes his paper stating that "a few western travellers of Dante's day knew Islam at first-hand and brought back a sympathetic understanding of the Muslim way of life, but Dante was not one of them. He was a wholly western man."

There is an excellent and very helpful tenpage introduction by Donald M. Nicol. And, marvel of marvels for this type of book, there is an excellent index.

SIDNEY R. PACKARD Smith College

FRIEDRICH PRINZ. Klerus und Krieg im früheren Mittelalter: Untersuchungen zur Rolle der Kirche beim Aufbau der Königsherrschaft. (Monographien zur Geschichte des Mittelalters, number 2.) Stuttgart: Anton Hiersemann. 1971. Pp. xxiv, 216. DM 60.

This important study traces the disparity between the officially stated Church position on the participation of the clergy in war and the actual practice of the clergy in military activities between the fifth and the tenth centuries. It is Prinz's thesis that changing societal, political, and military factors in Western Europe, especially the decline of Rome, compelled the Church to accommodate itself to new situations.

Beginning with an examination of the decretals of the fourth-century councils, Prinz shows that all clerical participation in warring activities was forbidden, even the bearing of arms. Yet, as early as the late fifth century, bishops such as Germanus of Auxerre, Hilarius of Arles, and Pope Leo I himself were already involved, by will or by circumstance, in political affairs that carried military responsibilities. An increase in the civil power of the bishops, coupled with the fact that the higher clergy were increasingly of noble background—a process the author aptly calls the "aristocratization" of the clergy—led to the justification of their participation in military functions from scripture and patristic literature. This is all solidly documented with examples from Gallo-Roman territories and in Merovingian times.

This evolving development was accelerated or "institutionalized" by Charlemagne who, in making the higher clergy integral parts of the imperial aristocracy, required them to produce fixed contingents of armed knights for his service. Evidence abounds of clerics participatMedieval 89

ing in Charlemagne's many campaigns, and imperial theologians rationalized this state of affairs by equating resistance to the king with resistance to God. The disintegration of the Carolingian Empire and the international crisis that followed it inexorably led to the increased military responsibility of the higher clergy, many of whom led contingents against the invading Normans and Hungarians. The late tenth century vita of Archbishop Bruno of Cologne, written by the monk Ruotger, depicts him as a model ecclesiastic, a "good shepherd," although Bruno violated every canon of clerical nonparticipation, which illustrates the drastic alterations the old ideals had undergone. By the time of Gratian's Decretum in the middle of the twelfth century, the development of the crusading ideal and the partial militarization of the papacy had brought the relationship of the cleric and war into a new ambient.

While much of this has been known or suspected since the studies of Kleinclausz and Lot, Prinz's contribution is in his careful examination of the social background of the Merovingian and Carolingian episcopates. The book is a soundly researched and authoritative work that students of many areas of early medieval history will want to consult.

BENNETT D. HILL University of Illinois, Urbana-Champaign

STANLEY CHODOROW. Christian Political Theory and Church Politics in the Mid-Twelfth Century: The Ecclesiology of Gratian's Decretum. (Publications of the Center for Medieval and Renaissance Studies, UCLA, number 5.) Berkeley and Los Angeles: University of California Press. 1972. Pp. xi, 300. \$15.00.

Basically this book expounds Gratian's concept of the constitution of the Church as embodied in his *Decretum*, a dialectical legal treatise completed shortly after 1139. Although the work became the standard medieval textbook for canon law, it was studied by scholastic lawyers not for the conclusions of its compiler, but for the multitude of sources he had collected there, from which later canonists drew different conclusions. Modern historians have studied Gratian's own opinions on many particular points, but Chodorow is the first to attempt a general view of the master's ecclesiology.

Necessarily the bulk of this work is descriptive, and the detailed explication of Gratian's doctrine will certainly be welcome grist to the

mill of specialists in medieval political and legal thought. They should particularly appreciate the author's incisive ability to express complex ideas in simple and precise language. I particularly admired the lucid topical organization, which might well be taken as a model for similar studies of other thinkers. Indeed the clarity of this presentation gives it a special value as an introduction to the categories and concepts of medieval political thought.

Chodorow, however, is no mere expositor. He maintains the bold and original thesis that Gratian was the theorist of a post-Gregorian reform party, the typical member of which was Bernard of Clairvaux. The nucleus of this party, which unquestionably did exist during the schism of 1130, was a curial faction headed by the French cardinal Haimeric, papal chancellor 1123-41. A generation ago Hans Klewitz argued that the party flourished throughout Haimeric's chancellorship and that it favored a shift from the old Gregorian stress on the temporal welfare of the Church to a new program of spiritual regeneration. This position was later elaborated by F. J. Schmale, who found that the faction at the Curia consisted of cardinals from France and northern Italy, and its supporters in the provinces were monks and canons regular. Against this proposal Gerd Tellenbach objected that the ideology of the party was not sufficiently distinct from that of its opponents. That deficiency Chodorow now attempts to supply by taking Gratian as the exponent of the party's ecclesiology, though he realizes that Tellenbach's objection cannot be wholly overcome as long as the position of the opposing party remains unknown on crucial issues (p. 246). Nonetheless the author is able to draw repeated and striking parallels between the thought of Gratian and Haimeric's supposed partisans, notably Bernard. These discoveries, I think, will be the seedbed for a fruitful reappraisal of the development and significance of twelfth-century political theology, but in the process I suspect that it will prove wiser to discard as anachronistic the conceptual model of organized political parties with ideologically based programs.

RICHARD KAY
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PERCY ERNST SCHRAMM. Kaiser, Könige und Päpste: Gesammelte Aufsätze zur Geschichte des Mittelalters. Volume 4. Part 1, Rom und Kaiser; Geistliche und weltliche Gewalt; Das Reformpapsttum; Zur Geschichte von Nordund Westeuropa; part 2, Zur Geschichte von Süd-, Südost- und Osteuropa; Zusammenfassende Betrachtungen. (Beiträge zur allgemeinen Geschichte.) Stuttgart: Anton Hiersemann. 1970; 1971. Pp. 432; 439-764. DM 170 the set.

With this volume, completed just before his death in November 1970 (the foreword is dated July 5, 1970), Schramm was able to realize what he must have conceived to be the major goal of his Gesammelte Aufsätze: the presentation in an organized fashion and in an updated version of his massive and diverse writings on the theme of Herrschaftszeichen, Staatssymbolik, and Staatsrepräsentation in the Middle Ages. Whether any of the plans Schramm outlines for future publications (pp. 6-8, 727-33) are realized posthumously, all medievalists can be grateful that they now possess Schramm's final and full position on the broad subject to which his contribution over five decades has been so seminal.

This volume, published in two parts, makes little sense by itself. It can only be judged as a continuation and completion of the three previous volumes (reviewed in AHR, 75 [1969]: 462-63, and 77 [1972]: 127-28). In general the substance is somewhat thinner and more diffuse than that of the earlier volumes, chiefly because Schramm in his effort to present all that he has written on his basic theme moves somewhat far afield from those topics, geographical areas, and eras to which he has devoted his most thorough investigations. A brief outline of the contents of this volume will serve best to indicate what Schramm has done to complete his collection of studies relating to the signs, symbols, and rituals utilized to represent the offices of medieval kings, emperors, and popes.

The first three sections (pp. 19-203) comprise a miscellany of studies on familiar themes: texts concerning Rome and the emperor in the High Middle Ages, aspects of the relationships between "geistliche und weltliche Gewalt," and the ideology of the reforming papacy of the eleventh and twelfth centuries. The major items include a study of eleventh- and twelfth-century Roman literature on the topography and history of ancient Rome (pp. 22-33); a study aimed at elucidating the Dictatus papae by tracing the history of how certain prerogatives of sacerdotium and regnum became intertwined to create key elements of that document (pp. 57-102); some remarks on the history of the papal tiara (pp. 107-12); a study of the "throne of the popes" in St.

Peter's (pp. 113-22); an analysis of the Old and the New Testaments "in der Staatslehre und Staatssymbolik des Mittelalters" (pp. 123-40); and a study of the two fragments of Cardinal Humbert's De sancta Romana Ecclesia viewed as a programmatic statement of the reforming papacy of the eleventh century (pp. 143-64). All of these studies except that on the throne of the popes are revised versions of earlier publications. Interspersed among them are assorted shorter items, chiefly book reviews, related to the major themes.

The fourth section, comprising the largest segment of this volume (pp. 207-631), is devoted to about forty items treating the broad theme of Herrschaftszeichen und Staatssymbolik in "Länder" other than the Holy Roman Empire: the Scandinavian kingdoms, England, France, the Spanish kingdoms, Italy, the crusading states, Hungary, Bohemia, Poland, and Russia. Although conceding a lack of expertise with respect to the histories of these many realms, I feel fairly confident in suggesting that Schramm fails to achieve his usual profundity, intimate contact with the sources, and insight into the fundamental nature of medieval political authority. Consisting chiefly of book reviews and papers presented to conferences, these studies do not effectively convey what one suspects Schramm wished to convey: a sense of the common elements and the local variations associated with the symbols, rituals, and forms utilized to express the political concepts of royal and ecclessiastical offices in the entire medieval world. What Schramm presents, however, suggests that a treatment in depth of this theme on a pan-European scale would be extremely fruitful; as he indicates in his foreword, Schramm had such a plan in mind for himself and his students (p. 7).

The fifth section (pp. 634-733), entitled "Zusammenfassende Betrachtungen," is frustrating. If the reader anticipates a master historian's overarching recapitulation on a theme so massively treated in four volumes and in other equally massive books, he will be disappointed. He will find a strange unintegrated collection of book reviews, unpublished papers, and extracts from previous volumes touching on historical research in general, art history, and "Geistesgeschichte," on Schramm's basic approach to the study of the signs and symbols through which medieval political concepts were expressed, on the matter of Europe and its nations, and on what constitutes the common characteristic of the

Medieval 91

Middle Ages. Yet every page presents a stimulating idea that makes one realize the profound insights that Schramm developed during his long career with respect to medieval civilization, history as a discipline, and the relationship between the past and the present.

In the foreword to this volume, Schramm wrote: "Ich schrieb viel, aber es ist Stückwerk geblieben." I would like to think that this statement absolves him of any obligation to make a meaningful summation of what he has achieved in the first four volumes of the Gesammelte Aufsätze that he obviously wished to be judged as an entity. In honesty, there can be no absolution; these volumes beg for a meaningful evaluation that would provide a key to Schramm's contribution to the study of the Middle Ages. This task, however, lies beyond my competence. Schramm's collection is too varied, too rich in detail, too innovative in approach and methodology, and too profound in implication to permit a yeoman medievalist to comprehend its full significance. Perhaps what is needed is a symposium of scholars from many branches of medieval studies who would focus their varied perspectives on Schramm's total scholarly production in order to render final judgment on what this great scholar has achieved. Such a collective evaluation would, I am certain, conclude that all medievalists are indebted to Schramm for charting a new access route to the medieval world, for delineating the rich meaning of innumerable signposts along that route, and for pointing toward unexplored territory still beyond. If this be "Stückwerk," more of the same kind would be a great gift to all medievalists.

RICHARD E. SULLIVAN
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HELMUT COING, editor. Handbuch der Quellen und Literatur der neueren europäischen Privatrechtsgeschichte. Volume 1, Mittelalter (1100-1500): Die gelehrten Rechte und die Gesetzgebung. (Veröffentlichung des Max-Planck-Instituts für europäische Rechtsgeschichte.) Munich: C. B. Beck'sche Verlagsbuchhandlung. 1973. Pp. xxiv, 911. DM 118.

The first thing that impresses the reader is the courage of the editor in listing and discussing the sources and literature of medieval "private law" in one volume, however formidable. But Coing seems to have been well aware of the risks, and he presents the limits of his enterprise very systematically (pp. 3-15). The main

purpose of the handbook is to serve as an introduction to the civil law of modern Europe. which will be treated in the three volumes to follow. It was necessary to include the later Middle Ages because that was the period when "the foundations were laid for a unity of European law that led to the system of Civil Law proper" (p. 3). These roots of later development are seen in the study of law at the universities, the spread of legal scholarship, and the emergence of a particular group of university-trained lawyers. In this spirit the volume concentrates on the intellectual and institutional developments on the Continent from the twelfth to the fifteenth century, while England on the one hand and the Byzantine-Slavic world on the other are adduced only as comparisons. Only those laws, statutes, and commentaries are discussed that were more or less touched by the impact of revived legal studies: legalistic and canonistic sources and literature, learned commentaries on territorial laws, literature on civil legal procedure, the legislation of later medieval principalities (including the Empire), and the jurisdictional practice of the Rota Romana. Wisely, no attempt was made at a rigid distinction between "public" and "private" law, which would have been anachronistic for the Middle Ages; actually, a whole section is devoted to an overview of royal, ecclesiastic, and corporate institutions (including the public notariate) of medieval Europe (pp. 401-514). Even with all this sensible limitation, the editor and his eight colleagues (Gero Dolezalek, Gunther Gudian, Norbert Horn, K. W. Nörr, Hansjörg Pohlmann, Winfried Truser, Peter Weimar, and Armin Wolf) had to cover an immense field of primary and secondary material. It is inevitable that some chapters are better than others. For example, the section on faculties of law, which logically opens the book, begins with a survey of all medieval universitiestheir history, structure, and legal status. Although this is a very useful and handy guide to which nothing similar exists, it leads somewhat beyond the scope of the handbook. On the other hand, medieval society and economy, hardly a neglectable element in the development of civil law, are each taken care of by a page with haphazardly chosen, very meager bibliographies. The sections on legal matters proper are less uneven, and, if so, for good reason: where up-to-date introductory works are available, for example, on canon law, the handbook can be summary, but where even the basic systematic research is wanting, for

example, on medieval legislation, the authors correctly go into detail.

Besides the excellent lists of sources-many of which are only available in early prints, hence not easily located—and modern literature, every chapter summarizes the present state of research and suggests problems worth studying. There seems to be complete agreement among the authors that the older, somewhat mechanistic notion of "reception of Roman Law," and, even more as it was sometimes put—the idea of "struggle" between the learned laws and traditional custom, need to be basically revised. The authors see the process rather as a part of the intellectual revival of the twelfth century, as an element in the increasingly scientific view of the world in which legal studies added a dimension of new ways of thinking and writing. They also point out that Roman law served everywhere-even, in England, where it has not been "received"-as a "gold mine of ideas and patterns" (H. U. Kantorowicz) that complemented local and traditional laws for solving the questions of a changing society.

Finally, a sincere compliment is due to the contributors for their veritably pan-European, comparative approach: without clouding the differences between the various territories and communities of Europe, they present the major lines of common development with characteristic examples from all parts of the Continent. The Max Planck Institute for European Legal History has done a great service in sponsoring this book and has presented a valuable example of successful teamwork.

JANOS M. BAK
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JOHN H. A. MUNRO. Wool, Cloth, and Gold: The Struggle for Bullion in the Anglo-Burgundian Trade, 1340–1478. Toronto: University of Toronto Press. 1972. Pp. xi, 241. \$20.00.

This excellent study can in some respects be regarded as a sequel to Dr. Thielemans's Bourgogne et Angleterre: relations politiques et économiques entre les Pays-Bas bourguignons et l'Angleterre, 1435-1467 (1966), and is itself complemented by Dr. Spufford's Monetary Problems and Policies in the Burgundian Netherlands, 1433-1496 (1970). Thanks to these three books we can at last really begin to understand how economic and monetary affairs shaped the policies of the Valois dukes of Burgundy and affected the course of events in and around their possessions in the Low Countries.

Munro writes without great elegance of style

but in a pleasant, direct, and straightforward way that enables him to explain the technicalities of a difficult subject with consistent success. He supports his exact scholarship with a copious documentation culled from the archives of the Burgundian mints and other muniments. The only predecessor in English of this wellfounded and original work is L. V. D. Owens's thin and ragged narrative The Connection between England and Burgundy during the First Half of the Fifteenth Century (1909). What astonishing progress in historical scholarship has been made between these two works!

Wool, Cloth, and Gold adds a new, hitherto almost unexplored, monetary and economic dimension to our knowledge of the diplomatic relations of England and Burgundy. It takes an altogether new look at the relations of Richard II of England and Duke Phillip the Bold. It investigates the successive though short-lived Burgundian bans on imported English cloths of 1434, 1447, and 1464 and their effects. It shows how the first of these in particular was not so much protectionist as retaliatory, for it was a countermeasure to English bullionism. Especially notable are the author's perceptive discussions of this protomercantilism, and of other contemporary economic and monetary theories and practices. He goes a long way to describe and explain the transformation of the English and Low Countries industries in the later Middle Ages.

We may hope that much more of the complex story of the decline of English wool exports and of the great Flemish cloth-manufacturing towns, and of the rise of the English cloth industry and of the "new draperies" in the Low Countries, will be elucidated in the author's promised forthcoming volume. He has made an admirable start in this one.

RICHARD VAUGHAN University of Hull

NICHOLAS ORME. English Schools in the Middle Ages. London: Methuen and Company; distrib. by Barnes and Noble, New York. 1973. Pp. xiv, 369. \$25.00.

This interesting study of English schools in the Middle Ages extends from the twelfth to the mid-sixteenth century. In it the author draws attention to his predecessors in the field and particularly to the merits and shortcomings of the chief of these, A. F. Leach. His concern in the present work is with "Education and Society," "Schools and Their Studies," and "Historical Developments." He considers vari-

Medieval 93

ous schools, conditions in which they developed, kinds of people and the variety of interests they served. The most prominent and widespread schools were "public and secular," that is, "'public'" or "not confined to any particular class of persons" but "open to all who could afford to attend them," and "'secular'" meaning they were staffed and attended "by secular priests or clerks, and later on laymen too," rather than by members of religious orders, which maintained their own schools. The author further draws attention to the effects on the schools of the separation from Rome and the Reformation.

According to their studies the schools were song or primary, grammar or secondary, and schools of higher learning. In the primary or song schools were taught the alphabet, basic prayers, and reading in church service books, such as the psalter and the mattins book, and plainsong, possibly also writing. The grammer or second tier schools, not necessarily separated from the song schools, concentrated on the teaching of the Latin language and literature, with special attention to the structure of language and literature. Here we are reminded that the author's sources are drawn largely from the later Middle Ages and may reflect the influence of humanism. Opportunities were also provided for more practical instruction in dictamen, the art of letter writing, keeping accounts, preparing deeds and charters, as well as for studies in French and, after 1949, in English. Higher studies, that is, the liberal arts and theology, were usually pursued in the secular cathedral schools and in the schools maintained by the regular orders. For all the schools treated Dr. Orme provides details regarding the texts used, as well as information relating to their management, daily routine, and the various methods of their support and maintenance. And he follows this by an account of their development historically as to numbers, endowments, and the interest they aroused among their contemporaries. He does not, however, consider the trade schools, Inns of Court, or universities.

Throughout the work Dr. Orme supports his discussion with specific examples and citations drawn from contemporary sources. He also notes that further elucidation of the actual content of the curriculum and definite conclusions as to the extent of literacy must await further painstaking research in the mass of extant literary material available, especially for the earlier end of the chronological scale. In the meantime, we are grateful for the present

study illuminated by photographic illustrations and maps showing the numbers and locations of the schools, together with the detailed listing of all known medieval schools. A bibliography and index complete the study.

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w. L. WARREN. Henry II. Berkeley and Los Angeles: University of California Press. 1973. Pp. 693. \$20.00.

Henry II is one of England's great kings, For thirty-five years he dominated not only his kingdom and its dependencies in the British Isles but also most of western France. England, which gave him his royal dignity, he won by force of arms from his cousin, Stephen. Normandy, with its claims to hegemony over Brittany, his father had won for him, while greater Anjou, which included Maine and the Touraine, he inherited on his father's death, and Aquitaine was his wife's duchy. His "Angevin empire," as modern scholars have dubbed it, was never a real unit, but rather a congeries of separate dominions. As Mr. Warren makes plain in this excellent study of Henry and his reign, a lesser man would have found it impossible to meet the challenges of all these diverse polities. To keep order over so large an area when the fastest method of communication was by horse and the laws were more often than not observed in the breach, to foster peace and maintain tranquillity when force and violence were so common, was to undertake the improbable if not the absolutely impossible. What made Henry great was his ability to realize all his rights and dignities, to keep law and order, and even more, to legislate and to administer innovatively, to put the stamp of his will on the intractable material of his lordships. Clearly he was in the great tradition of the Conqueror, of Henry I, and looking to the future, of Edward I.

How could Henry achieve so much? In the first place, he was very careful to collect all the revenue he could, though not like Richard willing to sell anything at all if he could get a good price. Mr. Warren makes clear how concerned Henry was with revenue, but he does not give us much in the way of figures; quantitative data do not interest him, it would seem. Rather he is more concerned with Henry's use of law as a means to political ends and in its development in England almost as an end in itself. Here he has made a major contribution

and whoever reads this book will understand better the development of the common law. With money at his disposal and law on his side, Henry could employ mercenary soldiers as a police force to keep order and if necessary, destroy the castles of any rebels in all his farflung dominions. Henry was lenient to rebels but not to their castles, which he dismantled at a great rate. Thus Henry reduced the power of his magnates and rejoined their power to his own authority. He was, of course, creating the precedents for the Capetians to use against his sons in order to subordinate the Angevin empire to the French kingdom. In his own time, indeed, his achievements nearly foundered on the rocks of clerical privilege and of filial insubordination. The longest single section of the book deals with Henry and the Church, centered inevitably on the Becket controversy. The account is enlightening in stressing Becket's habit of making grand gestures and Henry's flexibility, learned during the controversy, so that even after the martydom he lost little of what he wanted from the Church. Henry's last years were dominated by the family quarrels that are all too well known. Mr. Warren, one suspects, was a bit bored with the subject and this part of his book is a little less satisfying than the rest. Still, he shows how Henry won time and again until he became too sick in body and spirit to fight any longer.

What manner of man was Henry II to accomplish all that he did? He bemused his contemporaries because he seemed to love paradox, to say things that belied his actions, to do things that belied his words. Even to Mr. Warren he remains a bit of an 'enigma because Henry kept his own counsel. But everyone agrees on the force of Henry's personality, on his incredible energy, his restlessness, his seriousness, and above all, his intelligence. He was courteous, affable, even amiable, if he wished to be, but he could also be very caustic and insulting. His wrath was memorable; his laughter was ready and was as likely to be directed at himself as others; his love was given slowly but he was equally slow to withdraw it; his grief was great. But rarely did he allow his emotions to overrule his thoughts. His court was too sober and serious to appeal to chivalric youths such as his older sons and their friends. His recreations were hunting and reading. He abhorred war and could only be called the aggressor in three of his many campaigns, those coming early in his reign. Mr. Warren shows Henry changing over the course of his life from inflexible youth determined to recover all his inheritance, to flexible maturity attempting to

preserve his lordships. He shows Henry's policies reflecting his advisers. But there is fortunately none of the fashionable Freudianism of psychohistory, and the profitless speculations of Anouilh's Becket or Goldman's Lion in Winter are carefully eschewed. Indeed, Mr. Warren is so little interested in family relationships that his genealogy is not always quite correct. We will learn little here about either hereditary or environmental influence on the formation of Henry's personality. What we will learn is the character of the great king.

This is by all odds the fullest as well as the best study of the life and reign of Henry II yet written. The text amounts to 630 pages aside from twenty-six full page plates and seventy pages of prefatory material, appendix, bibliography, and index. Since the bulk is so great, some potential readers may be intimidated, and it may be worthwhile to say that Callimachus's judgment about big books being big evils does not apply to this one. Mr. Warren knows how to write to maintain his readers' interest. He has something to say and says it well. Altogether his is a splendid addition to what is already a distinguished series.

FRED A. CAZEL, JR. University of Connecticut

JOEL THOMAS ROSENTHAL. The Training of an Elite Group: English Bishops in the Fifteenth Century. (Transactions of the American Philosophical Society, New Series, volume 60, part 5.) Philadelphia: the Society. 1970. Pp. 54. \$2.50.

The election of bishops—in the original sense of the term: their choosing by whatever means—is in any age a central fact for the ecclesiastical historian, and in virtually any medieval context for political and social historians as well. For the former, even if he holds the loftiest views of episcopacy it is clear that the choice of the Holy Spirit is more likely to fall on some than on others; for the latter, patterns of influence and currents of elitism displayed in episcopal choices can illuminate matters far wider than mere church politics.

A commonly held belief about the fifteenthcentury English episcopate is that it was both predominantly aristocratic and skilled in government business. Professor Rosenthal's careful investigation of the backgrounds of the seventynine men elevated to bishoprics between 1399 and 1485 reveals this belief to be only partly true. Less than a quarter were from noble families, a higher figure than a couple of centuries earlier but nothing like a monopoly. More surMedieval 95

prising is the author's finding that a third of the churchmen in question had performed no appreciable government service before becoming bishops; though it is true that the most important sees tended to go to experienced civil servants, and that some of the "nonservers" acquired administrative experience in their universities. Most striking of all is the fact that two-thirds of the bishops had previously held archdeaconries. This is perhaps the clinching argument for an "apparatus for recruitment and selection of the ruling elite" that the author suggests, though without ever clearly demonstrating.

Two biographical tables sum up the research behind this brief but valuable monograph. After reading it one longs for more flesh on the statistical bones, and the title seems a bit misleading: what the work is really about is "the cursus honorum which led to a bishopric" (p. 5). But it is nonetheless useful to have the elements in this cursus laid out so thoroughly and compactly.

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JO ANN MCNAMARA. Gilles Aycelin: The Servant of Two Masters. [Syracuse:] Syracuse University Press. 1973. Pp. ix, 220. \$8.50.

Gilles Aycelin was, as Jo Ann McNamara puts it, "lawyer, councillor to Philip the Fair and two of his sons, president of the Parlement of Paris, diplomat, Archbishop of Narbonne and of Rouen, presiding officer of the papal commission to investigate the Templars, . . . [and] one of the most influential men of his age." One should welcome, therefore, a study of his career, one that has the potential of illuminating many of the still unresolved issues surrounding the personalities and policies of Philip, Boniface VIII, and their successors.

In a sense, Professor McNamara's work stands in a great tradition. At least since the Second World War medievalists have increasingly turned to detailed biographical studies as a vehicle for understanding the realities of their period, and these efforts have provided a refreshing counterbalance to the overwhelmingly institutional and abstracted monographs that preceded them. Late Capetian France has proved no exception to this development, and in this regard one thinks immediately of the publications of such scholars as Strayer, Pegues, and Favier, to mention only a few.

One has to report, regretfully, that the present book does not live up to the promise of

the tradition or of Gilles Aycelin's career. Much of the difficulty lies with the nature of the research actually done. Because personal circumstances prevented archival work abroad, Professor McNamara has had to rely on published resources, and one agrees with her judgment that they are in fact sufficient, providing that everything available is used. But that is not the case here. Of the 224 items cited in the bibliography, only nineteen were published after 1950, and with the exception of one article by Strayer, nothing in it originally appeared after 1969.

As a result, Professor McNamara appears almost totally oblivious to some of the most significant work done in the last generation. Further, even the works cited are employed with an indiscretion that is wholly unwarranted. For example, Lot's and Fawtier's Historie des institutions françaises is footnoted reverently as the final word on any subject under discussion; nowhere does Professor McNamara show any awareness of Fawtier's own cautionary introduction to the second volume in which he makes clear his own reservations about Lot's outdated conclusions, ones that he decided to publish only as an act of piety. And if Lot and Fawtier are overly relied upon, this monograph is ignorant of the latter's inventory of Philip the Fair's documents, a work that would help to plug many of the evidentiary gaps that Professor McNamara is content to slide over with frequent appeals to such formulas as "there can be little doubt that . . ." or "it is possible to imagine that. . . ."

One consequence of these bibliographic failings is an almost total conceptual confusion. At one moment Philip the Fair emerges more pure and saintly than St. Louis; at the next, however, Boniface VIII is so sympathetically treated that Philip quickly reverts to being the "bad guy" of the traditionalists. To cope with such protagonists Gilles Aycelin must have had even more talents than Professor McNamara is endlessly willing to bestow on him.

Combine these defects with reasoning that is evasive and shaky, and terminology that is sloppy and misleading, and one does not end up with a book whose contribution to scholarship is significant. One can only hope that the Syracuse Press will exercise greater discretion in future.

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CHRISTIANE PIÉRARD. Les plus anciens comptes de la ville de Mons (1279-1356). Volumes 1 and 2. Brussels: Académie Royale de Belgique, Com-

mission Royale d'Histoire. 1971; 1973. Pp. xlvi, 785; 213, 5 plates.

For historians concerned with medieval institutional, economic, social, and demographic history the records in this volume constitute a treasury of information on medieval urban institutions, particularly financial; on trade, industry, and transport; on construction techniques, implements, and skills of workers; on urban planning and expansion; and on the names and occupations of those living in Mons, the principal town of the county of Hainaut. Interesting, too, is that these records span the years between 1279 and 1356, a period that saw a leveling off and contraction of the medieval economy; a series of floods, famines, and epidemics that preceded the Black Death; and the opening phases of the Hundred Years' War.

The records of Mons, edited here for the first time, are among the earliest and best preserved of the urban financial records of the Low Countries. They consist of three categories. Most valuable are the comptes en rouleaux of the massarderie consisting of an annual accounting by the massard (the collector of urban revenues) to the échevins. Closely associated with these are other special accounts submitted to the massard by subordinate officers responsible for various administrative functions such as public works, payment of rentes and pensions, collection of taxes, and public assistance that were often appended to the account of the massard and served as pièces justificatives. A third category, the accounts of the échevins, consists of a statement of the financial situation of Mons and was rendered yearly on June 24 by the échevins whose term had expired to their replacements. Until 1338 these accounts were enrolled on pieces of parchment sewn end to end, a technique similar to that used by the English chancery in its enrollment of such records as the charters, letters patent and close, and writs of liberate. In 1338 they began to be placed in registers written on paper, a system continued to 1602. That so many of these records have been preserved is due to the good sense of the responsible officials who made one or two copies of each account.

With these accounts now published historians concerned with urban history can compare the urban administration of Mons with that of the Flemish towns of Ghent, Ypres, and Bruges, whose financial records have been edited by J. Vuylsteke, G. Des Marez, E. De Sagher, G. Wyffels, and J. De Smet. Besides yielding details on urban finance and economic activ-

ities, these accounts also illuminate the relations of Mons with the counts of Hainaut. By the late thirteenth century rarely did a comital official interfere with the internal government of Mons, which was under the control of an independent échevinage. This urban independence continued until Hainaut came under the control of Philip the Good of Burgundy, who then reimposed central control over the towns and forced the échevins to render annually a financial accounting to ducal officials.

Written in the Franco-Picard language and tinted with a large number of local words, these accounts should also be of interest to the philologist. Study of the vocabulary of Montois laborers of the nineteenth and twentieth centuries shows it similar to that of their ancestors in the thirteenth and fourteenth centuries. A glossary in volume 2 of words used most frequently lists, for example, the word acheter appearing as acater, accater, or akater, and the word cinq as chiunch, chiunc, ciunch, ciunq, or chienc.

This is an excellent edition and, as one has come to expect, is superbly printed by the Commission Royale d'Histoire.

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HENRI HUGONNARD-ROCHE. L'œuvre astronomique de Thémon Juif, maître parisien du XIV° siècle. (Centre de Recherches d'Histoire et de Philologie de la IV° Section de l'École pratique des Hautes Études. Fifth Series, Hautes études médiévales et modernes, 16.) Geneva: Librairie Droz. 1973. Pp. 429.

This book is a study of the life and works of Themo Judei, a very learned Parisian master of the fourteenth century, and it is accompanied by a detailed analysis of Themo's Questions on the Sphere of Sacrobosco and Question on the Motion of the Moon. The book also includes a critical edition of the latter, established from the three extant fourteenth-century manuscripts. This study, undertaken under the guidance of Guy Beaujouan, earned its author the title of "Élève diplômé de la Section des sciences historiques et philologiques de l'École Pratique des Hautes Études." It exhibits the same merits and drawbacks as a good American Ph.D. dissertation in the history of medieval science. It is scholarly, competent, and learned, and it shows that Mr. Hugonnard-Roche has mastered the techniques of study of medieval manuscripts and has otherwise learned the historian's trade. It also contains little mistakes Medieval 97

of, first, interpretation, second, of information, and third, and the most damaging of all, of misplacing numerous anachronisms and anachronistic transcriptions of medieval rhetorical discussions of mathematical topics into the modern language of symbolic algebra. Examples of mistakes of interpretation are that Aristotle's views on vision seem to be reduced to an intromission visual rays theory (p. 26) and that Themo's method is repeatedly and unwarrantedly called "experimental" (pp. 36, 48, 53, and elsewhere), while, strangely enough, on page 195 it is stated that "la conception de la causalité exposée par Thémon conduit donc à refuser toute valeur à l'observation ou à l'expérience et apparaît incompatible avec la formation d'une science expérimentale!" Examples of the second kind of mistake made are, first, that the hypothesis that all medieval references to Apollonius's Conics come from the Arabic translation is accepted uncritically (p. 32) and, second, that De crepusculis et nubium ascensionibus is mistakenly ascribed to Alhazen (p. 36).

As to the third area of mistakes made, Henri Hugonnard-Roche seems much too eager and overly zealous to translate ancient and medieval descriptions into symbolic language without further ado. Furthermore, he does not seem fully aware of the potential historical dangers involved when this ahistorical procedure is used indiscriminately. Thus, to mention only two examples (though instances literally abound), in speaking of Themo's proof of the "Merton Rule," which follows exactly William Heytesbury's proof in the Probationes conclusionum tractatus solvendi sophismata, Mr. Hugonnard-Roche says that "ce raisonnement est essentiellement une opération de calcul infinitésimal qui pourrait s'écrire: . . .  $dS_c/dt - d(S_a/2)/dt$ =  $d(S_a/2)/dt - dS_b/dt$  . . ." (p. 233), and that "ce raisonnement tout intuitif est, ainsi que le note M. Clagett, une opération de calcul infinitésimal" (p. 235). The reasoning in question is, most obviously, not a calculus operation for Themo, but it is indeed an intuitive operation, which is not the same thing!

There are also anachronisms in interpreting medieval concepts. Again a couple of examples will suffice: where Themo speaks of magnitude, the author interprets this as meaning mass (p. 88); and, on page 177, the scholastic distinction between virtus fatigabilis and virtus infatigabilis foreshadows for the author the concept of energy!

The two treatises analyzed by Mr. Hugonnard-Roche, though outwardly astronomical in character, are the product of a very widely read author who was not an astronomer, but a natural philosopher. They do not deal, therefore, with the main problem of astronomy, namely, saving the celestial phenomena as accurately as possible by means of an appropriate geometrical model. Themo accepted without any critical discussion Ptolemy's theory as the best geometrical representation of celestial appearances, and he tried to establish in his Questions on the Sphere of Sacrobosco the points of agreement, as he saw them, between the Ptolemaic hypothesis and Aristotelian physics. In this investigation Themo's sympathies lie squarely with the Stagirite.

Themo's positions, furthermore, are very close to those of the famous late scholastics. His dynamical ideas are indeed very similar to those of Albert of Saxony and Nicole Oresme. The doctrines expounded in *Questions on the Sphere* are quite often identical to Pierre d'Ailly's ideas in his own *Questiones in sphaeram Johannis de Sacrobosco*, in which d'Ailly might have used Themo's work. It is, however, impossible to establish accurately Themo's place among his contemporaries before reliable, scholarly editions of Oresme's *Juvenilia* are published.

The Question on the Motion of the Moon is a philosophical quodlibetal question, patterned after the older theological cuodlibetal questions. Indeed, like the latter, it contains both a disputatio and a determinatio. Dealing, atypically, with very recent sources (the works of the Mertonians, written between 1328 and 1350), it represents a transcription of a summary made by Themo in the wake of a public disputation that took place at Erfurt in 1350, and it is an interesting testimony on the teaching and organization of the sciences in the middle of the fourteenth century.

The issues discussed in it have to do with a basic cosmological topic in Aristotle, namely the intrinsic analysis of local motion as it applies to celestial motions. The question specifically asked is, does the moon move uniformly or not, and if not, how? Uniformiter difformiter or difformiter difformiter? Themo transfers Bradwardine's version of Aristotle's dynamical law to the kinematical domain. Furthermore, his ways of treating intensities of qualities, borrowed from the Mertonians, enable him to perform some remarkable mathematical feats: the summation of an infinite number of parts of a continuous magnitude by means of the summation of a decreasing geometrical progression and the enunciation and proof of a number of kinematical propositions that, when represented graphically (which Themo does not do), lead most naturally to Oresme's famous diagrams.

It is clear that Themo's methods represent a softening and mellowing of Aristotelian categories, and they prepare the way for considering velocity as an entity in its own right. Also, speaking explicitly in terms of spaces and times rather than forces and resistances (a procedure made necessary by the fact that there are no forces and resistances in the heavens) can be seen as a necessary step toward the unification of celestial and terrestrial motions under the same law. With Oresme, Themo is one of the first on the Continent to employ the results and methods of the Mertonians.

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JEANNE LAURENT. Un monde rural en Bretagne au XV° siècle: La quévaise. (École Pratique des Hautes Études—VI° Section. Centre de Recherches Historiques. Les hommes et la terre, 14.) Paris: S.E.V.P.E.N. 1972. Pp. 440. 82 fr.

Expanding her thèse for the École des Chartes, Mlle. Laurent presents a valuable and comprehensive study of the quévaise land tenure system found in Bretagne as well as a description of the rural world in which it existed. She also publishes the texts of the documents used to define the quévaise and to discover why it developed and how it operated. The most valuable aspect of this book, however, is its demonstration of methodology. With great skill and creativity Laurent shows how much information can be gleaned from relatively few documents that are fully exploited. From the existent material she constructs a picture of rural life and shows how the law governing the quévaise worked itself out in practice from the fifteenth through the eighteenth century. She painstakingly supports each of her points with relevant examples from the documents and indicates how her conclusions differ from those of other historians.

The quévaise itself was a form of land tenure found primarily in the holdings of the Cistercian and Hospitaler orders located in Basse Bretagne. Laurent forcefully demonstrates that the holder of a quévaise retained considerable freedom vis-à-vis the other tenants in the region. He could be deprived of the quévaise only if he was absent for a year and a day, and the obligations to his ecclesiastical lord were relatively light and easily commuted into money payments. Laurent claims that the quévaise developed in the thirteenth century to encourage

men to settle on monastic holdings in order to clear new lands for cultivation. The settlers were given a plot of ground and a house free of taxation as a base from which to work, and they could pass the holding on to their heirs according to the right of juveignerie. Laurent maintains that the juveignerie functioned well in the quévaise because the frontier situation allowed the older sons to acquire their own quévaises nearby. She also shows that while quévaises existed to the Revolution, from the sixteenth century many quévaisiers attempted to use their unique freedom of tenure to identify themselves as part of the feudal structure.

Laurent's book is particularly valuable as a source of information about research methodology to graduate students preparing to work in rural history. The book will also interest historians seeking additional insight into the development of Breton rural institutions and those individuals who enjoy seeing another scholar do her job very well.

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FRANK D. PRAGER and GUSTINA SCAGLIA. Mariano Taccola and His Book De Ingeneis. Cambridge, Mass.: MIT Press. 1972. Pp. xii, 230. \$17.50.

Mariano di Jacopo, surnamed "Il Taccola" (ca. 1382-ca. 1455), of Siena was an artist and notary who was famous in his day as now for his drawings and descriptions of technical devices. From the many Taccola manuscripts Frank Prager and Gustina Scaglia have reconstructed two treatises, De Machinis 1449 (1971) and De Ingeneis, completed in 1433. Mariano's works are especially significant as sources for the history of Italian technology-and art-in the century that culminated with Leonardo da Vinci. Prager and Scaglia have collaborated before to illuminate portions of this landscape. Their study of Brunelleschi (1970) showed a felicitous combination of the skills of the historian of technology (Prager) and of art (Scaglia). Scaglia independently published De Machinis in 1971. Here the authors treat Taccola's life and work, his position as an artist, the roots of De Ingeneis and the work's relation to De Machinis and to later copybooks. Their reconstruction of the autograph text, which is now divided between Florence and Munich, is a masterful piece of careful historical craftsmanship.

Yet, to this reader, their treatment of the treatise represents a serious disappointment. Although their scholarship is up to its usual

high standards, Taccola is a work gravely flawed by its format. The book as a technical product undermines the quality of its contents. Some questions by way of illustration: Was Taccola originally intended as an edition of De Ingeneis? If so, why is approximately half of the original manuscript omitted? If it was intended as a study, why is Mariano's original (and sometimes jumbled) order of drawings preserved? Why do the authors repeatedly refer to drawings by Taccola that are not published here or elsewhere? Why are folios from the original reproduced in Lilliputian format, making details of the drawings indistinct and lines (indeed, whole pages) of text illegible? What possible economies are effected by such measures when pages with figures are left half blank? In short, what went wrong with this work between conception and publication?

We would seem to have here an example of a publishing dilemma. Given that facsimile editions of source documents are needed badly in many fields of historical scholarship and that sharply rising publication costs threaten to price such volumes out of their intended market (at \$100 and up per volume!), then we should ask what sort of compromises should be made in order to make important source documents available. Perhaps MIT Press is to be commended for its concern with this problem, but if Taccola is any example, surely the cure is worse than the disease. Compromises that vitiate the scholarly purpose of a work, and threaten to drive serious readers to madness, through illconceived and awkward formats are unacceptable. Some other solution will have to be found.

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## MODERN EUROPE

JOSÉ ANTONIO MARAVALL. Estado moderno y mentalidad social (Siglos XV a XVII). In two volumes. Madrid: Ediciones de la Revista de Occidente. 1972. Pp. xiii, 529; 619.

José Antonio Maravall is a professor of political science at the University of Madrid and Spain's leading student of political ideas and theories for the late medieval and early modern periods, primarily from the fifteenth through the seventeenth centuries. The two volumes under review constitute a massive synthesis and compendium of the new ideas concerning policy and state structure in Western Europe during that period, together with the new concepts that emerged

concerning social roles and organization. The work is devoted exclusively to theories, ideas, and presuppositions; it is not a study of empirical history that analyzes the course of events or the chronological development of certain institutions in specific states. The field of study includes all of Western Europe, but the primary focus is Spain, particularly in volume 2, which deals with more specific ideas of government organization and functioning.

Perhaps the most interesting and original part of the entire work is the second half of volume 1, "Power, Individual, Community." In this section Maravall analyzes the new concepts of state power, sovereignty, and the claim to "absolutism." He agrees with many other students that the West European "absolute monarchy" was not really absolute in the sense of total institutional power, but he defends the continued use of the theory since this terminology was employed in the sixteenth and seventeenth centuries and corresponded to the pre-eminent political sovereignty of the new monarchist state. Maravall discusses the preconstitutional framework of "fundamental laws" that was accepted by all Western states at that time, while emphasizing the vagueness of such a corpus. Other chapters deal with the parallel emphasis on property rights, individual liberty, and the sense of nation and community that accompanied development of the early modern state.

There is not very much in these two volumes that may be considered novel or specifically original. The discussion of economic attitudes and policies in volume 2 is merely a rehash of what is already known by students of Spanish economic history in this period. However, the erudition in secondary works, literary sources, and contemporary works of law and political theory is immense. Taken simply as a study in ideas, Maravall's new work may serve as a useful compendium of new social and political concepts during the period of transition from the late Middle Ages. The author's assertion that "this book probably presents a new way of seeing the history of Spain" (vol. 1, p. 5) is considerably exaggerated, but his product will be a useful reference for the new trends in fifteenth- and sixteenth-century theory.

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SERGIO BERTELLI. Ribelli, libertini e ortodossi nella storiografia barocca. (Biblioteca di storia,

6.) [Florence:] La Nuova Italia. 1973. Pp. xviii, 377. L. 3,600.

In this lively and interesting volume Bertelli, a distinguished scholar in the field of Renaissance and post-Renaissance studies, concerns himself with the evolution of historiography from about the third decade of the sixteenth century until approximately the end of the seventeenth century. He calls this period baroque and defends this use of the term in the foreword.

In a world founded on religious faith and organization, with attendant superstition and authoritarianism, the development of a methodology on the part of writers on whom we would confer the title of historians was slow and laborious by necessity. It is this process of transformation that Bertelli considers in his study with copious documentation. He believes that the history of this historiography can be written only after well-documented observations are studied and discussed by specialists. He hopes that his work will be thought-provoking and will start fruitful discussions.

Bertelli's point of departure is the anti-Renaissance, that is, the reaction to the Renaissance, which created a new set of values, dictated by both the Protestant and Catholic revolutions. Machiavelli, and also Guicciardini, were rejected due to their rationalism and their lack of religiosity, and because their world had abruptly ended in 1530 with the defeat of the Republic of Florence and the affirmation of the supremacy of Spain in Italy. Their contribution was to become vital again a century or more later. For the moment the principle of authority prevailed in all aspects of life and it became inevitable that the tendency to codify and dogmatize everything should also affect historiography.

Bertelli's work is divided into two parts, each covering in general the same period of time but from different perspectives. Indeed, this method is followed also in the chapters into which each part is divided.

In the first part our author concentrates on religious history, in the second on the history of different peoples, nationalities. He shows how certain lines of force emerge and develop from clashes between groups attempting to defend, support, or justify opposing points of view and power supremacy. The clash between Protestant and Catholic historical writings, at the beginning largely based on assertions, later leads to the rediscovery of the importance of documents in supporting the arguments pre-

sented. Still later, recognizing but unwilling to admit the rightness of certain affirmations of the opposition, the clash loses aggressiveness and leads to research of an erudite kind. A similar cycle occurs in the historiography that the French, English, and other peoples develop in order to demonstrate their nation's complete independence from foreign influences, particularly from ancient Rome and Renaissance Italy.

Rebels, orthodox, libertines are terms used by the author to indicate succinctly the interplay of the various forces from which modern historiography will develop.

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JAMES M. STAYER. Anabaptists and the Sword. Lawrence, Kansas: Coronado Press. 1972. Pp. ix, 375. \$10.00.

Having published a number of articles in the last ten years describing the positions taken by various Anabaptist sects on violence, authority, and church-state relations, Professor Stayer has now put together his conclusions in this book. The main problem in writing such a book is how to reconcile, within the confines of a single movement, such wildly divergent standpoints as the chiliastic zealotry of a Thomas Müntzer, the Realpolitik of a Balthasar Hubmaier, and the apoliticism of a Menno Simons. The difficulty is compounded by the vastly differing milieux in which Anabaptism flourished: Switzerland, southern Germany, Bohemia, the Netherlands, almost the entirety of the possible political spectrum.

Stayer attacks the problem in the only reasonable way, by treating the various Anabaptist movements separately, without attempting to impose an artificial doctrinal or organizational unity upon them. He also keeps scrupulously to the middle ground between those who, basing themselves chiefly on the testimony of the Anabaptists' enemies, depict them as dangerous fanatics, and the writers of partisan martyrologies. Making use of a wide variety of sources, Stayer has produced what will surely be for some time to come the definitive treatment of all shadings of Anabaptist positions on worldly authority. It can be said, however, in criticism of his achievement that he has defined his task too narrowly. One finds almost everything about the subtlest and most arcane differences between the opinions of the various sectarians, not nearly enough about the contexts of these differences. One has the feeling, at times, that one is reading a taxonomy of doctrine, whose compiler is somewhat indifferent to both the generalized socioeconomic upheaval of the sixteenth century and the overall irenic tradition. There are curious omissions from the bibliography: Peter James Klassen's The Economics of Anabaptism, 1525–1560 and Leonhard von Muralt's Glaube und Lehre der schweizerischen Wiedertäufer, to cite but two, both works that might have added depth to the treatment. In sum, Stayer's book falls rather short of its goal.

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HARVEY MITCHELL and PETER N. STEARNS. Workers & Protest: The European Labor Movement, the Working Classes and the Origins of Social Democracy, 1890–1914. Itasca, Ill.: F. E. Peacock Publishers. 1971. Pp. v, 250. Cloth \$6.00, paper \$3.95.

This slender volume is an interesting publishing experiment. Harvey Mitchell and Peter Stearns have each contributed less than booklength essays taking divergent approaches to the study of the West European labor movement in the decades before World War I and then responded briefly to each other's analyses. Their debate is prefaced by Robert Wohl's critical introduction. Most of the key problems in late nineteenth-century labor history are raised in the course of these essays and this fact, along with the historiographical conflicts they pursue, will make this book a popular choice for classroom use.

Mitchell's essay alone would justify adoption. In one hundred pages he presents an excellent outline of the principal trends in the ideological and organizational development of unionism and working-class politics in Great Britain, France, and Germany from 1890 to 1914. Of necessity there is a certain amount of oversimplification (especially in his picture of the global economic development in each nation), but I know of no summary in English of the formal labor movement that captures the story with such clarity and brevity. There is no question, however, that Stearns is quite correct in terming this "conventional" labor history. The subject matter with which Mitchell deals—the growth of national political and union organization, the pronouncements of leaders, the resolutions of national congresses, the rhythm and nature of major strike activity-is well worn and time honored. His general theses, (1) that differences in national modernization processes make it virtually impossible to analyze the European labor movement as a unified whole during this period and (2) that the apparent trend toward reformism everywhere in Western Europe after 1900 (England possibly excepted) was rooted in the timidity of leadership and the bureaucratization of structures, are again unoriginal.

Stearns, on the other hand, is seeking to develop a long-needed new perspective on the labor history of the period. He wants to penetrate the world of working people themselves and to reconstruct labor history "from the bottom up." He also feels that with such an approach, one may discover more easily comparable trends in the entire West European labor movement and therefore begin to move away from national histories toward a more integrated view of labor's experience as a whole. In short, he is aiming at a broader social history of the working classes and an understanding of its implications for the formal labor movement. All of this deserves unqualified praise. But Stearns goes well beyond this methodological challenge. Freely admitting that the study of working-class social history is in its infancy, he nevertheless proceeds to offer this core interpretation: that a majority of workers in this era were social and political moderates (and became more so with the passage of time), and it was this fact, not the opportunism, irresolution, or bureaucratic mentality of their leadership, that pushed the movement toward reformism and even toward an accommodation of industrial capitalism. Such an interpretation was, of course, not uncommon at the time. Most capitalists wanted to believe it, and Lenin's opinion that the workers in advanced capitalist countries would only develop a "trade union consciousness" became one of the key ingredients in his theory and strategy of revolution in Russia. The question of what workers really wanted in this crucial age is undeniably one of the most important a historian of the modern world can ask. Unfortunately Stearns falls far short of proving his thesis. My immediate expectation was that he would produce a host of examples drawn from local studies, histories of individual unions and their locals, contemporary local newspaper accounts, local party and union meeting records, trade journals, court records, the fairly numerous worker autobiographies, diaries. and letters that date from this period. But little evidence of this sort seems to have been used. In all fairness, it is possible that the brevity demanded by the format of this book caused him to delete

much material of this order. His case is left to rest upon evidence already generally familiar to us. He comes closest to developing a convincing argument in discussing strike goals, which indeed were generally specific, short range, and unideological. Still, Mitchell points to political strikes in various countries and both agree that many British strikes in the years just before the war can be viewed as quasi-revolutionary. But there remains a more general problem. What is, after all, the function of a strike? Its very nature and the normal circumstances under which it occurs all but assure that it will be a pragmatic instrument for achieving immediate goals. Overall, then, Stearns's stirring call for approaching labor history from below remains an important contribution. But to draw general interpretations—especially one of such significance—from the skimpy evidence currently available is surely premature.

It must be said, finally, that both Stearns and Mitchell are hardly dogmatic in maintaining their respective positions. The dialogue is clearly open-ended, and we can look forward to many more years of research and debate on the issues raised in this book. There is one area, however, where both seem to think that the debate is closed—that the West European labor movement became less militant as 1914 approached. Wohl is concerned that such a thesis, if carried too far, will seriously distort our understanding of the radicalization that accompanied and followed World War I and the Russian Revolution because these external events are surely not the only causes of the later process. I can only echo this concern and hope that the intensive study of labor history at the grass-roots level and of the actual relationship between workers and their leaders will shed the needed light on this and the other basic questions posed by these essays.

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A. A. GUBER et al., editors. Rossiia i Italiia: Materialy IV Konferentsii sovetskikh i ital'ianskikh istorikov, Rim 1969. Russkii i ital'ianskii srednevekovyi gorod; Russko-ital'ianskie otnosheniia v 1900-1914 gg. [Russia and Italy: Material from the 4th Conference of Soviet and Italian Historians, Rome 1969. The Russian and Italian Medieval City; Russian-Italian Relations, 1900-1914]. (Akademiia Nauk SSSR, Institut Vseobshchei Istorii, Institut Istorii SSSR.) Moscow: Izdatel'stvo "Nauka." 1972. Pp. 476.

This volume contains the proceedings of the Fourth Conference of Soviet and Italian His-

torians held in Rome on October 25-26, 1969, which considered "mediaeval" Russian and Italian cities and Russo-Italian relations, 1900-14. The book contains three papers on each topic with a variety of commentaries ranging from critical evaluations through short research essays to occasional remarks. The session on cities was the better of the two, offering a comparative typology for Italian urbanization, a study of the conflict between the nascent bourgeoisie in sixteenth-century Russian towns and political centralization based on feudal relationships, and an analysis of governing structures in medieval Novgorod. Overt ideological questions played almost no part, though the common philosophical base for Soviet historical research showed clearly in the papers and comments, as did the range and depth of Soviet scholarship on non-Russian subjects. The twentieth-century session was less satisfactory. Assiduous readers will find nuggets of fact concerning diplomacy, trade, or Italian public opinion, but the main problems discussed were Lenin's "fatalism," varying perceptions on democratic revolutions, and data supporting Lenin's assessment of capitalist development, imperialist competition, and the dynamics of international relations. On balance, the conference was probably most valuable to the people who participated in it. The papers add little more than volume to what is already available in the published literature, though Professor Ernesto Sestan's typology re-emphasizes the importance of comparative analyses, and N. E. Nesov's essay on sixteenth-century Russia would be useful to scholars, who are not familiar with the Soviet literature, studying early European state-building processes. But this raises a broader issue. The tactical advantages of an accepted historical schema that asks the same questions about different historical cultures, thereby generating masses of mutually reinforcing data, become dramatically obvious from the Soviet contributions to this book. Similarly, the potential for distortion, always an important factor in such monolithic systems, is clear, particularly in the Soviet essays on the twentieth century. This fact should not, however, be considered grounds for dismissing Soviet primary scholarship out of hand. Non-Soviet Russian historians, though often critical of Soviet method and historical philosophy, have gained much from Soviet research, and now there is an established and growing corpus of Soviet work on non-Russian subjects that deserves to be more widely known. Given the comparative context for the conference, it was certainly pure cultural chauvanism

to publish this volume in Russian rather than one of the more widely accessible European languages, but this only underlines the need for non-Slavic specialists to consider adding Russian to their arsenal of scholarly tools, while routinely reviewing Soviet contributions to their respective fields. To do otherwise is to ignore a burgeoning historical literature, or worse, to reject it without critical examination.

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KARL J. NEWMAN. European Democracy between the Wars. Translated by KENNETH MORGAN. [Notre Dame, Ind.:] University of Notre Dame Press. 1971. Pp. 475. \$12.00.

A number of interesting and important questions are raised in this book: the central fallacy of the myth on which democracy is based, the fundamental conflict between equality and liberty; the possibility of its functioning in any but a small community; and the impact of the growth of numbers and of an increasingly complex technology on the organization of the state. Within this framework, the failure of the democratic experiment in the Central European milieu, the German most of all, constitutes the focus of the analysis.

The rash of post-1918 democratic constitutions, most of them modeled on the French, held in it the seeds of failure, for these constructions were suddenly imposed upon societies in which they lacked roots. There was also the problem of ethnic minorities in most of the new states; self-determination, the foundation on which their existence rested, was in large measure denied to the newly created minorities.

In these circumstances authoritarian rule in varying degrees was the alternative solution. Italian fascism was the first model, but far more threatening was German nazism to which the impact of economic crisis gave the opportunity. This new development gave rise to two questions. One was the threat inherent in renewed German power; the other derived from the fact that nazism as a philosophy, a view of the organization of the state, was applicable to milieux other than the German. But here in turn a contradiction was created by the narrowly nationalistic and racial content of nazism. The effect was confusion. To turn German minorities in other states into fifth columnsthe Sudeten case is the outstanding examplewas relatively easy; but where could inferior Slavs turn who might agree with other aspects of the Nazi creed?

These are the questions discussed in perhaps unnecessarily repetitious and lengthy detail. If the analysis is sound and contains valuable observations, some caution seems warranted in the use of this book. Some of the interpretations are questionable, not to mention careless editing, typographical and other errors. To present the Ruhr episode of 1923 as almost the result of a conspiracy between Poincaré, Mussolini, and Stinnes calls for stronger evidence. And the version presented of Benes's role in conveying information to Stalin is, to say the least, unorthodox. Certainly, it is not what one finds in Churchill's wartime memoirs, cited as evidence, incidentally with an incorrect reference. In sum, a useful book but also a mixed bag.

RENÉ ALBRECHT-CARRIÉ Columbia University

c. J. C. MOLONY et al. The Mediterranean and Middle East. Volume 5, The Campaign in Sicily, 1943, and the Campaign in Italy, 3rd September 1943 to 31st March 1944. (History of the Second World War, United Kingdom Military Series.) London: H. M. Stationery Office; distrib. by Pendragon House, Palo Alto, Calif. 1973. Pp. xix, 921. \$42.00.

No theater of operations in World War II was the subject of so much debate and, in the postwar years, of as much controversy as the Mediterranean. In the context of the cold war and of Soviet expansion, it seemed that Allied wartime strategy in the Mediterranean should have been directed, as Churchill supposedly urged against strong American opposition, toward the Balkans, the "soft underbelly" of Europe. This view is largely fiction. The British never seriously proposed such a strategy; the differences between them and the Americans were not over basic strategy but over priorities and timing. On the necessity for an invasion of northwest Europe there was no disagreement.

This volume, though it touches on these questions, deals primarily with operations. Fifth in the six-volume official British history of the war in the Mediterranean and Middle East, it opens with the invasion of Sicily in July 1943, the first large Allied landing on the Continent, and then describes in six chapters the subsequent surrender of Italy and the hardfought thirty-eight-day battle to secure that mountainous island. The invasion of Italy by way of the Straits of Messina and the landing at Salerno early in September raised the curtain on the long and arduous campaign up the Italian boot to the Winter line and the great Ger-

man fortress at Cassino. There the Germans held fast, foiling the Allied amphibious encirclement at Anzio, until March 1944 when Cassino finally fell before repeated Allied assaults. The drive to Rome, the move north through the Gothic line to the Po Valley, and the surrender of German forces in Italy in May 1945 are to be covered in the concluding volume of the series.

The decision to go to Sicily was made at Casablanca in January 1943, at the height of the struggle for North Africa. It was reluctantly agreed to by the American planners who not only feared that this move would divert forces from the main effort in northwestern Europe but also suspected British motives in pressing for further action in the Mediterranean. But there was no help for it. The invasion of France was at least a year away and the troops that had driven the Germans and Italians from North Africa could hardly be allowed to remain idle. There were other reasons for moving ahead in the Mediterranean, not the least of which was the advantage to be gained by opening the area to Allied shipping. Subsequent meetings of the Combined Chiefs in Washington and Quebec during May and August of the same year confirmed this decision and authorized such other operations as would contain the largest number of German divisions and take Italy out of the war. Such operations, it was understood, were to be subordinate to and supportive of the main effort in France. It was with this understanding that the invasion of Sicily was carried out and the campaign in Italy undertaken.

Fortunately Hitler proved most cooperative. He, like Churchill, had his eye on the Balkans, and on October 4, whether because of his fears of an Allied invasion as this volume suggests or for other reasons, he ordered Kesselring to stand firm instead of withdrawing slowly, as he had been doing. He was to hold the Allies in place, Hitler told him, and in the event the Allies showed any sign of crossing to the Dalmatian coast, he was to attack forthwith. Up to now small German forces had been giving ground slowly, skillfully utilizing the difficult mountainous terrain to tie down Allied forces. The effect of this new order was to commit a larger number of German divisions to the Italian front, troops that could have been used in Normandy, thereby ensuring the success of Allied strategy.

This is a large volume, almost one thousand pages with maps, plates, and tables to match. One would hardly expect that so large a volume on so thin a slice of the war, one so burdened with the imprimatur of official history and an impossible title (really a nontitle), and written by a team of military men from different services could hold the attention of any but the most dedicated specialist. Yet it does so. It is lucidly written, in simple and at times moving prose, remarkably free of jargon, judicious in tone (as in the treatment of General Montgomery), and well paced. The authors have a keen eye for terrain, the ability to explain intricate matters clearly, and are equally at home with the decisions of the Combined Chiefs and the actions of small units along the Winter line or amphibion operations off the coast of Sicily or Anzio.

Unlike the Americans, the British tell the story of the military side of the war from the defense rather than the service level. The result is a comprehensive and total view of the war that is largely lacking in the separate U.S. Army, Navy, and Air Force series. In one respect, however, the American volumes are superior: most of them are fully documented and include extensive bibliographies. The British decision to omit documentation, understandable perhaps at an earlier period, is hardly defensible now, thirty years after the war. But even with this limitation, this volume with its predecessors is likely to remain the standard and definitive account of the British side of the struggle in the Mediterranean, a monument to the industry, devotion, and skill of its authors.

LOUIS MORTON

Dartmouth College

RALPH MERRIFIELD. Roman London. New York: Frederick A. Praeger. 1969. Pp. 212. \$9.50.

TIMOTHY BAKER. Medieval London. New York: Praeger Publishers. 1970. Pp. 260. \$11.95.

MARTIN HOLMES. Elizabethan London. New York: Frederick A. Praeger. 1969. Pp. 123. \$5.95.

The deplorable lack until recently of good general histories of London is being dealt with by several enterprising publishers bringing out whole series of period histories of the metropolis. The volumes under review place particular emphasis on London's physical structure and appearance and especially on those material remains that can still be seen. Each could serve as an admirable guidebook for the historically minded tourist determined to discover what underlies the present city.

Determination is necessary, for little of the London of the millennium and a half that preceded the Great Fire is any longer there. London's very success has obliterated its past. Even had there been no fire, it is doubtful that we would have much more left of medieval and Tudor London than we do; while land values and the zeal of building developers being what they are, London by 1974 would probably look much as it does had not a single bomb fallen on it in 1940. The skills of the archeologist and the knowledge of the museum curator need to be combined with the imagination of the historian to re-create the vanished city. The three authors, respectively assistant director of the Guildhall Museum, editor of the Victoria County History of Middlesex, and an authority on Elizabethan costume and armor, combine the specialist's obsession with detail and the historian's search for broader relevance.

In Collingwood's terms they raise what might have been mere chronicle to the level of history by discerning the thought contained within the material shell. In their careful descriptions of buried foundations, fragments of pottery, coins, household implements, artisans' tools, street patterns, and monumental inscriptions they never lose sight of the question: what does it all mean? What, that is to say, can the scattered remnants of London's past tell us about the life lived in its buildings and the values that informed their builders? Their degree of success in finding satisfying answers serves as a challenge to the modern urban historian to make better use of his eyes: if so much can be learned about the life and nature of Roman, medieval, and Tudor London from cooking pots and tessellated pavements, from the foundations of vanished roads and artifacts dredged from the bottom of the Thames, how much more can we learn about Georgian and Victorian London, large chunks of which stand staring us in the face, not as antiquities arranged in display cases, but as integral working parts of today's metropolis!

Of the three, Merrifield's volume on the Roman city has the most to offer the specialized scholar. Unlike the other two it is provided with footnotes and serves as a useful guide to the recent archeological literature on the many discoveries being made in the city. Baker and Holmes provide less that is new, but, given the more abundant written evidence for their periods, they are able to give fuller descriptions of London as a functioning entity. Baker makes excellent use of William Fitz Stephen's vivid if adulatory description of the London of Thomas Becket, while Holmes finds in Elizabethan dramatic literature numerous passages

that illuminate the urban experience. The many illustrations contribute a great deal to an understanding of the text of all three.

In his recent inaugural lecture at Leicester University, Professor H. J. Dyos argued that "the authentic measure of urban history is the degree to which it is concerned directly and generically with cities themselves and not with the historical events and tendencies that have been purely incidental to them." Merrifield, Baker, and Holmes meet his criterion, and if their treatments pay less attention to London as a part of the larger European economy or as a laboratory for the examination of demographic, social, and economic change than many urban historians today would, their London is never merely the stage on which the play is performed, but the play itself. They treat, again in Dyos's words, "the city as such, the whole interlocking apparatus without which urban life could not function, the relations between generations of buildings and generations of men." And while they do not neglect, quoting Baker, "the dirt, the stench, the noise, the hectic indulgence, the violence and the suffering," they also account for the fierce devotion that London, then as now, inspired.

The three volumes represent the best sort of high popularization, and they combine the virtues of the old urban history that moved from monument to monument, inscription to inscription, literary association to literary association with the more recent concern with the city as central to an understanding of the total human experience.

DONALD J. OLSEN Vassar College

PAUL CERNOVODEANU. England's Trade Policy in the Levant and Her Exchange of Goods with the Romanian Countries under the Latter Stuarts (1660–1714). Translated by MARY LÄZÄRESCU. (Bibliotheca Historica Romaniae, Economic History Section. Studies, 41[2].) Bucharest: Publishing House of the Academy of the Socialist Republic of Romania. 1972. Pp. 156. Lei 8.25.

Paul Cernovodeanu presents a thorough picture of late seventeenth-century English trade with the Black Sea areas that presently constitute Romania. His study is based to a large extent on Savary's Dictionnaire universel de commerce (1726) and modern published works—to which his notes form a valuable guide. Cernovodeanu did some work at the British Museum and Pub-

lic Record Office in London and evidently exhausted the limited manuscript sources in the state archives of Bucharest, Braşov, Sibiu, and Cluj. The latter relate almost exclusively to trading conditions in Transylvania.

After an initial Marxian gloss on the nature of British trade with the northeastern Balkans in the period, Cernovodeanu rightly concludes that the English policy of trade expansion, while consistent with England's general ends, was largely a function of the needs of the Levant Company. The latter sought trade connections in the Black Sea area because its transit trade in oriental spices had been pre-empted by the East India Company. Cernovodeanu was evidently unable to search the Chancery Masters' Exhibits in the Public Record Office; had he done so the merchant records there would have borne out his argument.

Despite their desire the Levant merchants were never able to make direct contact with the Black Sea lands because of the restrictionist policy of the Porte. Instead, Greek Orthodox merchants of various Balkan ethnic groups did penetrate the area and then resold the goods to English merchants at Constantinople. This penetration by merchants from the south was facilitated by the difficulties that Eastland and other British and West European merchants encountered in their established trades with the area, principally in potash, because of the political disintegration of Poland.

After reading Cernovodeanu's copiously documented argument one is left wondering why the study was undertaken, at least with its English emphasis. Despite the fascinating picture of internal trade developments that he presents, direct English trade was virtually nonexistent by the author's own account and was about to undergo a complete eclipse. English trade with Wallachia, from the Baltic or the Levant, dried up in the period under discussion. Transactions with Moldavia and Transylvania were directly in the hands of non-English merchants. Finally, the market for England's chief export commodity, cloth, was an extremely limited one, consisting of the few towns of the region and among a few highly placed families.

JAMES E. FARNELL
Roosevelt, New Jersey

ERIC KERRIDGE. The Farmers of Old England. Totowa, N.J.: Rowman and Littlefield. 1973. Pp. 180. \$10.00.

This is an important, perhaps even a revolutionary book. Professor Kerridge believes that

the English agricultural revolution was pretty well completed before 1760, the traditional date for its beginning. He has written three books to support his views. The very first sentence in his most important work, The Agricultural Revolution, published in 1967, reads: "This book argues that the agricultural revolution took place in England in the sixteenth and seventeenth centuries and not in the eighteenth and nineteenth." The volume presently under review, The Farmers of Old England, contains much that is repetitious of the earlier book, but is a slighter work and attempts to be more popular. Less than half the length of the earlier work, it contains no footnotes or bibliography, both of which are very impressive in The Agricultural Revolution. The present work devotes more space to the way of life of the actual farmers and to the social and economic impact of the technical changes.

The heart of Professor Kerridge's thesis is found in his chapter entitled "The Great Inventions." Most important was the introduction of "up-and-down husbandry," which "consisted essentially in alternately ploughing grassland up for corn and laying cornland down to grass." This change resulted in greatly enlarged crops, and hence great profits, but it took many years and considerable capital. Almost equally important was the "floating of watermeadows," a highly technical process, illustrated by several elaborate diagrams, which made possible much larger sheep herds. The introduction of various new crops and grasses constituted a third major technical change.

These technical improvements were widespread by 1700 and led to specialization in crops and livestock and stimulated manufactures and trade. Technical improvements, specialization, and commerce all involved capital, hence larger farms. Inevitably many small farmers became wage laborers, but Professor Kerridge argues that such changes meant an improvement in the standards of living for the vast majority. English society lacked any rigid castes. Rather it was like moving freely up or down the rungs of a ladder. Professor Kerridge's whole thesis is closely argued and supported by much solid evidence. Its acceptance would mean a drastic revision of the traditional interpretation of the agricultural revolution.

The book is difficult to read, for it is filled with many archaic terms of the agricultural vernacular. A glossary would be useful. The book contains a long chapter analyzing the forty "farming countries" into which the author divides England, and which are determined by

such basic agricultural characteristics as soil, climate, crops, and livestock. The frontispiece is a map showing the "countries."

JOHN G. GAZLEY

Dartmouth College

ERNST SCHULIN. Handelsstaat England: Das politische Interesse der Nation am Aussenhandel vom 16. bis ins frühe 18. Jahrhundert. (Veröffentlichungen des Instituts für Europäische Geschichte Mainz, number 52.) Wiesbaden: Franz Steiner Verlag GmbH. 1969. Pp. xi, 390. DM 60.

An amazing feat of compression is represented in this detailed analysis of British pamphlet literature from More through Defoe about trade and the interest of state. Bibliographers should note the 26-page appendix that lists in chronological order the full titles of the relevant pamphlets. The text manages to comment on the views of most of these, as well as on the secondary literature evaluating them. The prodigious compactness of this achievement might make it seem especially irrelevant to criticize the author for what he has not done. Yet the state of scholarly debate about mercantilism makes it necessary to warn that there is less in this book than meets the eye. Mentioned in the study are most of the scholars who have written about mercantilism, but there is too little dialogue about the fundamental issues raised by them. The basic question of the relevancy of mercantile theories when compared to commercial practices is dealt with too slightly by Schulin. The reader will not find in this study a continuation of the great debate about whether or not there ever was a coherent system of mercantilism or only theories rationalizing unsystematic expedients. Therefore, the author is not able to be convincing that it is worth his time and ours to lead us through the ponderous arguments of the pamphlet literature he explicates. His basic thesis about the importance of the ideological debates fails to command compelling interest when compared with the thesis that most of the theories of mercantilism may have veiled an unsystematic maze of vested interest rather than interest of state. Schulin's central claim about the significance of his study is that it traces the common thread of political lobbying by the business interests who, after 1640, had enough confidence in their political influence to feel that they could impose their commercial views on their government. Schulin's Continental perspective is most valuable when he contrasts this

self-confidence with the very different attitudes of commercial classes in other European countries. This comparative approach is most welcome in light of the insular point of view that still characterizes too much scholarship about British history. But a comparative perspective, broader than Schulin's, was brought to the study of mercantilism long ago by Hecksher, Keynes, Schumpeter, de Roover et al. Schulin begs the question of major debate with such writers by claiming that his study assumes familiarity with the economic and political history of Britain between 1500 and 1715. But readers well read in those areas will realize the need for more skepticism than Schulin displays about the possible discrepancies between theory and practice. Schulin does provide insightful evaluation of some scholarly controversies that enter his review. The secondary literature is best weighed in his discussions of the free-trade controversy of 1604, of the debate over a rising or falling gentry, of the passage of the Navigation Acts. The author does depend on more than the pamphlet literature itself when he traces shifts in the power base of the landed gentry, commercial classes, and banking interests. In his discussion of these shifts Schulin is concerned with issues other than mercantilism, to be sure. But debate about mercantilism remains fundamental to the materials Schulin chose to survey. He has avoided too many of the issues of that debate.

DAVID CLARK
Hope College

CONRAD RUSSELL, editor. The Origins of the English Civil War. New York: Barnes and Noble. 1973. Pp. x, 286. \$12.00.

In this new volume in the Problems in Focus series, specialists set forth trends and interpretations of recent research in their fields and suggest future areas for inquiry. The articles do not deal with the period beyond the outbreak of the war in 1642, nor is the war regarded as inevitable. For the most part the contributors deal with subjects long considered fundamental for understanding the war's origins: government, central and local; Parliament and the king's finances; Puritanism and Arminianism; fear of popery; and two conflicting cultures. There is no article on the rise or fall of the gentry since, as Russell explains in his introduction, "social change explanations of the English Civil War must be regarded as having broken down." He suggests, however, that a profitable area in social history to explore

further would be the growing importance of the upper yeomen and their relation to the gentry.

Unafraid to agree with Clarendon that human beings loom large in historical explanations, these modern historians pay serious attention to the policies, tactics, ideas, behavior, blunders, and misunderstandings of leaders on both sides. Although long-standing causes are not ignored, the discussion begins essentially in 1625, not in 1603 or earlier. Unlike James, who at times realized what the traffic would bear, Charles, from the beginning of his reign, seemed to have no sense of the public pulse and reaction to his policies. His support of Buckingham in a reckless and disastrous foreign war; of Laud in his Arminian policies; and his stubbornness in employing doubtful financial and monopolistic devices and embarking upon an unpopular Scottish war, left the monarchy by 1640 "discredited and isolated," even from "its natural friends." Laud was perhaps more blind and dogmatic than Charles, for he unceasingly pushed Arminian doctrines and practices, unconcerned that he was alienating loyal Calvinist Anglicans, who, as Tyacke convincingly demonstrates in his article, had formed the backbone of the church under Elizabeth and James. "Thus the connection between Puritanism and revolution was largely of Charles I's [and Laud's] making."

If Charles and Laud failed miserably to understand the deep-seated reactions of men of their own time, Tyacke, Clifton, and Thomas succeed admirably in revealing to us how seventeenth-century Englishmen actually felt. Why Arminianism was repulsive to men still loyal to king and church; why popery was regarded as "the debasement of Christ's teaching,"—more damnable than any form of paganism"; why an elite court culture smacking of Catholicism aroused the violent feelings expressed in the Root and Branch petition are topics explored in depth by these writers.

The politics, maneuvers, and ideas of the Parliamentary opposition receive due attention. With the information now available to him from the Bedford manuscripts, Russell brings out the leading role played by Bedford, the patron of Pym.

Russell also emphasizes the significant part, too often neglected, played by the peers. He makes a clear distinction between the more responsible Parliamentary leaders, who recognized the king's financial problems, and the mere critics of Charles in 1628. In 1641 the more responsible leaders, Pym, St. John, and Bedford, desired governmental office "not so

much for changes in the constitution, as for changes in policy." Russell and the other contributors agree that any revolution came not from below, but from above, from the new policies Charles pursued against a conservatively minded opposition. Whether the term revolution should be used before 1642 may be questioned. If, however, their general thesis is correct, as I believe it essentially is (in the short run at least), then it follows that the English situation in this period resembles the continental in some respects, as Eliot points out in his article on England and Europe.

Chapters on Scotland and foreign relations have, as Russell states, regrettably been omitted. Clifton suggests that a new study of foreign affairs in Charles's reign, not undertaken since Gardiner, might well illuminate the timing of local anti-Catholic riots. It is unfortunate that in the planning of the book, more space was not allotted to the years 1640-42. The point is made that the men who assembled at Westminster in November 1640 were staunch monarchists who were desirous of reform, and not of war or revolution. We still need to know, however, "by what political processes" these reformers eventually came to fight the king. Mendle makes a brave beginning to clarify this problem in his discussion of the politics and political thought of these two years. His treatment of the largely traditional political thought is more successful than his handling of politics. Here, perhaps owing to the few pages allotted him, it is difficult for the reader to see a clear picture or analysis. Perhaps we need first, as Russell implies, an up-to-date narrative account of these years.

The articles are uneven, those by Russell, Tyacke, Clifton, Thomas, and Eliot are most rewarding to this reviewer. The other articles, nevertheless, make substantial contributions and must not be overlooked. There should be no doubt after reading this book that the search for the origins of the English Civil War will continue to be a fruitful subject for future research and debate.

MARGARET A. JUDSON Rutgers University

J. R. JONES. The Revolution of 1688 in England. (Revolutions in the Modern World.) New York: W. W. Norton and Company. 1972. Pp. xx, 345. \$11.95.

Recent books by John Carswell and J. R. Western have signaled a new interest in the Revolution of 1688, which for the past twenty

or thirty years has been played down in favor of the Great Rebellion and Interregnum of 1640-60 (called by many a "Revolution," though none of them have been able to justify the use of this term). But the English-speaking peoples on both sides of the Atlantic owe their present constitution and their frame of politics to the men of 1688, not the men of 1642, or 1649. Professor Jones makes this point with considerable emphasis, and though it may seem obvious enough, to some it is clearly not, and it is agreeable to have it restated so authoritatively and cogently.

However, his main interest lies not in the aftermath of the revolution, but its actual mechanics, and his chapters on the working of politics, the first year of the reign and the Catholic problem are fresh and stimulating, though they need to be compared with Professor Western's account of the same matters, and the last might be modified and expanded in the light of John Miller's new study of English Catholicism in this period. On the key question as to whether James would have succeeded in securing an amenable Parliament had William not invaded he is more optimistic than most of his predecessors. Again, his portrayal of the king as an energetic and competent electioneer is provocative and stimulating, but in view of the defective nature of much of the evidence and the fact that on this unresolved question many contemporaries were of two minds, his conclusions are rather overstated. Nevertheless, he is right to try to dispel some of the anticipatory gloom with which most historians have approached this monarch's activities.

His general interpretation, particularly of William's motives, is firmly Whiggish and conventional, as was Western's. They are probably right, though again the evidence is not such as to permit any final verdict, and both, I think, play down too much the dynastic motive that was a very important element in William's thinking at all times. Certainly it is difficult to understand Jones's animus against Lucille Pinkham. Her neo-Jacobite interpretation was certainly exaggerated, but it is by no means entirely untenable, and her work should not be neglected by any serious student of the revolution.

J. P. KENYON
University of Hull

WOLFGANG JÄGER. Politische Partei und parlamentarische Opposition: Eine Studie zum politischen Denken von Lord Bolingbroke und David Hume. (Ordo Politicus, number 15.) Berlin: Duncker & Humblot. 1971. Pp. 296. DM 56.60.

Historians and theorists of parliamentary government have found it easy to read back the last century's developments too far into the past. Dr. Wolfgang Jäger's monograph, which does not seem to have received the notice that it deserves, may be regarded as a corrective to this tendency. He has sifted carefully through the writings of two important pre-Burkeian theorists on his subject and brought their opinions on it into focus.

The first of these, Lord Bolingbroke, does not come off very well under this scrutiny. He is pretty well shown to have been an overambitious, violently emotional aspirant to political power, who set himself up as a great theorist, picking and choosing among the ideas of greater minds and providing grist, indirectly, for later thinkers because Montesquieu saw something in his concept of separation of powers. Incidentally, Dr. Jäger suggests that he had little idea of separation of powers as now conceived. H. T. Dickinson, whose biography of Bolingbroke (1970) evidently appeared too late for Dr. Jäger to use, thinks that Montesquieu may have misunderstood him anyway or got the idea elsewhere (p. 306).

By contrast David Hume, when he could free himself from a bent toward utopianism, was a serious and deliberate thinker, and saw things, on the whole, pretty much as they were. Ideally opposed to parties altogether, he recognized them as an indispensable feature of political life and fitted them into a soberly calculated scheme of things, involving a governing king and a regular parliamentary opposition, kept in a minority by the use of influence. His ideas ought to have had an important influence on the course of party evolution, but apparently they received serious attention only from the fathers of the American constitution.

Dr. Jäger has made a distinctly worthwhile contribution. Especially with reference to Bolingbroke, his meticulous survey of the many writings by other authors brings the subject together quite well. He demonstrates that Bolingbroke and his opposition party screamed against Walpole's destruction of the constitution without any clear idea that the House of Commons could be put on a permanent peaceful basis of opposition between "ins" and "outs." Hume, of course, set his mind to working out a practical system of balance and control. But he, too,

had no conception of the opposition as an "alternative government."

What it all comes to is that, although Dr. Jäger supports the theory that thinkers influence not only other thinkers but also the course of events (p. 31), the general tendency of his discussion points in the opposite direction.

CHESTER H. KIRBY Brown University

JOHN G. GAZLEY. The Life of Arthur Young, 1741-1820. (Memoirs of the American Philosophical Society, volume 97.) Philadelphia: the Society. 1973. Pp. xvi, 727. \$10.00.

Every schoolboy knows that Arthur Young started life as a Suffolk farmer, soon went bankrupt, and thereafter made his living and reputation by instructing Englishmen on how to succeed at farming. Among the valuable achievements of Professor Gazley's definitive biography, dispelling this myth is a notable one. Young became the squire of Bradfield Hall, Suffolk, in 1785 and staved there until his death in 1820, cultivating a large farm himself and renting two farms to tenants. Bradfield thus remained the cherished center of an extraordinarily active life that took him, as an itinerant agronomist, to all corners of the English countryside as well as to Ireland and France and, as secretary of the Board of Agriculture, regularly to London.

What historians, especially agricultural historians, will miss in Professor Gazley's book is an assessment of Young as an agronomist. His agricultural writings are usefully summarized, including his many contributions to the Annals of Agriculture. From this it is clear that Young was a skilled and indefatigable publicist. But it is less clear how adequate he was as an agricultural expert. Some modern agricultural historians, notably Eric Kerridge, have a low opinion of Young's technical competence; they take sides with his contemporary and rival, William Marshall, whom they rank higher than Young. The merits of their case might profitably have been examined.

On the other hand Professor Gazley's wonderfully detailed account of Young's personal life more than compensates for this omission. In a labor of many years he has sought out every trace of his hero. What impresses most in his evocation of the man is Young's obsessive quality. He had more than his share of disappointments and tragedies: marital wretchedness, a cantankerous son, the untimely death of a be-

loved daughter, and his own blindness in his later years. But he never faltered: the flood of written words went on almost to the end, as did his attendance at the Board of Agriculture. He was the sort of man who, in winter as in summer, rose at four o'clock in the morning and plunged into a nearby pond. On his wife's tombstone he inscribed that she was the daughter of John Leigh of Norfolk, the first "who there used marl." Possibly he had nothing else to say about the poor woman; on the other hand he was not likely to miss a chance to celebrate the glories of agricultural innovation.

DAVID SPRING

Johns Hopkins University

The Correspondence of Edmund Burke. Volume 9. Part 1, May 1796-July 1797, edited by R. B. MCDOWELL; part 2, Additional and Undated Letters, edited by JOHN A. WOODS. Chicago: University of Chicago Press. 1970. Pp. xxviii, 487. \$21.50.

With the exception of an index volume the Burke correspondence is now complete. Scholars who have used previous volumes will find this volume familiar, although of less value than some of the earlier ones. Now in declining health Burke still delivers his opinions on topics that interest him, especially Ireland and revolutionary France, with his usual vigor and point of view. Since Burke was no longer directly involved in public affairs the letters have little to contribute to the history of the period. A few reminiscences, especially of his role in economical reform, shed light on the great events in which he was once engaged.

Presumably this review concludes a series that began with Walter Love's review of volume 1 (AHR, 64 [1959]). In his review Love praised the high quality of the editing and notes and looked forward to the scholarly benefits expected to flow from a complete edition of several thousand Burke letters, most of them unpublished. Now that the series is complete it is clear that the highest editorial standards have been maintained throughout, but the value of the correspondence to the historian is less obvious than it seemed to Walter Love in 1959. The editors have cleared up a mass of detail about Burke's personal and family life, but Burke's public career and private affairs were in such incongruous contrast that one wonders what contribution this mass of biographical information can make to historical understanding. The Burke correspondence adds many bits and pieces to the political history of the period, but it does not bring important new insights nor does it open the door to significant new studies. The correspondence contributes something to understanding Burke's positions on the great issues of his time-America, Ireland, India, reform, the French Revolution—but the major sources for his ideas on these topics are still The Parliamentary History and his Works. On Burke's development as a political philosopher the correspondence has little to contribute. Professor Copeland and his dedicated crew have done their work well and have elucidated many aspects of Burke's long and active career, but it is difficult to see what new impulse the completion of the correspondence will give to studies of Burke and his time. Rather, this superb edition of Burke's correspondence may well mark the culmination and conclusion of the "Burke revival" that began about twentyfive years ago.

E. A. REITAN
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MICHAEL BROCK. The Great Reform Act. London: Hutchinson University Library. 1973. Pp. 411. Cloth £4.50, paper £1.95.

Michael Brock's study of the 1832 act and the events that led up to it has been over a decade in coming, but it is worth the wait. Not that Mr. Brock has anything new to say. Neither G. M. Trevelyan nor J. R. M. Butler would find much to quarrel with in this volume, but for the last twenty years or so the conclusions of the so-called Whig historians have been out of fashion. A new study was necessary to vindicate the good sense of the past over the nonsense of the present. Mr. Brock does just this, brilliantly and exhaustively.

The first chapters deal with the disintegration of the old political system after the death of Lord Liverpool and the growth of pressure for reform, which began in earnest with the general election of 1830. The mounting agitation after the election and the influence of the July Revolution and of industrial and agricultural unrest are carefully traced. Mr. Brock leaves us in no doubt that public agitation was critical in launching the bill and in every stage of its subsequent history, nor that its Whig framers saw it as a necessary concession to public opinion. Equally clear is that the Whigs firmly believed that future political stability depended upon appeasing the middle classes and bringing them within the political system. The Whigs also believed that the result

of such reform would be to re-establish the authority of the natural leaders of society, people like themselves. Reform would restore confidence in the political system and allow aristocratic administrations to practice those arts of government that only they fully understood.

Were the Whigs right? This is a very difficult question, and perhaps not one to be answered by the historian of the Great Reform Act. The pressures that Grey's government perceived acting upon them and the solutions they devised are one question, the effects of the resulting act another. Mr. Brock has exhaustively investigated the first question. If he has missed a significant monograph or an important collection of manuscripts, it is one unknown to this reviewer. Few probably would quarrel with his broad conclusion that the act was a crucial determinant in the relatively peaceful progress of political reform in Victorian Britain. But why this was so will probably be debated for some time to come. Mr. Brock seems to believe that the answer lies in the re-establishment of a deferential attitude among the middle-class electorate. But the Whigs never talked of deference, but rather of the influence of property. It is unlikely they ever expected the new electorate to defer to that influence where its own immediate interests and prejudices were involved. They may have been surprised by the extent to which postreform politics were dominated by just such questions.

RICHARD W. DAVIS
Washington University

DAMIAN MCELRATH, O.F.M. Richard Simpson, 1820–1876: A Study in XIXth Century English Liberal Catholicism. (Bibliothèque de la Revue d'Histoire Ecclésiastique, number 55.) Louvain: Bureau de la R.H.E., Bibliothèque de l'Université. 1972. Pp. xix, 163. 200 fr. B.

FREDERICK J. CWIEKOWSKI, s.s. The English Bishops and the First Vatican Council. (Bibliothèque de la Revue d'Histoire Ecclésiastique, number 52.) Louvain: Bureaux de la R.H.E., Bibliothèque de l'Université. 1971. Pp. xxvii, 341. 500 fr. B.

These books are works of sclid scholarship. Damian McElrath has given us a portrait of Richard Simpson, the man, as well as a more complete account of Simpson's liberal Catholicism than heretofore available. Next to Acton he was the leading figure of the English liberal Catholic movement of the mid-nineteenth century. In addition the versatile Simpson was a

competent historian and Shakespearean scholar. His wide range of interests included theology and philosophy, geology, the classics, and architecture. Cwiekowski's book is a detailed study of the English Roman Catholic bishops at Vatican I. Both scholars have made good use of unpublished sources in relevant archives and manuscript collections. Among other sources Cwiekowski drew material from various diocesan archives in England.

McElrath's book begins much better than Cwiekowski's. He leads the reader right into his subject, whereas the first section of Cwiekowski's introduction, "England in the Nineteenth Century," seems unnecessary for the range of readers likely to be attracted to his subject. The early chapters in McElrath's book are among the most interesting. He relates Simpson as a young man to Simpson later in life as a liberal Catholic; for instance, a connection is made between Simpson's lack of moderation in his essays as an undergraduate at Oxford and his later writing for the liberal Catholic periodical, the Rambler. McElrath uses, among other sources, manuscript material in the public library at Mitcham (where Simpson was raised and where he became an Anglican vicar before his conversion to Roman Catholicism) to provide interesting new background and detail about him. He emerges from the pages of McElrath's book as an attractive personality, albeit one with considerable pugnacity in controversy. John Henry Newman and Simpson disagreed over the latter's theological writing in the Rambler and some other matters, but they remained on cordial terms and regarded each other highly. Again, despite many disagreements with W. G. Ward, a leading ultramontane, Simpson personally liked him. And notwithstanding their squabbles during their association with the liberal Catholic Home and Foreign Review, the successor of the Rambler, T. F. Wetherell and Simpson respected each other. So, too, Cwiekowski indicates that disagreements in ecclesiastically related matters did not necessarily preclude feelings of respect. Although Bishop T. J. Brown of Newport had delated Newman's article, "On Consulting the Faithful in Matters of Doctrine," to "Rome, he later spoke highly of Newman's Letter to Pusey (1866), and he wanted Newman to attend the Vatican Council.

One wishes McElrath had elaborated upon the following statements he made: "Their [Acton's and Simpson's] Liberal Catholicism anticipated by a hundred years present currents within the Roman Catholic body" (p. xviii) and "It is surpising [sic] how many of the questions over Catholic issues undertaken by the Liberal Catholic Simpson, convert and layman, anticipated by a century debates presently raging in the Roman Catholic body between liberals and conservatives. In fact, some issues already anticipated by Simpson have yet to appear in the contemporary arena" (p. 157). But one wishes, too, that the expression, a "siege mentality," had not been used several times in the book; it has become a cliché.

Cwiekowski's study shows clearly that the extreme position of Archbishop Manning regarding the pope was not representative of the English bishops generally. Most English bishops were among the moderates and the minority at the Council. Besides Manning, a main figure at the Council, Bishop Ullathorne of Birmingham and Bishop Clifford of Clifton emerge as the most significant English bishops at Vatican I. Ullathorne, ultimately a moderate member of the majority, is shown to be a man of good sense and practical ability. Clifford was an active figure in the minority, many of whom opposed a definition of papal infallibility as inopportune. Cwiekowski describes the English bishops' involvement in the work of the Council, debates in the general congregations, deputations, suggested revisions of schemata, and so forth. Besides various archival materials, he apparently knows the published works relevant to his subject. Though his knowledge is impressive, the book seems somewhat too heavy with details; a judicious pruning would have been useful. With its complex details, sometimes bulky footnotes, and lengthy quotations some readers may feel a bit of the tiredness that bishops must have felt as the days and months at the Vatican Council went by. It would have been an aid to some readers to provide English translations of the quotations from texts and speeches in Latin at the Council.

Both McElrath and Cwiekowski have striven to give fair and balanced views of their subjects, and they have largely succeeded. McElrath obviously admires Simpson, but he is critical where need be. Cwiekowski gives a scholarly, fair-minded presentation of the English bishops at the First Vatican Council and of Acton, Newman, and others concerned.

WILLIAM J. SCHOENL
Michigan State University

DENNIS G. WIGMORE-BEDDOES. Yesterday's Radicals: A Study of the Affinity between Unitarianism and Broad Church Anglicanism in the Nineteenth Century. Foreword by ALEC

VIDLER. Cambridge: James Clarke and Company. 1971. Pp. 182. £2.10.

An apologetic purpose informs Mr. Wigmore-Beddoes's little essay: to demonstrate the affinity between liberal Anglicans and Unitarians a hundred years ago may clear Unitarians today from the label of heresy and so help to restore the influence they have lost in this century. One wonders if a present-minded activity like theology today will be any more affected by historical demonstration than, say, politics is. Even granting that, would much positive result emerge from Mr. Wigmore-Beddoes's wistful descriptions of the influence of Coleridge, Blanco White, and James Martineau on a few Anglican contemporaries?

But the book is less about apologetics than about history, and here there are questions, too. The chapter titles are indicative: "The Affinity shown" with respect to higher criticism; biblical inspiration, miracles, everlasting punishment, the atonement, and Christ's divinity; traditional language, liturgical practice, and architectural style; mutual interaction; and the idea of a broad church. We are shown briefly, and not unusefully, what leading (not necessarily representative) Unitarians thought on these subjects and then what leading Broad Churchmen thought—a catalogue raisonnée with no surprises; the common characteristics are seen in the end as a commitment to reasonableness, a desire for comprehension, and a similar "moral and religious sensitivity." In such a catalog so much must be compressed that disagreements about interpretation are inevitable—a broader look at stylistic development or a study of the commissioning of particular churches would, for example, almost certainly alter what is said about architecture. But a more serious difficulty lies in the limitation of the study largely to the generation of James Martineau. Thus, like most Unitarian historians, Mr. Wigmore-Beddoes misses the strife-torn development of Unitarian thought in the nineteenth century-and so the perspective needed to measure its "radicalism," its relevance, and its influence.

R. K. WEBB
American Historical Review

PHILIP WARNER. The Crimean War: A Reappraisal. New York: Taplinger Publishing Company. 1973. Pp. 232. \$9.95.

R. L. V. FFRENCH BLAKE. The Crimean War. (Concise Campaigns, 1.) [Hamden, Conn.:] Archon Books. 1972. Pp. x, 181. \$9.50.

The Crimean War is enjoying a revival triggered over two decades ago by Cecil Woodham-Smith's Florence Nightingale and the more sensational The Reason Why, recently made into the movie The Charge of the Light Brigade. In 1961 Christopher Hibbert produced The Destruction of Lord Raglan: A Tragedy of the Crimean War and in 1970 came John Selby's well-illustrated Balaclava: Gentleman's Battle. In 1968 the memoirs of surgeons George Lawson and Douglas A. Reid were made available, to mention but a few items. Thus the question arises, do we need any more books on this limited mid-nineteenth-century war between the great powers? The answer is yes only if they really have something to contribute to our knowledge of the conflict. In this case the answer is basically no.

This is not to say that these books are not useful popular accounts that undergraduates or ROTC students will not find enjoyable; it is to say that for scholars they really tell us nothing we do not already know. Philip Warner of the Sandhurst establishment labels his work a reappraisal. It is a lightweight attempt to put the army in perspective and to rebut Mrs. Woodham-Smith, while at the same time placing the medical work in its proper place within the history of the development of the health services. The select bibliography makes no mention, however, of medical literature or even of the official medical reports that so influenced the Union medical service in the American Civil War. Colonel ffrench Blake's account is really the more useful since it is written strictly from the military point of view and pays attention to tactical affairs with excellent maps and timetables of the battles, as well as providing marked battle panoramas, a list of regiments of the British army by number and names, and a reasonable bibliography. If ffrench Blake is representative of the new series, Concise Campaigns, then the military historian has something to look forward to at that level.

ROBIN HIGHAM
Kansas State University

PETER HARNETTY. Imperialism and Free Trade: Lancashire and India in the Mid-Nineteenth Gentury. Vancouver: University of British Columbia Press. 1972. Pp. ix, 137. \$7.00.

FRANCIS E. HYDE. Far Eastern Trade, 1860-1914. (The Merchant Adventurers.) New York: Barnes and Noble. 1973. Pp. xii, 229. \$11.50.

Professor Peter Harnetty has written about still

another aspect of the relationship between free trade and British imperialism in the nineteenth century. As is well known, the classical view of this relationship, maintained by Hobson, Schumpeter, and Lenin (as well as W. L. Langer, R. L. Schuyler, and, most recently, D. C. S. Platt), was that free-trade doctrine was at the heart of mid-Victorian indifference, even hostility, to empire. On the other hand, J. Gallagher, R. Robinson, and others have pointed to the reality of imperial expansion and exploitation during this period when statesmen mouthed the rhetoric of anti-imperialism, and D. N. Winch and I saw in the character of classical economic theories a stimulus to imperialism. Albeit on a free-trade base, an industrial Britain was persuaded that it needed to establish commercial ties of an essentially mercantilist character with agricultural nations that would supply England with raw materials, markets for her surplus manufactures, and profitable sites for investment. In the early decades of the nineteenth century a number of economists, most prominently F. List in Germany and H. C. Carey in the United States, feared that without tariffs to protect the growth of domestic industry, their countries would be reduced to "informal" colonies of a British commercial and industrial metropolis.

In Britain's economic relations with India we recognize, in the heyday of Cobdenism, the survival of a mercantilist imperialism. Unlike the German states and America, as Harnetty tells us in his monograph, India could not protect herself against the low prices of British textiles that threatened the ruin of the native cotton industry, for the imperial metropolis deliberately intervened to prevent the colony from erecting tariffs against the manufactures of the mother country. Since the British workshop of the world had no reason to fear competition, the true character of this policy was somewhat hidden from contemporaries because it masqueraded as devotion to a cosmopolitan free trade.

Moreover, as Harnetty demonstrates, the Lancashire cotton manufacturers, who at home marched beneath the banners of laissez faire, persuaded the British government to adopt an interventionist policy in India. In the interests of Lancashire the government of India guaranteed the profits of railway construction and financed public works to facilitate English commercial penetration, as well as actively working to improve the cultivation and marketing of Indian cotton. Harnetty has made a short but useful contribution to the shamefully

neglected field of the nineteenth-century British economic relationship with India. A perhaps minor cavil might be that a book on economic policies should devote some attention to the economic theory of the time.

India, whose offer of opium and cotton for Chinese tea and raw silk was the opening wedge in Britain's trade with the Far East, plays only a peripheral role in Professor Francis E. Hyde's account of Far Eastern trade between -1860 and 1914. Just as Britain in the first quarter of the nineteenth century was moving toward free trade, Holland, which had previously administered her colonies more or less on that basis, revived a mercantilism that may have served as a model for the British in India. By the middle decades of the century a growing Dutch liberalism was to undermine this policy, thus helping British and Malayan Chinese merchants to break the commercial monopoly of Batavia for the distribution of Indonesian produce in favor of British Singapore. Hyde demonstrates that of all the European countries, Britain appears to have been the chief beneficiary of Asian trade, especially of trade with China.

The development of the mainline steamship companies between Europe and East Asia occupies a central role in Hyde's story. Along with the growth of telegraphic communication, this made possible an expansion of trade with Europe as well as the stimulation of local trading. Great entrepôts, with their rich merchants' houses and banks, sprang up at Hong Kong, Singapore, Yokohama, and Shanghai, altering the character of the traditional trade. The production of the traditional Chinese staple of tea was captured by India and that of silk by Japan. Western capital, soon supplemented and to some extent replaced by indigenous capital, mostly supplied by the resident Chinese merchants of Southeast Asia and the Japanese traders in China, promoted the production of Indonesian coffee, sugar, and tobacco, Siamese rice, and Malayan rubber and tin. Once brought out from its traditional, xenophobicisolation into the arena of world trade by the force of Western capital and technology, the Far East did not long remain a mere offshoot of the Western industrial economy, but became the hub of a Pacific economy in which Japan was to play the dominant role. Hyde has written a straightforward, valuable study.

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ROBERT C. GREGORY. India and East Africa: A History of Race Relations within the British Empire, 1890-1939. New York: Oxford University Press. 1971. Pp. xviii, 555. \$24.00.

Asians have occupied a pivotal place in East Africa's twentieth-century history. Asian laborers helped to build the railroad across East Africa. Asians constituted the dominant commercial class, spreading the new money economy into remote African areas, and they organized early and influential political associations. At a time when this community's very existence is threatened it is important to have a serious historical study of the origins and growth of the Asian population, such as Robert G. Gregory's India and East Africa. Despite its all encompassing title this book basically seeks to describe how the Asian immigrant community sought to make a place for itself within the British Empire between 1890 and 1939 by mobilizing support for its cause, not only in East Africa itself, but among Indian leaders in British India and humanitarian and colonial circles in Great Britain. The most salient feature of this book is its exhaustiveness of archival research. Drawing data from the Colonial Office archives in London, the India Office, and the Kenya colonial government archives, Gregory provides a fuller story of the Indians and the development of colonial policy toward the Indian population than we have previously had. Most East African history has been written on the basis of London and Nairobi records; Gregory's use of New Delhi material provides fresh new insights and dimensions on Indian leaders in East Africa, men like Isher Dass and M. A. Desai, as well as on Indian nationalists like Gandhi as they developed policies toward the British colonies in East Africa. Although the author does not develop radical new interpretations of East African history, he does show in great detail how the India Office, spurred on by its problems of administering a colonial society already troubled by anticolonial nationalism, took an interest in Indian problems in East Africa and sought to ameliorate the economic and political conditions of that population. Quite naturally, the book focuses on Kenya, not only because the Indian population there was larger and more dynamic but also because the most explosive racial questions were fought out in that troubled colony. Much less serious consideration is given Uganda, Tanganyika, and Zanzibar, although all three countries are discussed.

Despite the obvious achievements of this work and the commitment to collecting full data there are certain flaws in the author's organization and presentation. Most of these stem from too great a reliance on official British sources, especially India Office and Colonial Office documents, for assembling a portrait of the Indians. These documents tend to give a rather detached view of the Indians and often fail to convey a clear understanding of the aims, ambitions, and driving motives of this admittedly diverse people. As a result the author displays a tendency to depict the Indians as constantly reacting to the initiatives of others, usually European settlers and the British Colonial Office. To be sure, the Asians did not have political power, but they enjoyed more autonomy within certain limited economic and social spheres than the author suggests. Moreover, because of his reliance on official sources, Gregory structures his narrative around issues the British regarded as important, but to which the Indians sometimes did not attach the same immediacy. This mode of presentation works well enough for the events of the 1920s when critical questions of landholding, voting rights, and immigration exercised Indians as fully as the British. But it is not so successful for the more diffuse events of the 1930s. To take but one salient example from the 1930s, Gregory discusses at great length the influential Kenya Land Commission while relegating to a subordinate position a discussion of changes in marketing procedures. The land settlements suggested by the Kenya Land Commission were indeed critically important for Europeans and Africans, and they were symbolically significant for the Indians because they reaffirmed the exclusion of the Indian population from landholding rights in the so-called white highlands of Kenya. But commerce was the life-blood of the Indian population; changes enacted in the marketing of African produce in 1935 were seen as jeopardizing the very existence of the Indian population. Thus Indian delegates to the Kenya Legislative Council walked out in protest over this question, not over the findings of the Kenya Land Commission.

This flaw in perspective notwithstanding, India and East Africa is an extremely important, well-researched study that provides new perspectives for viewing the making of policy in twentieth-century Kenya.

ROBERT L. TIGNOR
Princeton University

OZER CARMI. La Grande-Bretagne et la Petite Entente. (Travaux d'histoire éthico-politique, 24.) Geneva: Librairie Droz. 1972. Pp. 377.

The Little Entente has bulked larger in the consciousness of historians than it did in calculations of major Foreign Offices at the time. The British, whose attitudes from 1920 to 1937 are chronicled here in abundant detail, were relatively indifferent to political arrangements in the Danube basin. Britain's one vital interest was to promote the region's economic rehabilitation in order to preserve the market for British goods and capital.

Carmi appears to recognize the fragmented and essentially parochial nature of his subject, though, as is only natural, he prefers not to call attention to its limited significance explicitly. In fact, the other great powers took the Little Entente hardly more seriously than did England. The Germans, who remained economically dominant in the area throughout, never considered the formal arrangements among the Danube states more than a subsidiary obstacle to their own ambitions. Even the French, as the newly opened archives confirm, always remained properly skeptical of the concrete military worth of their Eastern alliances.

The members of the Little Entente themselves were bound together by a common fear of regional Hungarian revisionism, not by wider European concerns. Hungary aside, the Romanians were worried primarily about Russian and Bulgarian designs; the Yugoslavs about Italian ambitions; the Czechs, at least until the 1930s, more about the Polish and Russian than the German danger. A firm basis for united action was thus often wanting.

Nevertheless, Carmi strains to portray the smaller nations of East Central Europe as continually at the center of diplomatic action. His narrative of relations between Great Britain and the Little Entente in regard to a variety of issues in which the Danube nations played a peripheral role-say the Four Power Pact of 1933 or the Italo-Ethiopian Warat least has the merit of unusual perspective. However, his geographical focus does lead to frequent distortion. Carmi contends, for example, that negotiations for a British guarantee of French security broke down in 1922 primarily because France insisted on analogous assurances for its East European allies, when the stumbling block actually proved to be Britain's unwillingness to make a precise military commitment to the French frontier. Similarly, Carmi seriously overstates the degree to

which France, no less than England, was preoccupied with East Central European problems at the time of Locarno and during the Danube customs union negotiations of 1932. The collapse of the Little Entente, at the first sign of real danger in 1936-37, should have led the author to reflect upon the hollow nature of that alliance earlier.

Carmi's scholarly apparatus is flawed. His monograph is based primarily on five hundred volumes of Foreign Office political correspondence, but these evidently have been sampled rather than studied exhaustively. No less than thirty-seven volumes actually deal (according to Public Record Office directories) with countries other than those under which they are listed in the bibliography. Carmi makes no use of Cabinet or other British primary materials. Nor has he examined either the French or German archives, or the copious secondary literature in German, which includes Günter Reichert's study of the collapse of the Little Entente.

STEPHEN A. SCHUKER Harvard University

NIGEL HARRIS. Competition and the Corporate Society: British Conservatives, the State, and Industry, 1945–1964. [London:] Methuen and Company; distrib. by Barnes and Noble, New York. 1972. Pp. 384. \$12.75.

The focus of this study is the adjustment of the Conservative party to the changes in British society of the last several decades, but particularly since the end of World War II. The author seeks to show how the Conservative party has been able to maintain its position as one of the most successful political parties in modern history, and to this end he is more interested in describing the party's response to changing conditions than in assessing the validity of the response.

Insisting that the party has no "theory" of the status quo, and rarely needs one, Harris stresses that what the Conservatives defend at any given moment depends upon the balance of forces within the party and on what is being attacked by opposition parties. Indeed the party resists theorization because a consistent theory would threaten the coalition of contradictory interest groups, that is the core of the party's strength. Nevertheless, it is possible to construct a hypothetical Conservative theory, and this theory, according to the author, is a blend of "etatist corporatism," more or less opposed to liberal ideology, and "pluralist

corporatism," the latter largely a legacy of the liberals. Much of the party's rhetoric deals with the free market version of political economy, but over time, Harris argues, there has been a decisive shift toward acceptance and even expansion of state planning and the role of large-scale corporate enterprise. Hence the socialists were right, he suggests, to identify the overall process of capitalism's history as quasisocialist.

The new order of the Conservatives, however, was created not as a response to popular demands, but as a result of involuntary changes in the structure and needs of corporations and shifts in the foreign environment. Nevertheless, fragments of a commitment to free enterprise coexist with attempts to regulate or suppress competition and extend state ownership. Both positions represent different elements in party history, and while the resulting policymix would frequently appear to be muddled and ambiguous, a clearer and more consistent result would precipitate open conflict within Conservative ranks. In short, the Conservative party survives not despite but because of its lack of principle and refusal to espouse any coherent political philosophy.

Of course much of the same statement could be made about the Labour party or, for that matter, almost any modern democratic political party. The only ideological parties these days are to be found in authoritarian societies and police states, and more and more often not even there. The conditions that make for flexibility and compromise are present everywhere, and certainly Harris is correct to forecast that the next major challenge to the British Tories will be the adjustments that are made necessary by the changing patterns of international business and the multinational corporation that already dominates the European Common Market. Such adjustments are not made easily, as witness the fumbling of the Heath government, but the opportunism and resilience of the British Conservative party, if the long past is any guide, should never be underestimated.

ARNOLD A. ROGOW
City University of New York

L. M. CULLEN. An Economic History of Ireland since 1660. New York: Barnes and Noble. 1972. Pp. v, 8–208. \$8.50.

Dr. Cullen has provided a wide-ranging survey of the economic history of Ireland. In fulfilling his aim to outline its main features since

1660 he has covered more than 300 years, during which Ireland has progressed from an agricultural country in the mid-seventeenth century to a point where Ireland's interest today lies in stable international conditions, a rising level of world economic activity, and ready access for expanding exports to foreign markets. Ireland has gone through repeated cycles of stagnation and prosperity with periods of famine, high emigration rates, and distressed living conditions as well as those periods when the country's economy flourished. All aspects agriculture, industry, export-import trade, employment and unemployment, banking and currency, living conditions, transportation, population, prices, etc.-are touched upon. As might be expected in a small book (181 pages) details on any one subject are limited. Because of the many topics discussed it is frequently difficult to follow the course of a given industry or phase of the economy during the three centuries.

Froude and Lecky, writing in the second half of the nineteenth century, set the pattern of considering Irish economic history in the context of politics and legislation. As a result there had been a general acceptance of the view that the economic background was reflected in the two dominant political themes: the land question and home rule. Cullen asserts that the modern dilemmas—political and economic—are less intertwined in their historical backgrounds than has often been assumed. He feels that economic interests are not the chief cause of political conflicts today; the political, religious, and racial forces have had great strength in their own right in Irish history.

Throughout the book Cullen attempts to explode long-standing ideas about Irish backwardness and stagnation. His views are contrary to those of many previous writers on such subjects as the potato, the long dependence of Ireland on exports, the unduly dark picture of Irish social conditions in the prefamine decades, the attitudes toward landlords, and the economic vitality of postfamine Ireland. Future historians will find much to accept and expand or to challenge. An absence of any footnotes will make it difficult to check with ease the specific data that are given. The author has provided, however, notes on the many primary sources that he has used.

Since Irish economic history as a study is still in its infancy Cullen's book may be expected to remain a standard work for many years. Based on extensive research in archives pertaining to economic and social conditions in Ireland it not only provides a good introduction to the development of Ireland's economy, but will serve as a reference for those investigating areas that are not yet fully explored.

HOMER L. CALKIN U.S. Department of State

DONALD HARMAN AKENSON. Education and Enmity: The Control of Schooling in Northern Ireland, 1920–50. (A publication of the Institute of Irish Studies, The Queen's University, Belfast.) New York: Barnes and Noble. 1973. Pp. 287. \$13.75.

It is difficult to summarize this book because Professor Akenson has chosen a chronological approach that does not give the reader enough generalizing statements to carry him along and help him get his bearings.

Akenson seems interested mainly in the primary school system in Ulster, but he remarks on intermediate, technical, and grammar schools as well. The book might have been easier to follow if the author had focused exclusively on the primary schools and left the rest of Ulster's educational "old curiosity shop" alone.

To summarize the argument about primary education, the government of Northern Ireland inherited from the British government a primary school system that was in fact denominational (that is, Catholic and Protestant schools controlled by the respective clergies) even though it was financed largely by the state. In the early 1920s the Stormont government required that any primary school must, to qualify for full public funding, come under the jurisdiction of regional and borough educational committees. Since those committees, given gerrymandering in Ulster, were dominated by Protestants, the law was anathema to the Roman Catholic hierarchy, and Catholic schools remained "voluntary" schools that were funded only in part by the state. From the first, then, the public school system was Protestant in clientele. The Protestant clergy soon succeeded in making them Protestant in doctrine as well by putting pressure on Stormont to make "Bible training" obligatory in public schools. This made the Catholic hierarchy even more reluctant to have their schools become part of the public system. Thus evolved the system of separate and unequal schooling in which the primary schools have been "twisted into Protestant institutions fully dependent upon public funds" while Catholic schools get less than full public funding.

Akenson's account is scrupulously evenhanded. He calls attention to an interference in politics on the part of the Protestant clergy that far exceeds that of the Catholic clergy in "the former United Ireland," even though Protestant clergymen have ritualistically denounced "priests in politics." But he shows that the Catholic bishops and priests are also responsible for the unequal status of Catholic schools. He makes the telling argument (p. 116) that the law could have been manipulated so that Catholic schools could have received full public support while remaining Catholic in clientele, teaching staff, and doctrine if the clergy had been willing to admit the laity into the management of their schools. But the clergy were determined to keep control solely in their hands, thereby perpetuating "the usage of inadequate and outmoded school facilities." It would take a Voltaire to exploit the ironies in this account of Protestant and Catholic sins committed in sweet Jesus' name.

Though unequal, the Catholic schools were not all that unequal. The state has from the first paid teachers' salaries and has raised building grants from fifty per cent in 1930 to eighty per cent in 1968 (though Catholic schools must change their status from "voluntary" to "maintained" to be awarded eighty per cent). This amount of state support would turn an American bishop green with envy. Northern Ireland, then, has not been merely the instrument of the Protestant clergy in educational matters. Unionist politicians have made progressively larger grants to the Catholic schools in the hope, which the British raj also entertained until 1921, that concessions to the bishops would persuade them to keep the natives quiet.

The major criticism of the organization of this book is that Akenson ends the account at 1950. Have there been no educational developments since then that are worthy of close examination? To choose 1950 as a cutoff date is even more baffling since Akenson makes, in his concluding chapter, suggestions for reform of the system as it stands today. And one can criticize his assumption that education has that great an influence on enmity between Protestants and Catholics. As he notes, the family and peer group are far more important in shaping the attitudes of Protestants and Catholics toward one another. Akenson recommends that Catholic schools be fully supported by public funds if the clergy agree to admit laymen to a larger share in school management—a big if (as he realizes). This proposal may be more feasible than his suggestion that experiments in integrated schooling be made in the hope that they might "reduce ill-feeling and misunderstanding" between the two communities, and he specifies the ideal conditions under which integration should take place. Such a recommendation seems to assume radical changes in Protestant-Catholic relations if it is to be workable. Akenson furthermore ignores the psychological advantages that follow from a segregated system. As Rosemary Harris points out in her excellent study, Prejudice and Tolerance in Ulster (1972), segregation in education and in every other social activity endows the individual Protestant and Catholic with a strong sense of personal and group identity. There is no generation gap; parents communicate with their children because they (the parents) are anxious that the children remain with their own kind and keep the faith. This sense of belonging may be more valuable than whatever integrated schooling might achieve.

Akenson is to be congratulated for having taken on so complicated and controversial a subject and for reading through statutes, debates, and reports that are not only technical and dull but depressing in their revelation of selfishness and self-deception. His book will be a standard work of reference for historians interested in Northern Ireland.

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JOSEPH GOY and EMMANUEL LE ROY LADURIE, editors. Les fluctuations du produit de la dîme: Conjoncture décimale et domaniale de la fin du Moyen Age au XVIII° siècle. Communications et travaux. (École Pratique des Hautes Études—Sorbonne. VI° Section: Sciences économiques et sociales. Centre de Recherches historiques. Cahiers des études rurales, number 3. Association française des Historiens économistes. Premier Congrès national—Paris 11-12 janvier 1969.) Paris: Mouton & Co. 1972. Pp. 396. 75 fr.

This volume extensively illustrates both the rich potentialities and the practical difficulties that characterize the statistical method that has become the hallmark of the VI° Section of the École Pratique des Hautes Études. The original proposal that led to this publication was made by Ernest Labrousse who urged that the total agricultural production in France during the early modern period might be measured by examining the relatively voluminous records of the ecclesiastical tithe. Ful-

fillment of this fruitful suggestion clearly required the cooperative effort of many researchers and was undertaken by a group at the above-named institution, with the help of additional personnel. Their initial findings, which they presented to the Congress of the Association of Economic Historians in Paris, January 11-12, 1969, are set forth in this volume, together with an introduction and summary-conclusion by Joseph Goy and Emmanuel Le Roy Ladurie in which the latter synthesizes the group's researches and presents tentative conclusions. The reports contain extensive statistics concerning the fluctuations of the tithe in sixteen different areas from all parts of France, with the notable exception of the west, and trace this source of ecclesiastical revenue with the sophisticated techniques of the school.

This investigation of the tithe in many different areas of France over several centuries inevitably encountered a host of difficulties. First, there were various categories of tithes: those collected by the clergy as a percentage of certain products, and those collected by tax farmers on a single crop or several crops, either in money or in kind. (If in money, the statistics are here converted by a complex process into equivalents in produce.) Then there were innumerable local variables: different rates of assessment and products assessed, and differences in the form, duration, and application of the tax farmers' contracts. Occasionally the tithe was levied merely upon units of land, and certain lands, notably newly cleared acreage, were entirely exempt. Also the collection of tithes was powerfully influenced by such external factors as inflation and deflation, the impact of wars, antitithe agitation especially during religious upheavals, graft and corruption, and demographic changes. Many of these factors may be partially neutralized, however, and a majority of the contributors claim to have established a reasonably accurate estimate of the tithe in their respective areas. In fact, the editors deduce broad trends from the collected statistics and tentatively trace the variations in the tithe over units of decades from the mid-fourteenth century to the Revolution. This analysis permits them to conclude that the often violent fluctuations of the tithe from year to year were caused chiefly by temporary conditions and that the French agricultural system remained essentially static until the mid-eighteenth century, after which there was a moderate expansion.

The editors of the volume, however, readily

recognize the impossibility of estimating total agricultural production from the statistics thus far collected. A few contributors, notably Georges Frêche, argue that such an effort will never succeed because of the nature of the sources and the many imponderables that beset the researcher. Le Roy Ladurie, on the contrary, takes the position that this objective, while unattainable at present, will be achieved after the completion of vast additional research and monographic analysis. The volume therefore represents an interim report with much valuable but incomplete information on the fluctuations of the ecclesiastical tithe and indications of extensive projected research into the agricultural foundations of the French economy.

WILLIAM F. CHURCH Brown University

SALVO MASTELLONE. Venalità e machiavellismo in Francia (1572–1610): All'origine della mentalità politica borghese. ("Il pensiero politico" Biblioteca, 4.) Florence: Casa Editrice Leo S. Olschki. 1972. Pp. 257. L. 4, 500.

Professor Mastellone has further developed the thesis that he first described in the last chapter of La reggenza di Maria de' Medici (Messina, 1962) and has now projected it back into the period 1572-1610. Briefly stated, the thesis in its present form holds that between the accession of Henry III and the institution of the paulette in 1604 the upper echelon of legally trained French bureaucrats underwent a mental transformation that had its origins in the ideas of Machiavelli and the practice of venality of office. This led them to a new sense of their importance as a separate group at the same time that the French monarchs were finding it necessary to rely on their services to govern the state. Thus the feudal-noble monarchy became the administrative-venal monarchy.

The defense of this thesis places the author in the midst of a steadily growing controversy concerning the nature of society in early modern France. Mastellone has tried to present an answer to one of the central questions of the controversy—the place in society of royal officials. Since his answer is the result of a traditional approach to political theory and is derived from the study of a selection of contemporary controversial writings and secondary sources, it must be regarded as provisional. As such it would best have been presented as a long article. This is especially true since the time lapse necessary for the publication of a book means that several recent articles and

books (notably Myriam Yardeni, La conscience nationale en France [1971], and William F. Church, Richelieu and Reason of State [1972]) that might have led the author to modify some of his ideas could not be consulted.

Before the thesis of this book can be accepted or rejected a more thorough study of manuscript records is necessary. For example, Mastellone's ideas concerning the Estates-General of 1614 are not substantiated by recent research on the constitution and action of that body. Nor, until more is known, can the development of the process that he describes be said to be limited to the late sixteenth century.

But a warning is in order. Mastellone's intuition led him, when writing the same type of book on the regency of Marie de Médicis, to suggest a number of themes that have been substantiated by subsequent research. In considering the late sixteenth century he has isolated currents of thought that largely escaped both Yardeni and Church. His interpretation of their effect can be ignored only at great risk.

J. MICHAEL HAYDEN
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GÉRARD BOUCHARD. Le village immobile: Sennelyen-Sologne au XVIII° siècle. (Collection Civilisations et mentalités.) [Paris:] Plon. 1972. Pp. 386.

Sennely-en-Sologne, a peasant community of some six hundred inhabitants located south of Orléans, experienced neither the economic nor the demographic expansion characteristic of eighteenth-century France. In Sennely the continual erosion of human life caused by persistently high mortality rates throughout the eighteenth century threatened to destroy the demographic balance. Only the immigration of adults from nearby villages staved off the demographic decline that finally set in during the last decade of the Old Regime.

Sennely was swept along economically by a long-term movement of enclosure for pasturage that reduced the amount of land utilized for the cultivation of grain and undermined the market for rural laborers. Peasant ownership of land was virtually nonexistent, and the little wealth that was generated in Sennely was drained off by absentee landlords, both noble and bourgeois. Fairs and markets declined.

Sennely was disintegrating socially. To the extent that there was social integration this was provided by popular religious traditions. Neither family life, frequently interrupted by the death of a parent, the seigneurial system,

nor village political institutions provided the support necessary for full social existence. Sennely, in short, suffered from acute anomy.

Such, in general, is the "total history" of Sennely. It is unfortunate that this history is not substantiated by adequate documentation. The study rests essentially on eighteenth-century narrative sources, the memoires of the curés, and the cahiers de doléances. Indeed, the main source and the chief guide for interpreting the history of this village is the memoir written by Christopher Sauvageon, curé from 1676 to 1710.

By far the best segment of the book is that which deals with demography, but the author has intentionally limited himself to an investigation of only the broadest outlines of Sennely's demographic history. No attempt was made to reconstruct families; there are no tables for ages at marriage; all intervals between births after the first child are lumped together. Immigration is said to have been essential, yet there are no acceptable statistics on it. The tantalizing discussion of mortality rates indicates that a full demographic history of this village would have been a major contribution. The documentation was available, but it was not used fully.

The section dealing with the agrarian economy and landownership rest on extremely narrow documentary bases. The loss of virtually all the seigneurial records, notably the terriers, is regrettable, but the eighteenth-century fiscal records, the surest source for landownership, were scarcely tapped. There is no demonstration of the advance of large property at the expense of small, no proof that land was in fact passing out of cultivation. Similarly, there are no convincing statistics on social classes and their composition, only rough estimates. We are told that there were landless laborers, small farmers, and artisans, but never how many or how the ranks of these various social groups fluctuated in the course of the century. Social mobility through marriage was supposedly minimal, yet there are no figures on marriage patterns. Crime is said to have been a symptom of social malaise, but the discussion of criminality is anecdotal. Again, there are no statistics on the patterns of rural crime.

With so many excellent French village monographs based on proven methodology as guides, it is difficult to understand how the author could hope to write the "total history" of a peasant village without hard data.

JAMES L. GOLDSMITH
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F. V. POTEMKIN. Promyshlennaia revoliutsiia vo Frantsii [The Industrial Revolution in France]. Volume 1, Ot manufaktur, k fabrike [From Handicrafts to Factories]. (Akademiia Nauk SSSR, Otdelenie Istorii.) Moscow: Izdatel'stvo "Nauka." 1971. Pp. 453.

The Soviet historian Fedor Potemkin has long been active in French history, having published works on the upheavals of 1789 and 1848, as well as studies of labor unrest in the early nineteenth century. The present work is thus the capstone of a half-century's researches in the economic history of France; yet it is not so much a survey of the era 1750–1850 as it is a series of topical studies, arranged roughly in chronological order and preceded by an extensive bibliographical essay.

Throughout the book historiography vies with history for the reader's attention, for it is Potemkin's stated intention to offer a corrective to the numerous Western or "anti-Marxist" studies of this period of French history, marred as they are by the "bourgeois evolutionist" notion that the Industrial Revolution tended to provide eventual solutions to the social problems that it created. The introductory material, some seventy-five pages of historical criticism, shows Potemkin thoroughly familiar with recent Western literature on the subject, though inclined to spar with such earlier works as Toynbee's Lectures on the Industrial Revolution in England. There is also a somewhat labored inquiry into the origins of the term "industrial revolution."

Once this preliminary skirmishing is over, Potemkin examines various sectors of the French economy, particularly textile manufacture (wool, linen, cotton, and silk), coal and iron production, and agriculture. The treatment is in each case two-fold—an examination of the industry and a critique of historical literature relating to it. Several of these chapters are particularly impressive, and this is notably the case with the material on the silk industry. The analysis is drawn from a considerable mass of manuscript materials, mostly from the Archives Nationales. Similarly well done are the chapters relating to iron production. Here too, manuscript sources are extensively used; the profile of the industry is complete down to regional nuances. In these final chapters Potemkin is most convincing in his effort to show the linkage between the growth of capitalism and deepening social antagonisms, exploitation, child labor, etc.

There is a certain unevenness about the book, some chapters showing a greater depth of research and perception than others. While many readers in the West will disagree with some of the author's conclusions, it remains nonetheless a serious and well-documented study.

LEE KENNETT
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JOHN E. N. HEARSEY. Marie Antoinette. New York: E. P. Dutton and Company. 1973. Pp. 295. \$7.95.

Marie Antoinette is among the classically tragic figures of history and as such has perennially attracted popularizing biographers. The latest is John Hearsey, an English free lance. Although he provides neither notes nor bibliography, it is evident that he has utilized the principal published sources. His factual errors are few and, in this context, relatively inconsequential. Certain episodes stand out in this account, particularly the affair of the diamond necklace and the queen's imprisonment and trial; over half the book deals with her last four years. Hearsey misses no chance throughout this work to exploit, perhaps a little too insistently, the rich dramatic ironies of the story of this "unwise consort of an incompetent sovereign." Choosing to focus, like so many before him, on Marie Antoinette "simply as a wife and a mother," he has meanwhile narrowly limited his field of view. The wider drama of events in France and Europe is glimpsed only fleetingly, while the secondary characters, outside the royal family itself, remain sketchy, even Count Fersen, the one real love of Marie Antoinette's life. The author contents himself with straightforward narrative, providing little practical motivation or psychological insight. The queen thus appears as essentially the bearer of a destiny, moving inexorably toward its (preordained end, which she yet transcends through the growth of her character. To fill in historical background there are appendixes, including glossaries of terms and personalities for the Old Regime and revolutionary period, chronological and genealogical tables, maps, and a brief essay on the causes of the Revolution. Although useful, much of this material could better have been integrated into the text itself. Illustrations are few but well chosen. This work, in short, compares well with others in its genre, for instance, those by Stefan Zweig or Stanley Loomis, though it adds nothing new to the subject, as did André Castelot. It was hardly expected that it give serious attention to Marie Antoinette's considerable political role, studied so long ago by Alfred Arneth, Max Lenz, Jeanne Arnaud-Bouteloup, and Alma Söderhjelm. The definitive biography of both the woman and the queen is still awaited.

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GERLOF D. HOMAN. Jean-François Reubell: French Revolutionary, Patriot, and Director (1747– 1807). The Hague: Martinus Nijhoff. 1971. Pp. viii, 202. 36 gls.

In French Revolutionary scholarship, as Professor Homan suggests, much remains to be done in the area of biographical studies. This is especially true of the lives of the important secondary figures in the Revolutionary drama. Homan's study of Reubell closes just such a biographical gap left open for a surprisingly long time. It also provides a close personal look at the least well-explored episode of the Revolutionary era, the Directory, the period when Reubell reached his eminence.

Homan's articles on Reubell, appearing over the last decade and a half, engendered the hope that they would culminate in a biography of the Alsatian revolutionary. The expectation of a full-scale study of Reubell was heightened by awareness that his life was a fascinating one. Homan's monograph succeeds in maintaining a high pitch of interest in Reubell as a person and as a revolutionary, even though it does not answer all questions raised by Reubell's career. The biography is quite conventional in concept-there are no forays into psychohistory here—and in structure: it progresses in orderly fashion from Reubell's provincial, pre-Revolutionary career through his membership in the Revolutionary national assemblies in Paris, his term as procureur-général syndic of the Upper Rhine, to his directorship, the culmination of his life and the most important section of the book.

Reubell was, if anything, an exceedingly "political" man, and accordingly one able to change his stance with relative ease; this may have been the key to his political success, though Homan seems somewhat surprised at every political tergiversation. Homan faces up to the controversies surrounding Reubell's career though he is not always able to resolve them. The connection drawn between Reubell's national politics and his provincial patriotism helps to explain his attitude toward Jews and his special interest in foreign affairs; but that he died relatively poor is not a convincing argument against his possible corruption, in face of his periodic possession of valuable

properties and the sizable retirement grant made to him from secret government funds when he left the Directory.

Reubell's was a fascinating life, one well worth knowing more about. Homan's thorough and extensive research allows him to tell us perhaps all that can be told about Reubell, and he is generally successful in offering answers to an explanation for the issues raised by the director's career. Thus, it is all the greater pity that this biography is so ungracefully written. This may be due, in large part, to poor editing; the book is so full of verbal errors as to suggest that it was not edited at all. Certainly Reubell, and possibly Homan, deserves better.

CHARLES A. LE GUIN
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MARC BOULOISEAU. Bourgeoisie et révolution: Les Du Pont de Nemours (1788-1799). (Commission d'histoire économique et sociale de la Révolution française. Mémoires et documents, 27.) Paris: Bibliothèque Nationale. 1972. Pp. 250.

There is no gainsaying that Marc Bouloiseau, the diligent and distinguished student of the French Revolution, has written a coherent, even an engrossing, narrative of the fortunes of the Du Pont family, specifically of Pierre Samuel, the father, and his two sons, Victor the elder and Irénée the younger.

The data Bouloiseau makes use of lie in the countless private papers, letters, and memoranda of the fabulous Eleutherian Mills History Library that he first began to examine in 1964. As his research developed and deepened, the goal of his investigation became clearer. Here he found a good occasion for "reflection" and an opportunity to restore and reopen "the dialogue of me and others" around the theme of the Revolution. He deals with the repercussions of the great maelstrom on the material life, the opinions, and the attitudes of a family so representative as this "bourgeoisie of talent." He asks in what measure the Revolution engaged or even compromised the future of the Du Ponts. He does not claim to give definitive answers to the queries he raises, but merely to provide the key elements of a broad appreciation.

In the first part, roughly half of the work, he vividly tells the story of the family from the years he calls "The Time of Hope, 1788–1791" up to the resolution of the family in 1799 to abandon France and settle in the

New World overseas. They called this move to America, which they and their descendants were to conquer, the "transplantation." Part 2, entitled "Documents" (private not official papers), backs up the narrative. These documents give the texts that illustrate such developments as the meetings of the Society of Thirty in 1789 and the sporadic peasant "troubles" of 1790. Thus as Bouloiseau tells his story, the emphasis falls not on the subtitle "Les Du Pont de Nemours (1788–1799)" but on the full title "Bourgeoisie et révolution."

What is one to make for the claim that we are here in the thick of the newest of the new history? If the author's words especially in the introductory matter are taken literally, no one but a scholar thoroughly at home with the Revolution itself but also trained in some sociology, social psychology, and "collectivist mentalities" would be qualified to write on the subject. But Bouloiseau's book is not of the avant-garde history. It makes no novel demands of the reader. It is an exceptionally informative and thoughtful study that in fact relates the family to the larger movement and the movement of revolution to the fortunes of a representative liberal bourgeois family. Occasional references to the new vocabulary of the new historians notwithstanding, the volume is not a pioneer essay in collectivist social history. It is written more in the spirit and letter of Georges Lefebvre or Lucien Febre than, say, of Mandrou or Vovelle, Furet or Cobb when the latter deals with collective mentalities. And to think and write in that spirit and manner is in itself no mean achievement.

LEO GERSHOY
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A. V. ADO. Krest'ianskoe dvizhenie vo Frantsii vo vremia velikoi burzhuaznoi revoliutsii kontsa XVIII veka [The Peasant Movement in France at the Time of the Great Bourgeois Revolution at the End of the Eighteenth Century]. Moscow: Izdatel'stvo Moskovskogo Universiteta. 1971. Pp. 452.

This volume, a careful study of an immense, complicated subject, is the work of a member of the faculty of Moscow State University, the most prestigious teaching institution in the Soviet Union. It is one of a considerable number of books in the field of modern French history published in the Soviet Union since 1956 or 1958, primarily by men in Moscow, and it reflects the growing interest in French history, especially in radical ideas and move-

ments. On the other hand, the Moscow State University Press published only nine hundred copies of this volume, so scholarly books in the Soviet Union apparently attract even smaller markets than they do in this country.

This study covers only the period from 1789 through 1792, with a fifty-page introduction on the peasants' situation before the Revolution. It concentrates almost entirely upon the countryside, with little reference to the impact of peasant movements upon political developments in Paris or on the international impact of the various upheavals in France. Professor Ado, unlike most Soviet historians, has been allowed to leave his country for research abroad. He has therefore made use of the Archives Nationales, as well as the archives in Allier, Ardèche, Ariège, Gard, Gers, Isère, Yonne, Cher, and Indre. Like all Soviet scholars, in his bibliography he lists Soviet sources first. Thus, he lists Soviet archives before those he used in France, Soviet secondary sources before foreign, and the works of Marx, Engels, and Lenin before those of any other observers or scholars. He relies heavily on Saboul, obviously has a special appreciation for Jaurès, knows well the French scholarship of the Third Republic, and is not well acquainted with English or American publications on the Revolution.

Ado's main thesis is that the Revolution brought benefits to all peasants in demolishing the feudal system, but that the more prosperous peasants were the main beneficiaries, as he believes they were in all revolutions before the November Revolution. The land arrangements made were such that industrialization in France in the nineteenth century was much slower than in England.

The Peasant Movement is much less dogmatic than most Soviet scholarship, perhaps because Ado's extensive work in French archives has persuaded him that he should study all materials before reaching conclusions. He even suggests that additional research needs to be done on this subject before definite conclusions can be reached concerning the peasants' goals and role.

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c. STEWART GILLMOR. Coulomb and the Evolution of Physics and Engineering in Eighteenth-Gentury France. Princeton: Princeton University Press. 1971. Pp. xvii, 328. \$13.50.

The name coulomb was given to a quantitative unit of electricity long after Charles Augustin Coulomb (1736–1806) had made an early estimation of the efficiency of laboring men and later determined the force laws of electrostatics and magnetism. After a career as engineer and physicist and public service in the years before and after 1789, he died at a moment when individuals were apt to be quickly forgotten and thus neglected by later historians of science. In the absence of any full study of the man, Professor Gillmor's well-documented book makes much information available concerning Coulomb's work and times.

Two chapters of this revised dissertation present biographical details, while four more offer a technical survey of problems ranging from the theory of structural stresses and design to soil mechanics and revetments, leading in turn to activity in the context of the Académie des sciences, torsion balances, and the quantitative investigation of static electricity and magnetism.

Coulomb's contribution to the general history of science is characterized by Gillmor's interesting claim that Coulomb's studies led to the final rejection of Cartesian vortexes and effluvia as explanations of magnetism and light and to the general acceptance of Newtonian mechanics as the basis for emerging branches of physics. Elsewhere the author suggests that the rise of the modern mode of this science grew from Coulomb's fusion of the curiosity of the natural philosopher with the engineer's contact with reality and the harmony of rational analysis.

Service on public commissions marked Coulomb in later years as a social engineer. He worked on the design and planning of canals for upper Brittany, the improvement of water supply and hospitals in Paris, and a report (1776) for the reorganization of the Corps du génie, of which he had been a member for thirty years. In this document, reproduced on pages 255-61, one finds that Coulomb's thinking about the value of useful work recalls that of Voltaire, who desired to see manpower usefully employed, and, in particular, that enrolled in monastic orders. Apart from this one text, the reader finds little contact with the ideological trends of the century. Coulomb lived a quiet life politically, apparently unconcerned about the state of France; he stayed away from Paris during the Terror except for a brief visit for the funeral of Lavoisier, and he served the newly formed Institut under the Directoire and Bonaparte. He remains a shadowy figure about

whom Gillmor could find few personal details. From the inventaire après décès the author re-creates the modest milieu of the apartment near the Institut—a poor substitute for the characterization that might have been drawn from analysis of such highly personal documents as the report on the reorganization of the Corps du génie.

This fully researched book is perhaps a product of the comprehensive survey of the whole field of the history of science associated with the editing of the *Dictionary of Scientific Biography* now well advanced in production.

HARCOURT BROWN
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RENÉ PICHELOUP. Les ecclésiastiques français émigrés ou déportés dans l'État Pontifical, 1792-1800. (Publications de l'Université de Toulouse—Le Mirail, Series A, number 15.) Toulouse: Association des Publications de l'Université de Toulouse—Le Mirail. [1972.] Pp. 302. 51 fr.

This book opens with a preface by Jacques Godechot, who explains that general studies have given the impression that French emigré priests were better treated in Protestant countries than in Catholic states where they were regarded as potential carriers of revolutionary ideas. This study offers confirmation for the Papal States. It is based almost exclusively on manuscripts found in the Vatican Archives.

The organization is effective. Abbé Picheloup examines successively the arrival of emigré and deported priests between 1791 and 1794, the conditions of their existence while there, and their return to France between 1795 and 1800. His research has revealed that only about 200 priests arrived by the end of September 1792. Following the expulsion order of August 26, 1792, this trickle turned into a flood, and by the end of 1792 there were about 2,200. Only 300, however, were allowed at Rome. The remainder were scattered in religious houses throughout the papal territories where superiors, guided by rules laid down by Monseigneur Caleppi, provided supervision. After a tapering off in 1793, another wave came in 1794 before the French advance in that year. As a result there was a total of 3,000 emigrants by the end of 1794. Caleppi refused to take any more because of popular discontent caused by a rise in prices that accompanied the emigration.

Those who were allowed in had to demonstrate their religious and political orthodoxy and adjust to the life style of the house that

sheltered them; they were forbidden to leave the diocese without permission and found that they had little to occupy their time except for the saying of occasional masses. This discussion of their daily life suffers from a dearth of source material from French priests except for the fragmentary evidence that happened to find its way into the Vatican Archives. And Picheloup fails to describe the activities of Cardinal de Bernis or Cardinal Maury.

In conclusion, though the evidence is sketchy, Picheloup says that, while the emigrés accepted Napoleon, most of them probably still awaited the restoration of the monarchy and, as a result of their dependence on the pope during this period, took their first steps in the direction of ultramontanism. In an appendix the author offers a series of documents and provides lists of those present in the Papal States and at Rome. The lists, however, appear incomplete and, because of their arrangement, are difficult to use. There is no index.

LYNN OSEN
Beloit College

G. GERBAUD et al. La Révolution dans le Puy-de-Dôme. Foreword by A. SOBOUL. (Commission d'histoire économique et sociale de la Révolution française. Mémoires et documents, 26.) Paris: Bibliothèque Nationale. 1972. Pp. 320.

This is a fine collective work in French Revolutionary regional (Puy-de-Dôme) economic and social history—the fruit of research in the department and municipal archives by four students of the Faculté des lettres of Clermont-Ferrand. As Albert Soboul indicates in the preface, too often in the past French Revolutionary history has concentrated on the stirring events in Paris-the hub of the Revolution, true, but then again significant economic and social events in the provinces, later departments, would have a profound impact on the capital. In fact, capital and regional history are closely interrelated. Within the last forty years authorities such as Georges Lefebvre and Albert Soboul among other well-known scholars have used and have encouraged others to investigate valuable historical sources found in regional archives. There is much yet to be done, however.

Gérard Gerbaud in the first piece in the collective work, "Esprit public et police générale dans le district of Clermont-Ferrand (juin 1793-été 1794)," gives a vivid portrayal of the politically mixed bourgeois leadership categories, whether Girondin or Jacobin, and their frictions and sometimes cooperation in the face

of general peasant and worker opposition to such measures as the raising of troops, requisitioning of grain, fixing of prices, and the enforcement of anticlerical legislation. By the summer of 1794 local Revolutionary administrations were discredited, and the sans-culotterie regarded the Ninth Thermidor and its consequences with relief.

Jean Pételet in the second article, "La Vente des Biens du Clergé dans le District de Clermont-Ferrand (1790-1804)," which was reinforced by valuable statistical tables (pp. 111-34), concludes that although the sale of clerical lands brought very limited revenue to the state, because of undervaluation of lands and inflation of Revolutionary currency, and did not relieve the misery of poor peasants, it redounded in favor of the bourgeois (merchants and lawyers largely) who got 71.4 per cent of the lands and 61.4 per cent of the buildings of the clergy in the district. Indeed, many peasants would simply change masters.

Daniel Martin in the third article, "La Vente des biens des Émigrés dans le District de Clermont-Ferrand (1792-1830)," reviews for a longer period the disposition of emigré property in the district. His information is backed by an excellent series of statistical tables and graphs (pp. 218-61). Once again the peasants would profit but modestly from the sales. They acquired twenty-seven per cent of the landed property while they represented forty two per cent of the purchasers. On the other hand, amazingly, members of emigré families, generally represented by agents, were able to purchase eventually more than twenty-six per cent of the lands offered for sale. The bourgeois category (merchants, lawyers, and proprietaires) would get the lion's share—forty per cent.

The last article in the collection, "Les Fêtes civiques dans le Department du Puy-de-Dôme sous la Révolution" by Annie Lamadon, is an interesting vignette of one aspect of French Revolutionary social history—civic festivals. Despite the patriotic ardor of Revolutionary leaders, whether deists, agnostics, or atheists, who wanted to replace traditional festivals, such as Catholic feast days, dynastic celebrations, and baladoires (games and dancing) with new patriotic holidays (July 14, August 10) or festivals dedicated to Robespierre's Supreme Being, the mass of people in the department would retain their preferences for the traditional festivities (excepting the dynastic). It was the bourgeois, the class that benefited the most from Revolutionary change in the area, that proved most flexible in adapting to the directives emanating from Paris relating to civic festivals.

To conclude, this collective study, a result of solid research in regional archives, is a significant contribution to French Revolutionary regional economic and social history and is recommended to all serious students who want to know more about events outside of Paris during the French Revolution.

JAMES MAXWELL MOORE
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WILLIAM SCOTT. Terror and Repression in Revolutionary Marseilles. New York: Barnes and Noble. 1973. Pp. xii, 385. \$23.75.

The core of Professor Scott's book concerns the Revolutionary Tribunal of Marseille, the judicial body that administered the Terror in that city following the federalist revolt of 1793. He makes extensive use of the verbatim proceedings of the tribunal to illumine the attitudes and ideologies of the judges and, to a lesser extent, of the judged. His study of the tribunal is preceded by a narrative of the Revolution in Marseille from the calling of the Estates-General to the federalist revolt. It is a puzzling story: always on the extreme left in the early years of the Revolution, Marseille suddenly veered away from the radicalism of Paris in April and May of 1973. The revolt, interestingly enough, was led by the sections, the same bodies used by the sans-culottes to impose their radical policies on the Convention. How and why the sections adopted a moderate line remains something of a mystery in Scott's narrative. There was no significant turnover of personnel that might have caused such a change, no dramatic event propelling erstwhile revolutionaries into reaction. Indeed, in the beginning the events that became the federalist revolt must have seemed to most Marseillais much like earlier episodes in the Revolution, in which the Marseillais had always showed a willingness to take action on their own, independent of any direction from the capital. Always an enigma, the federalist revolt remains so in Scott's account.

Nor is the enigma clarified in his chapters on the Revolutionary Tribunal. In his analysis of the tribunal's proceedings Scott has other questions in mind: What kinds of offenses did the tribunal consider most serious? Were the tribunal's decisions based on adequate evidence? Were suspects allowed to defend themselves? In other words, his analysis is meant to inform us about the court and its officers, not about the federalists. His conclusions are interesting and

solidly documented. The tribunal was harsh but judicious. It sentenced nearly three hundred to death, but its judgments were based on a serious effort to establish guilt. No one was condemned without adequate documentary evidence, and defendants were permitted to argue their cases at some length. The tribunal was harsher in its judgments of rich and welleducated defendants than of poor and ignorant ones; peasants, artisans, and workers were often forgiven their participation in the federalist movement on the grounds that they had been misled. The death penalty was imposed above all on those who had acted as officials of the sectionary regime; serving in the federalist army or national guard or merely displaying federalist sentiments earned lesser penalties and were sometimes forgiven altogether. In short, the Terror in Marseille showed none of the excesses evident in Lyon, Nantes, or Toulon.

This book is a competent and useful piece of scholarship that adds to our knowledge of the way the Terror operated in the provinces. But it fails to illuminate the central question of the Revolutionary history of Marseille—How did the city whose volunteers helped topple the monarchy on August 10, 1792, and whose name adorns the greatest anthem of the Revolution, turn to counterrevolution in 1793? The federalist revolt of Marseille still awaits its historian.

WILLIAM H. SEWELL, JR. University of Chicago

ALF ANDREW HEGGOY. The African Policies of Gabriel Hanotaux, 1894–1898. Athens: University of Georgia Press. 1972. Pp. viii, 161. \$7.50.

A historian writing about a historian who became a statesman presents an intriguing situation. Dr. Heggoy, of the University of Georgia, deals with the prolific historian and archivist, Gabriel Hanotaux, who was the French foreign minister from May 30, 1894, to November 1, 1895, and from April 29, 1896, to June 15, 1898. Affirming that Hanotaux, Théophile Delcassé, and Étienne were offspring of the imperialistic school of Ferry, de Freycinet, and Gambetta, the author's theme is that Hanotaux's motive was the attainment of prestige for France, economic considerations being of no great significance. Carefully balancing his policies, Hanotaux specifically sought the consolidation of all French territories in Africa. Dr. Heggoy states that Hanotaux's method was pragmatic, that is, first creating a problem and then offering to negotiate it and any other outstanding issue. Hanotaux approached each question with a mastery of details—geography, history, and treaties.

He emerges with high marks. In Morocco his introduction of a vice-consul and maintenance of the status quo made possible the eventual French takeover. In the Congo Hanotaux persuaded Leopold II to allow French access to the Upper Nile. Although preferring to keep her as a protectorate, Hanotaux annexed Madagascar. In Tunisia he negotiated the cancellation of twelve conventions with European powers and kept Tunisia as a protectorate. In West Africa he secured Nikki and a large area east of Say on the Niger's left bank. Except for French Somaliland Hanotaux consolidated all French holdings in Africa. His failure was the Egyptian question. Here he is faulted because his customary moderation gave way to challenging Britain, which enjoyed naval superiority.

Unhappily Dr. Heggoy's notes are relegated to the rear where they are closely clustered together on fourteen pages. There is a wide range of sources. Missing are Stengers's Revue Belge articles on the Marchand expedition and John Flint's Sir George Goldie and the Making of Nigeria (1960).

The map of French Africa in 1894 does not pinpoint the French possessions. Ras Makonnen was never a king in Ethiopia, which, mistakenly, is repeatedly called Abyssinia.

For the general public the author presents a convenient summary of a remarkable man's achievements. A moderate Republican, Hanotaux promoted the mission civilisatrice that, for him, entailed consultation with cabinet colleagues and courteous negotiations with European powers. He was a man of his time in not considering the right of Africans to manage their own affairs. Hanotaux's love was France, the country that, sadly, though he lived to 1944, never recalled him for a third term at the Foreign Ministry.

ERVING E. BEAUREGARD University of Dayton

DOMINICK LACAPRA. Emile Durkheim: Sociologist and Philosopher. Ithaca: Cornell University Press. 1972. Pp. x, 315. \$12.50.

Emile Durkheim has been getting a lot of attention of late. Dominick LaCapra offers an explication of Durkheim's thought as it evolved in his major works. This is a hard task. By the time Durkheim came along, lucidity had gone out of fashion in French writing on man and society. His work, highly abstract, vague, and ambiguous, has invited intense controversy.

LaCapra regards Durkheim as as much a moral philosopher as a modern social scientist. The author ascribes Durkheim's ambivalent cast of mind to his position as a transitional figure. A thoroughgoing rationalist—"a Cartesian neo-Kantian" in his early years, when he published On the Division of Social Labor (1893)—Durkheim came increasingly to feel that this position was inadequate, both as a tool of social analysis and as the basis of prescriptive recommendations for reform. In a later period, without abandoning his philosophical stance, he gave freer rein to a dialectical bent and to the preoccupation with religion that culminated in The Elementary Forms of the Religious Life (1915).

Like Marx, Durkheim sought to make sense of the rapid transformation of his own society. LaCapra insists on the importance of understanding Durkheim's distinction, fundamental to all his work, between social normality and pathology. It is unfortunate that these are among the most abstract concepts in Durkheim's vocabulary, and he never got around to applying them to any historical societies. Nevertheless, he clearly regarded his own society as pathological, "beset with varying sorts of internal contradictions." A state of social normality, on the other hand, "was characterized by a highly specific sort of functional integration."

This book belongs to the tradition of intellectual history that concerns itself almost exclusively with the explication of formal systems of thought. LaCapra, his attention riveted on Durkheim the thinker, has little to say about the milieu in which Durkheim lived and worked. But Durkheim was in some respects the perfect sociologist of the Third Republic-in his sympathy for a reformist socialism and his hostility toward revolutionary syndicalism, in his intense patriotism and his preoccupation with order—a pillar of the republican academic establishment as well as a major social theorist. Steven Lukes's monumental Emile Durkheim: His Life and Work, a Historical and Critical Study (1973) restores Durkheim and his circle to prewar France.

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SUZANNE BERGER. Peasants against Politics: Rural Organization in Brittany, 1911-1967. (Written under the auspices of the Center for International Affairs, Harvard University.) Cambridge,

Mass.: Harvard University Press. 1972. Pp. xi, 298. \$11.75.

The author writes that "the modern European state has lived upon a reservoir of soldiers and electors provided by the peasantry, but the peasants have remained the object of politics and not its master." With the modern political organization of the countryside, however, peasant protest assumed two contrasting forms. One was the corporative movement that sought to regulate problems beyond the state; the other was political formations that transmitted peasant grievances to the state. The present study concerns rural corporatives in Finistère, a department of conservative traditions and struggling family farms at the dead end of Brittany. Its most interesting chapters concern the conservative origins of the movement near the beginning of the present century when rural nobles set its foundations in social catholicism: "To tighten the ties which unite the great rural family, to make our associations a vast school of professional solidarity, of la paix sociale, and of Christian charity"—in a word, to advance rural life without disrupting its traditional social relationships. The road led from agricultural syndicalism to professional corporatism, but the results were the same: the elites led, the peasants paid dues, and Finistère remained the same. After 1958, however, a surge of grass-roots activity and direct action suddenly modernized Finistère agriculture and opened a way toward the larger community of France. But the dynamic advance of some farms left others behind, and the resulting social conflicts caused a retreat into the conservative fortress of the "great rural family." New tractors did not change the old politics.

Such are the outlines of this competent work in political science, based on personal interviews and research in departmental and syndical archives. At times Berger attempts to widen the significance of her research by inquiring into the associational behavior of peasant organizations in neighboring areas. Her remarks on left-wing syndicalism are sketchy, but her statistical measurements of the more advanced state of political opinion in Côtes-du-Nord, an adjacent department where peasant protest assumed party-political form, support her conclusion that corporatives obstructed the integration of Finistère into the national political system. The powerlessness of the peasantry, she contends, "reflected more or less deliberate decisions by the political elites, but the policies succeeded only because those organizations that

the peasants built themselves did not challenge—in fact, supported—the exclusion of the peasants from full political participation." In this sense *Peasants against Politics* is a study of politics against peasants.

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JEAN-JACQUES BECKER. Le carnet B: Les pouvoirs publics et l'antimilitarisme avant la guerre de 1914. (Publications de l'Université de Paris-Nanterre. Series A: Thèse et travaux, number 19.) Paris: Éditions Klincksieck. 1973. Pp. 226. 48 fr.

The incident of the Carnet B is a historical event that in a sense never occurred. For decades before 1914 the French government kept a list of suspects to be arrested in case of war. Although the original purpose was to identify spies, by 1909 the list was an elaborate system focused upon labor revolutionaries who might sabotage mobilization. In August 1914, however, the interior minister decided not to invoke the Carnet B, correctly predicting that French labor would prove loyal in the crisis.

Some years ago Jean-Jacques Becker published an interesting if unsurprising paper on this famous list of saboteurs. In response to his letters of inquiry, only twenty-nine out of eightyseven departmental archives could locate papers relating to their local carnets, the other dossiers having been destroyed or never released by the government. The spotty nature of these sources (only forty per cent of the French nationals on the list could be identified, and some industrial areas were not represented) prompted Becker to write an impressionistic description of the more complete departmental records. He found great variation between areas as to the criteria and the care used in compiling the local carnets. The implicated men were known, in general, for their revolutionary syndicalism, anarchism, or Hervé socialism, and the essential criterion for inscription in the carnet was not planning but merely advocating the obstruction of French mobilization.

This present work is an expansion of the previous research to include the more general question of French governmental reaction to labor antimilitarism before 1914. The result is a disjointed union between Mr. Becker's study of the Carnet B and a rapid survey of betterknown events, a survey that serves to place the carnet in context with other events. The author stresses, for instance, the great expansion of the carnet to include syndicalist revolutionaries at

a time when organized labor was constantly clashing with the Clemenceau and Briand cabinets. Immediately before the war, moreover, many syndicalists were removed from the list as harmless, a change that reflected declining radical labor activity. It is unfortunate that Becker's summary of events is so broad that it neither introduces the general reader to the subject (why, for example, does the author fail to note the French Labor Confederation's prominent efforts to make antimilitarism part of the international labor program when such efforts were undoubtedly related to official actions against the Confederation?) nor balances the specialized nature of the Carnet B analysis.

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EVELIO VERDERA Y TUELLS, edited and with an introduction by. El Cardenal Albornoz y el Colegio de España. In three volumes. (Studia Albornotiana, 11–13.) Bologna: Real Colegio de España. 1972; 1972; 1973. Pp. 726; 742; 708.

Sometimes one wonders what might have been if, at a certain point, history had taken another turn. If the papacy had remained in Avignon, for instance, where the supreme pontiffs resided since 1305, how would Europe have developed? What of the future relation of powers between France, Spain, Italy, the Empire and the rest of the Continent? Would Luther have found the Holy See at Avignon as offensive to him as that of Rome? Perhaps the Reformation, Counter Reformation, and innumerable other conflicts, spiritual and military, might never have occurred.

That it came otherwise, that the popes were able to return from their "Babylonian exile" to Rome, is generally accredited to the efforts of Cardinal Gil Alvarez de Albornoz (about 1300-67). A consummate diplomat, military strategist, and legislator, Albornoz, a native from Cuenca, under Alfonso XI had been archbishop of Toledo from 1339 until he exiled himself in 1350 from Castile on the accession of Pedro I, the "Cruel." In 1353, when Innocent VI sent Albornoz as his legate to Italy, giving him full powers to bring the lost ecclesiastical territories back under the authority of the Church, nothing was left of the papal domains except two tiny townships, Montefiascone near Viterbo and Montefalco, in the duchy of Spoleto. The rest had splintered into particularist anarchy. In Rome Cola di Rienzo, the People's Tribune, was still in power. Other cities and principali-

ties in Umbria, the Marches, and the Romagna were held by individual "tyrants," such as the Malatesta and the Visconti. "Single-handed" seems a fitting attribute for the cardinal's achievements: he succeeded on his own in raising the necessary finances and a capable military force. Having taken part earlier in the battle of Salado against the Moors (1340), Albornoz was used, like many Spanish clerics of the reconquista, to fight—figuratively speaking -with the lance in one hand, the crucifix in the other. A master especially of siege warfare (the indispensable method of bringing down those dozens of walled townships and castles), he managed to terrorize central Italy into submission. As the former patrimony of Saint Peter was being restored, Albornoz also wrote a new constitution for the Church of Rome. Often the cardinal of Spain acted, if not holier, more in the popes' interest than the popes themselves. Innocent VI and his successor Urban V, Frenchmen both, were not always in accord with the Castilian's efforts in their behalf. Near the end of his second sojourn in Italy, where he spent thirteen years in all, Albornoz was able to persuade Urban to come to Italy for his triumphant entry into Rome. But the cardinal died on the way, in Viterbo, before he could set foot in the place of his vision.

So, two hundred years before the Spain of Philip II would perfect its domination of disintegrated Italy, this exiled Castilian had already exerted perhaps the most decisive Hispanic influence on that country. Still, in view of the fact that Urban's temporary residence at Rome was followed by rekindled warfare about the Papal States and, thereafter, by the Great Schism, Albornoz's accomplishments might have been undervalued and even forgotten had he not set to himself a lasting monument by founding in 1366 the Spanish College at Bologna.

Seven years ago the college honored the sixth centenary of Cardinal Albornoz's death. The memorial events extended into 1969 with an exhibition of books and a congress of studies of the cardinal. The rector of the college, Evelio Verdera y Tuells, who directs a series of Studia Albornotiana of which these three massive volumes are numbers 11–13, has assembled here various speeches from that congress and augmented the collection by other recent contributions, a total of seventy-five, all dedicated to Albornoz and to the college. Despite this link, the studies by the long international list of writers deal with the most divergent matters and range in time from the twelfth century to

the present. In the opening article, for example, Salvador de Moxó describes the rise of the house of Albornoz from minor provincial nobility to first-line aristocracy under the Trastámaras by the end of the fourteenth century; the following studies of volume 1 examine in a roughly chronological order all possible aspects of Albornoz's activities in Italy, often based on new documentation. Much of volume 2 deals with the college, including biographical studies of some sixteenth-century alumni, such as the doctor Pedro Carnicer, one of the physicians of Emperor Ferdinand I, discussed by Marcel Bataillon who reflects on the Spanish medical profession of that time as pre-empted by New Christian descendants of Jews; Joseph R. Jones sketches one of Emperor Charles V's councillors, the doctor Hernando de Guevara. Volume a has about a dozen papers on the wide scope and influence of Albornoz's contributions to canon law; it contains also the first book-length installment of a major study by Antonio Domingues de Sousa Costa about the Portuguese at the college in the fifteenth century and ends with biographical sketches of some outstanding Spaniards, twentieth-century alumni. The articles, all of impeccable scholarship, should be of great interest not only to historians of Catholicism but also to aficionados of Iberian and Italian political and cultural history.

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N. CAULIER-MATHY. La modernisation des charbonnages liégeois pendant la première moitié du XIX° siècle: Techniques d'exploitation. (Bibliothèque de la Faculté de Philosophie et Lettres de l'Université de Liège, number 192.) Paris: Société d'Édition "Les Belles Lettres." 1971. Pp. 308.

"The study of the modernization of the coal mines of Liège in the course of the first half of the nineteenth century has shed light upon the important phenomenon of the industrialization of a region which has played a capital role in the economic growth of Belgium and it has permitted the specification of several ideas often too vaguely raised in the explanation of that fundamental social transformation which one is still in the habit of calling the Industrial Revolution." Thus, in a sentence characteristic of his prose, Professor Caulier-Mathy states the justification for his book. Readers who would consult it for information regarding "modernization" and the "Industrial Revolution" should

he warned, however, that the author conceives of these notions exclusively in terms of technological innovations. His study begins in the early eighteenth century and follows Liégeois coal mining through the Habsburg, French, Dutch, and Belgian regimes up to 1850. Caulier-Mathy catalogs even the most incidental developments in coal mining technology in abundant, and sometimes excruciating, detail. He supplements his text with twenty-one illustrations, five graphs, and eleven statistical appendixes. There is even a glossary, so that if you have been bothered by what a bouxtay is, or a pahage, or even a tocque-feu, your troubles are over. As Lucien Febvre once objected of a book under review, "Est l'homme dans tout cela?" Caulier-Mathy lays great stress on the role of both engineers and entrepreneurs in initiating the various innovations, but in his account they are only engineers and entrepreneurs, not people. The miners themselves invented nothing of value, and thus they appear largely as the collective breathers of gas and raisers of the annual yield. It may be useful to learn, for example, that the Mueseler lamp replaced the occasionally explosive Davy lamp in 1838, or that Lefebvre d'Hellancourt was probably the real author of the ministerial circular of 18 Messidor of the Year IX. But until we relate such "facts" to human experience, we are not going to learn much more about such subjects as "modernization" and the "Industrial Revolution."

GEORGE FASEL
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OLE FELDBÆK. Dansk neutralitetspolitik under krigen 1778-1783: Studier i regeringens prioritering af politiske og økonomiske interesser [Denmark's Neutrality Policy during the War of 1778-1783: Studies in the Government's Assessment of Political and Economic Priorities]. (Københavns Universitet, Institut for Økonomisk Historie, Publication number 2.) Copenhagen: G. E. C. Gads Forlag. 1971. Pp. 212. 34.50 D. kr.

The American War of Independence, in which France, Spain, and Holland were also involved, affected in no small measure many of the minor European powers. One of these was the dual monarchy of Denmark-Norway, which during the previous century had developed into a maritime nation of some importance and was also an imperial nation of sorts with far-flung colonies in Greenland, the West Indies, West Africa, and India. Like most trading nations

without an actual stake in the conflict, Denmark-Norway desired to stay neutral in the war that commenced between England and France in 1778.

Denmark was to some extent greatly hampered in its wish to reap the benefits that fall to a neutral nation by the fact that it had entered into an alliance with Russia in 1773; Danish maneuverability was indeed greatly restricted, but through the efforts of able statesmen it may be said that the country weathered this particular storm with flying colors.

The so-called League of Armed Neutrality was formed in 1780, and it proved a rather successful undertaking. Denmark, for one, experienced an economic boom from 1780 to 1783, with the shipping and trade industries flourishing. Although the country suffered in many ways and went through a difficult time, the government managed to exploit to the fullest extent the alternatives that flow from a neutral status.

The nation's relations with the outside world were guided, then as now, by three overriding considerations—political, economic, and those relating to security. How the Danish government succeeded in putting behind it what may be likened to an obstacle course, how it weighed the economically desirable against the politically possible, and how it assigned priorities to the various conflicting interests are well brought out in this interesting case study.

The author, who is an associate professor of history at the University of Copenhagen, has produced not only a very readable volume but also a fine contribution to the study of this period in Danish history.

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JOHNNY LEISNER et al., editors. Festskrift til Povl Bagge på halvfjerdsårsdagen, 30. November 1972: Fra en kreds af elever [A Festschrift for Povl Bagge on His Seventieth Birthday, 30 November 1972: From a Group of His Pupils]. Copenhagen: Danske Historiske Forening. 1972. Pp. 400. 95 D. kr.

A miscellaneous collection of articles with an appended bibliography reveals something of both the person honored and its contributors. The bibliographical listing at the end provides a complete view of Bagge's work, which runs mainly to editing—with Aage Friis, documentary collections on Schleswig-Holstein; the Excerpta Historica Nordica; the Historisk tidskrift from 1943 to 1966; and others—and the

writing of a large number of reviews and necrological notices. Bagge also liked involvement in historical and current affairs through writing long articles for both local and Copenhagen newspapers. Povl Bagge's warmth of personality, close comradery with colleagues and students, and his dominant position in the Danish historical profession are indirectly evident, as is also a careful methodology. At the same time the narrowness of Danish seminar methods crops up in the minute, precise articles on small matters.

Predominant in the festskrift are studies of historians and historiography, as in an examination of Erasmus, the theory of kingship and Christian II in the Shibbykrønik, or Niels Pedersen Slange's purported fabrication of a letter concerning an agreement between burghers and king on royal absolutism. Then, too, there are critical treatments of Ludvig Holberg, Gerhard Ritter, Edvard Holm, and Jens Schelderup Sneedorff that provide insights into concepts of the "good emperor," German nationalism and class concepts, politics and diplomacy of eighteenth-century Denmark, and the Danish intelligentsia of the same period. More detailed and typical are studies of voting behavior in the plebiscite on the sale of the Danish West Indies in 1916, negotiations for laying telegraph cables in the Far East, Danish neutrality in 1853, or payment of parliamentary members, "one item on which members of parliaments can agree."

To make sense of this dispersion of subjects from the sixteenth to the twentieth centuries is obviously impossible. Some reference should be made to the excellent interpretation of Gerhard Ritter's philosophy and writing or to the several articles touching on social classes and their relation to politics and religion. An article on slavery and religion in the eighteenth century is, however, both an attack on an unrelated thesis of the contemporary religious leader and writer, Hal Koch ("better western Christian capitalism that East European Communism"), and on the hypocritical attitudes of eighteenth-century clerics. The grotesque aspects of this subject become gross with careful selection of evidence, even if it reveals interests of clergy in converting African slaves.

Where authors get down to "normal" subjects, it is a detailed narrative of documentary evidence. Complications of mergers of cooperative and private slaughteries in the 1890s run out in thin air since the conclusion concludes nothing or the diplomacy of the Crimean War and Danish neutrality eventually mean nothing

either in that war or European politics. They are of some use for Danish history and offer a bit of insight into international matters of more concern to historians of other cultures. The festskrift is an addition to Danish historical memorabilia and to commendation of and celebration for Povl Bagge's seventieth birthday. But its contribution to either European or Danish historical study is marginal both because of topic, language, and content.

As a footnote, it is interesting to eye the first item in Bagge's bibliography: a review of Lawrence D. Steefel's work of 1932 on Schleswig-Holstein. It and other contributions reflect Bagge's interest and knowledge of non-Danish scholars and in particular his many friends among Americans.

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OTTO STEIGER. Studien zur Entstehung der Neuen Wirtschaftslehre in Schweden: Eine Anti-Kritik. (Wirtschaftswissenschaftliche Abhandlungen: Volks- und betriebswirtschaftliche Schriftenreihe der Wirtschafts- und Sozialwissenschaftlichen Fakultät der Freien Universität Berlin, number 28.) Berlin: Duncker & Humblot. 1971. Pp. 235. DM 68.60.

Steiger's book addresses itself primarily to the economist. It contends that the new economics (Neue Wirtschaftslehre) does not initially derive from Keynes's The General Theory of Employment, Interest and Money (1936) but that from 1908 on, the Stockholm School (Knut Wicksell as a precursor and then M. B. Hamilton, Erik Lindahl, Gunnar Myrdal, Bertil Ohlin, and Ernst Wigforss) worked out the principles of the theory. Stress is also put on the English predecessors of Keynes: the Fabians, Beatrice Webb ("The Minority Report of the Poor Law Commission"), G. H. D. Cole, and others. While the theory and its mathematical formulations, particularly with regard to money and prices, are of but marginal interest to the historian, two aspects concern him directly: the author's contribution to the history of economic thought and the part dealing with the influence of the Stockholm School on Swedish politics, especially among the Social Democratic party, between 1908 and 1930.

In the face of depression and unemployment, which constitute the focus of the book and the chief concrete economic issues of the time, the Stockholm School sought explanations for the economic factors underlying the recurrent ec-

onomic crises resulting from oversupply, unemployment, and patterns of savings activities. It maintained that in times of stagnating private industry, it was the duty of the state not to hoard money, balance the budget, and postpone expenditures but to spend, plan useful work projects, and thereby accelerate economic activities (p. 79). This view challenged not only laissez-faire concepts but also the progressive policy of supporting the unemployed by Notstandsarbeiten (though at minimum pay) or by unemployment insurance. It also challenged many postulates of Marxism.

His polemics against Keynes's primacy lead the author to emphasize that when the Socialists came to power in Sweden at the beginning of the Great Depression they harked back with their projects not to Keynes but to the earlier teachings of the Stockholm School—teachings that, to be sure, represented ideas then developing in various places.

The author's concise and competent presentation will stimulate the historian to comparisons with labor policies in Bismarck's time, with German experiences under Brüning, American experiences under F. D. Roosevelt, and programs in Russia, England, and elsewhere. With its accent on unemployment and depression it will also show the difference against recent developments with opposed trends: inflation, devaluations, population increases, new international relationships, and changed attitudes of labor parties and organizations. The historian will do well altogether to occupy himself with the specific issues raised in the book and will greatly profit from it when tracing the historical process underlying the evolution of economic thought.

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PATRICK RILEY, translated and edited with an introduction and notes by. The Political Writings of Leibniz. (Cambridge Studies in the History and Theory of Politics.) New York: Cambridge University Press. 1972. Pp. viii, 206. \$13.00.

That something has not yet happened hardly provides a sufficient reason that it ought to happen. As Leibniz might have said, one can imagine an infinite number of books not compossible, given the inherent limitations of the enterprise, with the best of all scholarly worlds. In the preface to what purports to be a representative selection from Leibniz's political writings, Patrick Riley commends his publisher

for "willingness to revive interesting and unaccountably neglected" material. Although the slender volume is certainly respectable, neither Riley's lengthy introduction nor the relatively few pages of text hitherto unavailable in English are quite enough to inspire complete confidence in his publisher's judgment.

Riley is quite right, of course, that no one will pretend that Leibniz's political writings rival those of his great contemporaries, Hobbes, Spinoza, and Locke. Where political thought is concerned, be it admitted, Leibniz was apparently fully as conventional and prosaic as he was innovative and prescient in mathematics, logic, and dynamics. The question, alas, is not whether the variegated assortment of letters, manuscript fragments, casual remarks, and commissioned exercises assembled here are "worthy of some attention," but rather whether their merit derives exclusively from the fact that they are Leibniz's. If indeed Leibniz's political thought has hitherto suffered neglect, in light of the power of his philosophical ruminations in other areas, one may hardly consider such neglect "unaccountable." Had Leibniz actually produced the "political system" with which he is credited, but which fails to materialize clearly either in Riley's introduction or in the assembled documents, such neglect would remain eminently accountable, if not quite as unexceptionable as otherwise is the case.

It seems only fair to add, however, that had a political system emerged out of the rather amorphous farrage evidenced in this collection, it would no doubt have done so as an integral feature of Leibniz's metaphysical synthesis. Nor is there any reason to quarrel with the attempt to read any aspect of Leibniz's thought programmatically, in terms of his quest for a true philosophia perennis. For, indeed, Leibniz envisioned a universal architectonic structure with God at the vertex and a perfect commonwealth of independently orchestrated, yet concordant voices expressing his glory in ordered sequence throughout. If this is what Riley means when he speaks of Leibniz's "rationalized medieval system," then, doubtlessly, Leibniz's political utterances (such as they were) should be read against that harmonic continuum. In fact, it is possible that one could deduce Leibniz's rejection of any attempt to found sovereignty and right on principles extrinsic to "eternal verities" or truths of reason. Koyré remarks that it is difficult to imagine that there was something Leibniz could not understand. Perhaps the nation-state and concomitant theoretical justifications were among the things Leibniz could not, since he would not, follow, because to do so would be much like explanation on the basis of extensive magnitudes alone—mere mechanism.

For the rest, Riley's selections, however dubious their claim to intrinsic merit, are gracefully translated and well ordered. The introduction, furthermore, amounts to a substantial and confident piece of scholarship in its own right. Finally, specialists should also find the critical bibliography useful.

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HARTMUT KAELBLE. Berliner Unternehmer während der frühen Industrialisierung: Herhunft, sozialer Status und politischer Einfluss. With a foreword by Otto Büsch. (Veröffentlichungen der Historischen Kommission zu Berlin, number 40. Publikationen zur Geschichte der Industrialisierung, number 4) Berlin: Walter de Gruyter. 1972. Pp. x, 302. DM 68.

In his study of Berlin entrepreneurs in early industrialization the author attempts to reorganize some of the traditional concepts applied to the phenomenon. These are treated under two broad subheadings: first, the relationship between entrepreneurship and upward social mobility and, second, the effects of industrialization on the political influence of entrepreneurs. The assumption of authors like Werner Sombart and Joseph Schumpeter that entrepreneurs rose from all classes and were more mobile than now is effectively disproven for the Berlin case by simple statistical analysis. More than half rose from some kind of merchant class, and perhaps one-fourth developed from craftsmen classes in transition to more paraprofessional occupations, for example, in the machine tool industry.

Dr. Kaelble's statistics also make clear that most of the entrepreneurs who originated in Berlin were already mercantile to begin with and that there was very little opportunity for upward social mobility for most of Berlin's citizens. Somewhat less than half of the entrepreneurs migrated to Berlin from other parts of Germany. Many of them were recently emancipated Jewish businessmen and bankers, who made up about half of the middle group among the Berlin enterpreneurs. The author overlooks the fact that upward social mobility does not occur as a result of drastic changes achieved in one generation. According to contemporary sociologists like Bernhard Barber, upward mo-

bility is predominantly to contiguous classes. One could expect most Berliners to aspire to more than a one-step rise only if the educational system provided the opportunity. Obviously it did not. Most of the entrepreneurs studied were near millionaires who rose from a contiguous merchant or banker class. Little was done with the petty entrepreneurs, although some of them seem to have expanded their workshops to middle-sized plants in light industry or in service industries.

As the author is not acquainted with the archival material on economic development for the earlier centuries, he tends too easily to assume that nineteenth-century phenomena herald a new appearance. Thus by the mid-nineteenth century he sees the rise of a new vocabulary and a new consciousness of the factory owner, Fabrikant. However, many of the words he selects were already in use in the eighteenth century, both in state papers and in merchant handbooks, whenever there were already factories with machine equipment as, for example, in the early cotton printing industry.

In regard to the political influence of entrepreneurs, the author has a tendency to generalize on the basis of too few facts. He concludes that the entrepreneurs did not withdraw from politics after the Revolution of 1848 because they formed organizations and entered parliament, the Prussian diet. However, the two leagues in question (a free-trade society and a welfare league) failed soon after the Revolution. There was only one entrepreneur in the Prussian diet in the early 1850s, entrepreneurial representation on parliamentary commissions declined markedly, and the Berlin Kaufmanns Korporation fell into the hands of a conservative oligarchy with little real influence. By the 1860s the majority of the Berlin entrepreneurs seem to have been "radical" progressives (favorable to the Fortschrittspartei)—a significant change deserving more study and explanation than is offered here.

HELEN P. LIEBEL
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ADOLF M. BIRKE. Bischof Ketteler und der deutsche Liberalismus: Eine Untersuchung über das Verhältnis des liberalen Katholizismus zum bürgerlichen Liberalismus in der Reichsgründungszeit. (Veröffentlichungen der Kommission für Zeitgeschichte bei der Katholischen Akademie in Bayern. Series B: Forschungen, number 9.) Mainz: Matthias-Grünewald-Verlag. 1971. Pp. xviii, 135.

In the foreword Birke remarks that, despite general realization of Bishop Ketteler's antiliberal position, studies of him have concentrated largely on his social concerns. Birke undertakes to set Ketteler in place "as representative of the Catholic Church of Germany, in the history of the development of political Catholicism," and to consider how his early antiabsolutist attitude became increasingly antiliberal.

The book is a well-organized discussion of the problem, much less defensive than Ludwig Lenhart's recent biography of Ketteler. There is a good account of Ketteler's aristocratic antiabsolutism and the maturing of his views of society and politics through study of Thomas Aquinas and contemporary Catholic thought, particularly German and French, and through his participation in public affairs. The study then follows the development of Ketteler, who in the early days of the Frankfurt National Assembly sat with the far Left, to the time when he practically identified liberalism with absolutism.

Despite some new material and careful combing of published sources, the answers are not clear. One asks how "liberal" Ketteler ever was, considering his background and training, and the central role of religion in his thinking and actions. His only speech at Frankfurt dealt with schools; it stresses forcefully German unification-not that the liberals would have disagreed, but that his concept was more romantic than modern, and the Christian-Germanic theme, like his enthusiasm for Stände and Genossenschaften as forms of social and economic life, suggests updated medievalism. His increasing hostility to liberal individualism and economic self-help, to the "pulverizing of humanity" through unlimited freedom of occupation, trade, and movement, and his view of liberalism as a "mechanically rationalistic concept"-all these from Die Arbeiterfrage und das Christentum (1864)—lead to doubts whether he could have allied with liberalism beyond the realm of temporary tactics in which he included political parties, elections, and trade unions.

After 1848 liberalism gradually replaced for him absolutism as the enemy; it combined the mechanical and the bureaucratic, the ruthless centralization of old-style absolutism with egoism in economics and morals; it enforced "liberties" that he regarded as harmful. Somewhat curiously, it merges with his fear of "Borussianism," so that during the Kulturkampf he regarded the attack on the Church as the result of a conspiracy, with Bismarck's support, of Freemasons, Jews, and liberals. The appendix,

incidentally, includes two commentaries by Ketteler on Bismarck's actions at that time, viewed as tactical moves in service of "monarchistmilitary absolutism."

Large questions remain. Was German liberalism after 1848 so sharply different from before as to justify Ketteler's change of view? Was a lasting Catholic-liberal alliance in Germany really possible? How much needed still is a thorough analysis of German liberalism in the nineteen century!

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GÜNTHER BORMANN and SIGRID BORMANN-HEISCH-KEIL. Theorie und Praxis kirchlicher Organisation: Ein Beitrag zum Problem der Rückständigkeit sozialer Gruppen. (Beiträge zur soziologischen Forschung, number 3.) Opladen: Westdeutscher Verlag. 1971. Pp. 381. DM 24.80.

This is an example of what appears to be a new species of book in the social sciences. Authors and publisher have combined to make it as hard as possible to discover what is being said, why, and on what authority. The fruit of scholarship is hoarded as a treasure and defended against the curious reader with every weapon in the arsenal. Written in atrociously stilted and opaque sociologese, poorly printed from typewriter composition, equipped with a footnote apparatus so bizarre it must be seen to be believed (1.0001.554.0000.0—eleven pages of this, four columns to the page—to 8.1355.049.0204.2), this volume not only makes reading difficult, it positively prohibits it.

But I had agreed to review the book, and so I labored through it. The yield, at least for me, was small, certainly incommensurate with the enormous theoretical and mechanical edifice erected by the authors. What we have is a sociological analysis of the organization of the evangelical church in Germany today, specifically of its increasing alienation from the secularized environment in which it must function. This estrangement provides the authors with their problem. How can the church relate to its surroundings while remaining true to its goals? The authors propose reorganization and provide a model for it, but before they do so they subject the church's present structure to examination. Heavy theoretical artillery is moved up at once (Alvin Gouldner, Talcott Parsons, Amitai Etzioni, Georges Gurvitch, and others) for the obligatory opening chapters on social organization and the theory of organized groups. The relevance of these considerations to what follows remains as obscure as their language.

What follows in the major part of the book is a detailed description of parochial organization, pastoral duties, and the problems of relating the organization to its functions. Here the authors rest their discussion on interviews with 105 pastors in selected parishes in Württemberg. We discover that rural parishes predominate in the organization, that attitudes prevailing in these shape the ideas of most pastors (hence the Rückständigkeit of the subtitle: they lag behind the rest of society), and that this explains the inadequacy of the church to its surroundings (Umweltinadäquatheit). Stuck in the rigid frame of its original model, the church cannot relate to the society it wishes to guide. The work of pastors has become so formalized, problems of communication between pastors and people and within the organization so complex (though not too complex to prevent the authors from expressing them in a mathematical formula), and leadership patterns so hierarchical that the bureaucracy has lost touch with real life outside the structure.

All this is set out in massive detail and ponderous prose, enlivened all too rarely by illustrative figures that are fun to read as concrete poetry. The authors' solution to the problem of the church, a restructuring of ecclesiastical organization on the principle of functionality, is displayed in a particularly pleasing diagram on page 326. Cut it out of the book, color some of the little squares with magic markers, and it will be suitable for framing.

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STANLEY B. KIMBALL. The Austro-Slav Revival: A Study of Nineteenth-Century Literary Foundations. (Transactions of the American Philosophical Society, New Series, volume 63, part 4.) Philadelphia: the Society. 1973. Pp. 83. \$3.50.

Because of the Enlightenment and especially the Romantic period in literature, several Slavic nations living in the Austrian Empire in the nineteenth century developed a national self-awareness. There is an eleven-page chapter on Serbia, an eighteen-page chapter on Bohemia and Moravia, a ten-page chapter on Croatia, a fifteen-page chapter on Slovakia, and eight pages on the Slovenian awakening. The author explains that a greater variety of organizations accounted for the relatively longer Slovakian

chapter. Since the Poles never experienced an interruption of their language and literature, they are not included.

One should note that the first national language magazine was the Serbian Ljetopis, which was printed in Novi Sad. In regard to the tables on pages 71 and 72, I would like to point out that the Serbian organization called matica had by far the greatest financial resources. Also, the Czech-Moravian matice was the largest one in terms of the number of members. One can also observe peculiar spellings, such as "Sour-kromma Spolecnost Ucena," that is, "Private Learned Society," since the Czech language was spoken more in villages than in towns.

In Croatia the matica movement first had to decide which of the three dialects-namely Stokavski, Cakavski, or Kajkavski-should become the literary language. It is interesting to note that Emperor Ferdinand forbade the use of the term Illyrian. While the Croats had a relative degree of autonomy, the Slovaks experienced the toughest Hungarian oppression. At that time there were three linguistic orientations in Slovakia. One favored a linguistic union with the Czech language, another favored a western Slovakian dialect, while a third one was that of central Slovakia, which became the basis of the Slovak ortography. Slovenia, which was the smallest Slavic nation living in Austria, had the greatest proportion despite their small number of books published. Also, despite these limited numbers, they still did not display the worst financial resources.

How shall I criticize the book? First, let me stress that Dr. Kimball's visits to Czechoslovakia, Yugoslavia, and Russia were very fruitful. I was impressed by his ability to correctly spell names in different Slavic languages. However, it is probably insufficient to refer to the Ruthenians only on two pages. Since they were a part of the Austro-Hungarian Empire, I feel that more information should have been provided. Second, the nations of Czechoslovakia (the Czechs and the Slovaks) and the nations of Yugoslavia (the Serbs, Croats, and the Slovenians) should also have been discussed. The author could have pointed out more of the similarities rather than the dissimilarities between the Czechoslovak or Yugoslav ethnic groups.

I must, as a final evaluation of Kimball's book, emphasize that I appreciate not only his detailed search for names of the small organizations started by the *matica* in different Slavic nations, but also his description of these groups in terms of financial resources, number of

members, frequency of periodicals, and quantity of books published (as far as one could have established this). This more or less quantitative information gives us not only an insight into Central Southern Europe in the nineteenth century, but it also invites readers to think of similar aspects in other societies of the world in the nineteenth century.

JIRI KOLAJA
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ALEXANDRU DUŢU. Cărțile de înțelepciune în cultura română [Books of Wisdom in Romanian Culture]. (Academia de Științe Sociale și Politice a Republicii Socialiste România. Institutul de Studii Sud-Est Europene. "Biblioteca Istorică," 34.) Bucharest: Editura Academiei Republicii Socialiste România. 1972. Pp. 167. Lei 11.

The current concern of Romanian historians with the study of the Enlightenment and the pre-Enlightenment is manifested in Alexandru Duţu's book. Duţu, one of the leading Romanian specialists associated with the Institute of Southeast European Studies of the Romanian Academy, has summarized the latest data on "books of wisdom" published in the Romanian provinces in the seventeenth and eighteenth centuries.

The summary consists of an annotated list of books on deportment, political and civil, a lengthy commentary on works on political theory published in the eighteenth century, and a chronological compilation designed to ascertain the exact dates of composition of the titles listed. Three "complementary studies" on topics peripherally related to the books of wisdom but nevertheless concerned with the Enlightenment round out the volume.

Dutu propounds the theory that the Romanian publications of the pre-Enlightenment and Enlightenment periods, while directly imitative of Western prototypes, contain elements unique to the historic experience of the Romanians. Concern with the role of the individual and of the citizen, for instance, is more pronounced in Romanian than in non-Romanian writings. Similarly, the Romanian historical tradition is evident in the theological rather than philosophical context of secularization.

There can be no quarrel with Duţu's conclusions, which are self-evident. The book, however, is more valuable as an analytical reference work than as a critical interpretation

of Romanian civilization in the seventeenth and eighteenth centuries.

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Istoriia Rumynii [History of Romania]. Volume 1, 1848-1917, edited by v. N. VINOGRADOV et al.; volume 2, 1918-1970, edited by N. I. LEBEDEV et al. (Akademiia Nauk SSSR, Institut Slavianovedeniia i Balkanistiki.) Moscow: Izdatel'stvo "Nauka." 1971. Pp. 667; 741.

A period of about 120 years of Romanian history is covered in two volumes of over fourteen hundred pages, a massive work by a team of Soviet historians, with space provided for richness of detail and for broad panorama as well as comprehensive surveys and penetrating analyses of events presented in their proper context. This work is a valuable addition not only to Romanian history of the nineteenth and twentieth centuries, but also to the history of the adjoining Central and Southeast European regions. The multilingual bibliography that is included, if added to those already available in English in works dealing with Romania, probably represents a conclusive compilation of sources for the period for the Romanian regions as well as the Banat, Bukovina, and Dobruja, but not for Bessarabia, which is treated somewhat apart.

This work is well organized, and each chronological period is examined from three viewpoints: international affairs and diplomatic history; domestic affairs, government, the history of political parties and movements, and social and economic history; and cultural history. This organization allows for examinations of regional as well as national developments,. and this book is given an additional dimension by the inclusion of a number of political developments and movements, especially in the nineteenth century, that remain somewhat tangential in their significance for the history of Romania proper, but are by no means without significance for the history of Southeastern Europe as a whole. The reference here is, of course, mainly to those political movements in the mid- and late nineteenth century, which glowed briefly in the then Habsburg provinces of intertwined ethnicity, so that the movements themselves were a part of the Serbian or Hungarian, as well as Romanian history. This, too, is a valuable contribution.

On the other hand, this work is marred only

too frequently by rather transparent efforts to make past events fit preconceived interpretations. A local strike, a protest march, or an angry article in a local newspaper are made to appear in instances too numerous to list, and in a manner that renders them suspect of pseudodialectical approaches, as harbingers of future revolutionary movements elsewhere or other developments. One wishes that more space and a fuller coverage were accorded, for instance, to Romania's part in the Second Balkan War and to the Treaty of Bucharest of 1913, or to Romania's role in the Little Entente, the relationship between the decline of the Little Entente and the rise of General Ion Antonescu's dictatorship, Romania's adherence to the Tripartite Pact, and her role in World War II.

The Soviet view of Romania's role in World War I, and of the forces that had brought her into the war in August 1916, deserves attention. The blame for ending Romania's neutrality and her declaration of war against the Central Powers is placed on the "Anglo-French insistence" that Romania join the Allies, rather than on the pressures brought to bear upon Romania by the government of Prime Minister Boris Stürmer of Russia. The subsequent, perhaps predictable, chain of events-Romania's quick near-collapse, the doubling of the length of the Russian front, the collapse of tsarism and the start of revolutionary developments in Russia, as well as Romania's separate peace treaty of February 1918 followed by her redeclaration of war in November of that yearis, in a way, made to appear as a product of the alleged "Anglo-French demands." They no doubt existed, but they could not exonerate the Stürmer government of its responsibility for a policy of such far-flung effects.

The flow and ebb of tides of war is unavoidably a major part of any history of Romania. If the beginning of World War I in Romania is discussed in one given manner, the end of World War II is described in details that are not without fascination. The king, a number of generals, and the representatives of the middle-of-the-road political parties developed a feverish, increasingly desperate activity toward the middle of 1944 in an attempt to remove Romania from the Nazi alliance and avert Soviet occupation. After the Red Army's rout of the Nazi armor at the Battle of the Kursk Salient, Romanian envoys in the Near East tried, in moves described vividly, to persuade the Western Allies to dispatch an airborne expeditionary force to Romania. On its landing the Romanians would arrest Antonescu, declare war on Germany, and thus place themselves on the Allied side. Nothing of course came of this. Their next step was to secure a region centered on Bucharest that would be exempt from the Red Army's occupation-but the text is silent concerning how and from whom this was to be obtained. This failed, too. By August 23, with the Red Army in Romania, the decision to overthrow Antonescu was finally made: "Realizing the hopeless position of the Fascist clique, the king arranged for I. Antonescu's arrest. . . . Under the pretext of a Cabinet meeting, the members of the government were assembled in the palace in the evening of August 23rd, and arrested." It was neither the army, nor the police, nor the royal guards that arrested them. "An armed unit of patriots arrested the Rumanian dictator and his collaborators, and took them from the royal palace to a secret apartment on the outskirts of Bucharest, belonging to the Central Committee of the Rumanian Communist Party, where they were held until they could be turned over to the Soviet command."

Thus came the end of one of the major supporters of nazism in Eastern Europe—taken away by armed patriots through a capital city held by his Nazi masters to a "safe" apartment. Strange endings to a drama can take place anywhere; in Romania, as in Southeastern Europe, they illustrate the intensity and power of forces bearing upon the area from east and west, and north and south—depending on the period of history—and sweeping before them men who tried, often desperately, to stop or to deflect what could be neither halted nor diverted.

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CHRISTOS THEODOULOU. Greece and the Entente, August 1, 1914—September 25, 1916. (Hetaireia Makedonikon Spoudon, Idruma Meleton Chersonesou tou Aimou, 129.) Thessaloniki: Institute for Balkan Studies. 1971. Pp. iv, xxiii, 379. \$5.00.

The war that was to be over by Christmas 1914 had escalated terrifyingly by 1915. To reduce the pressures on the western and Russian fronts, the Entente powers gave much thought to the idea of a third front in the Balkans. Gallipoli was one diversion. Another was raised by bringing Bulgaria in on the

Allied side. This volume deals with the pressures also put on Greece and the difficulties of maintaining neutrality in the face of them. Step by step Greek sovereignty was reduced by the occupation of Salonika, the seizure of Corfu and other islands, the threat of economic sanctions, and the criticism of the king's "unconstitutional" behavior. Constantine eventually forced to abdicate, and Greece was taken into the war by Eleutherios Venizelos, the mini-imperialist who wanted a greater Greece encircling the Aegean. A Foreign Office official writing in 1915 provides a text that explains why Entente behavior toward Greece was variously threatening, deceitful, cajoling, and insensitive. He wrote that "Bulgaria is the key to the Balkans. . . . Greece will come in if she does." King Constantine did not like it a bit. Neither does Dr. Theodoulou.

His volume is mostly documents, whether in the text or the footnotes. The greater part are from the British Foreign Office archives, released over the last dozen years. French documents are used less because of the "erroneous way" in which the French saw events in Greece. Bolshevik documents are left out because of their "dubiousness." These seem poor explanations. Nor are the archives of the Central Powers used, and this causes difficulties, in spite of the book title, in interpreting the attitudes of the Greek court. The court may have been pro-German or merely neutralist. The documents here do not, and cannot, prove the issue one way or the other.

Theodoulou's use of the documents has led him to provide a commentary, intended as an interpretation but in effect only a rudimentary frame. The material in the footnotes, which are voluminous, often seems to contain more critical material than the text, and it in many places refuses to sustain the argument of the author. By the end of the book one is left wondering if the documents should not have been published in a chronological block, with a detailed introduction. There are many signs of haste, as if the significance of the documents had yet to be fully assimilated before the author began to write. Examples of this are the long footnote on page 26 and also the footnoted material on pages 237 and 251.

The author also overlooks the valuable material already published that would have helped him bring out the views of Lloyd George, Grey, and Joffre, among others, and give life and substance to these people. The occupational risk of diplomatic history is the tendency to disconnect documents from men.

The author proposes to avoid the biases of the Venizelist and royalist camps in Greek historiography, but he often reveals the bias of the indignant patriot. The performance of Entente diplomacy provides ample justification for this. In places, too, the author speaks the language of Venizelos (pp. xxi, 12) and retells many of the irritations all Greeks must have felt with their imperious guests. His views on Napier, Fitzmaurice, and Elliot (pp. 151, 152, 177) are highly partial and suggest an unfamiliarity with these men in other phases of their careers. One hopes, nevertheless, that Theodoulou, having dug so deeply in some archives, will persist in his intention to explore those of the Central Powers. This will allow him to produce a more balanced account than this one and to make a signal contribution to the history of Greece in this highly complex and important period.

The book contains many spelling mistakes and typographical errors, and pages are bound out of order between pages 55 and 72.

ALLAN CUNNINGHAM
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Istoriia rabochikh Leningrada [History of the Leningrad Workers]. Volume 1, 1703-fevral' 1917, edited by s. N. VALK et al.; volume 2, 1917-1965, edited by A. R. DZENISKEVICH et al. (Akademiia Nauk SSSR, Institut Istorii SSSR, Leningradskoe Otdelenie.) Leningrad: Izdatel'stvo "Nauka." 1972. Pp. 554; 459.

The two volumes of this work trace in approximately one thousand pages the experience of the working class of St. Petersburg-Leningrad from 1703 to 1965. It is the product of a research group at the Leningrad branch of the Institute of History of the USSR of the Academy of Sciences, undertaken to remedy the palpable absence of a work so detailed and chronologically extensive, and to set straight the record against the distortions of bourgeois historians of the Russian proletariat. Its function is best understood in the light of a statement in the conclusion that it "reflects the glorious path of the working class of our country, and reveals its role as a leading force of socialist society." One can understand that approach—the working class of Leningrad has indeed played an important part in the history of Russia and the USSR, and can easily constitute individually and collectively the hero(es) of the story—in the early growth of. the workers' movement, the Revolution, and during the heroic days of the siege of Leningrad in the Second World War. The names of many of the individual heroes are appropriately sprinkled through its pages.

For a student of Russian history, however, this work is probably of only slight interest or value. The approach is basically narrative and descriptive, with incident following incident for two and a half centuries. There are extensive citations of archives, but insofar as I could judge, these sources seem not to have been exploited to advance any new interpretations. Indeed most of the raw material seems to have been standard sources on Leningrad history. The specialist may well find some nuggets of information-chronologies, names, incidents, etc.—as in the description in the second volume of the counterattack by the Central Committee against the Leningrad opposition at the time of the Fourteenth Party Congress in 1925. But the general tone of the book is to rise above most controversies, taking the standard line on any issue and avoiding polemics.

Indeed what is missing for the generalist is any effort to examine significant theses or interpretations regarding the big themes—the issue of the working class in tsarist development policy (as explored, say, by Zelnik in his Labor and Society in Tsarist Russia) or in the struggle over the role to be played by labor unions in Soviet society. In the extensive literature on labor in Russia and the USSR, on economic development, on Leningrad, this seems much less than a landmark work. It attests to the great industry of its authors, but is directed and motivated by no very exciting scholarly quest.

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L. M. IVANOV and M. s. VOLIN, editors. Istoriia rabochego klassa Rossii, 1861–1900 gg. [History of the Working Class of Russia, 1861–1900]. (Akademiia Nauk SSSR, Institut Istorii SSSR.) Moscow: Izdatel'stvo "Nauka." 1972. Pp. 317.

IU. I. KIR'IANOV. Rabochie Iuga Rossii, 1914-fevral' 1917 g. [The Workers of Southern Russia, 1914-February 1917]. (Akademiia Nauk SSSR, Institut Istorii SSSR.) Moscow: Izdatel'stvo "Nauka." 1971. Pp. 306.

A. A. MUKHIN. Rabochie Sibiri v epokhu kapitalizma (1861–1917 gg.) [Siberian Workers in the Period of Capitalism (1861–1917)]. Moscow: Izdatel'stvo "Mysl'." 1972. Pp. 334.

The level of Soviet scholarship in the area of

Russian labor history has been rising significantly over the past several years. Two of the three books under review are illustrations of this welcome trend, while the third reminds us that old constraints continue to be felt, particularly when Bolshevik relations with the labor movement are at issue. Extremely impressive is the collectively written History of the Working Class of Russia, of which the "responsible editor" is the late L. M. Ivanov, who has provided so much of the guidance to the recent achievements in labor history within the Academy of Sciences' Institute of History. "Collectively written" is not quite accurate, for, with the exception of chapter 2 ("The Beginning of the Road," which is collectively authored by B. S. Itenberg, the late Iu. N. Shebaldin, and Ivanov), each chapter is attributed to a different author (chapter 1, "The Rise of the Working Class," to Ivanov; chapter 3, "On the Road to Unification with Socialism," and chapter 4, "The Rise of the Mass Social-Democratic Labor Movement," to Iu. Z. Polevoi). Even the collective chapter is divided into sections whose individual authorship can be readily identified by readers familiar with the authors' previous works, and the hand of Ivanov is clearly visible in certain sections of chapters 3 and 4 (notably pp. 152-60 and 258-65). For a collective effort of this kind, the book is written in an unusually smooth and readable style, a pleasant surprise that is probably not unrelated to the authors' relatively flexible and open approach to their subject.

Although footnote references are mainly to primary sources, the book does not purport to be a work of original research, but a synthesis of previous publications by the authors and certain of their colleagues. Indeed, the contribution of Polevoi, which comprises the bulk of the book (pp. 129-308), goes over much of the ground covered in his earlier Zarozhdenie marksizma v Rossii (Moscow, 1959), albeit with greater flexibility and thoughtfulness. The authors succeed in covering a chronological range of four decades, all industrial areas of the empire (including those inhabited by national minorities, although these, for defensible reasons, receive considerably less attention that St. Petersburg and the Central Industrial Region), and a broad range of diverse topics such as the transformation of peasants into urban workers, the evolution of spontaneous labor unrest into a politically conscious labor movement, and the parameters of tsarist labor policy. As a synthesis the book is unlikely to be rivaled in the foreseeable future. While to the research scholar it is no substitute for narrower, more detailed monographs (such as the two other books under review), this is easily the most useful book to assign to graduate students as an introduction to the best Soviet scholarship in labor history.

The overall framework is a familiar one. Reduced to its starkest outline, it is (except for the rigid treatment of "economism") fairly unobjectionable, but not very promising. The emancipation of 1861 launches Russia into the period of capitalism (already anticipated in the 1830s and '40s), laying the groundwork for the transformation of the semipeasant labor force into an economically and socially identifiable but politically still unconscious proletariat ("class in itself" but not yet "for itself") by the late 1880s and early 1890s. In the 1870s a mounting wave of labor unrest accompanies this process, but although the strike now becomes the labor movement's characteristic weapon, its primary features are its "spontaneity," confinement to immediate economic grievances, and narrow isolation to particular factories or, at best, particular regions. Only the most rudimentary expressions of class consciousness begin to appear. Marked by intensive unrest at the Morozov factory (1885) and other textile works of the Central Industrial Region, the labor movement in that part of the country advances in the mid and late 1880s to a new and higher stage, characterized by its stubbornness, the unity and solidarity displayed among workers from different factories, and the more generalized character of workers' demands. But it is only during the industrial upsurge of the 1890s, and especially after 1895, that a combination of objective economic conditions and the influence of Marxist leadership brings about the transformation of much of the working class (the boundaries are never described with precision) into a "class for itself," offensive rather than defensive in its basic demands, national rather than regional in its outlook, and prepared, despite the growing "economist" danger of the late 1890s, to engage simultaneously in political struggle against autocracy and class struggle against capitalism.

The most valuable feature of the book is the authors' willingness to enrich this framework by highlighting the significance of certain "peculiarities" (osobennosti) of Russian conditions as determinants of the process they describe. The main "peculiarity" is, of course, the social, economic, and political backwardness of the conditions in which the Russian labor movement evolved. In the words of the authors, "capitalism developed swiftly, but along its

path stood vestiges of a feudal legacy [that is, serfdom and autocracy] which had not been completely liquidated. Autocracy as a form of government, the estate privileges of some, and the people's lack of political rights came into increasing contradiction with the entire course of historical development. Hence the sharpness of social-economic contradictions, the oppressive situation of the masses" (p. 7). Although there are occasional attempts to reduce the concept of osobennosti to the truism that there are bound to be some differences in the essentially similar paths followed by the working classes of different countries, these statements are largely pro forma. For it is precisely the analysis of uniquely Russian conditions that provides the book with its essential freshness and attractiveness. Having acknowledged the importance of Russia's preindustrial legacy as an obstacle that the nascent labor movement had to overcome, the authors, however, fall short of taking a final and more daring step: approaching that legacy as a condition which contributed to the successful spread of revolutionary Marxism among the workers of Russia. This suggestion is delicately advanced from time to time (namely, in the discussion [p. 226] of objective conditions that impeded the influence of Russia's "labor aristocracy" and in the conclusion [pp. 304-05]), but the brakes are invariably applied before the argument can be developed fully. It is as if the real challenge were to analyze the unique manner in which the workers of Russia reached a historical moment shared by all the proletariats of Europe, rather than to explain why, in fact, they followed a separate path.

Kir'ianov's detailed study of the workers of the Southern Industrial Region (primarily the Ykaterinoslav, Kherson, and Kharkov provinces and the Don basin, centers of mining, metallurgy, and machine building) covers a later chronological period, the years of World War I to the eve of the February Revolution, but the flexible approach is similar to that of the volume discussed above. (Not surprisingly, L. M. Ivanov was also "responsible editor" of Kir'ianov's monograph.) One of its best features is an explicitly critical attitude toward the earlier Soviet literature on this topic. Thus Kir'ianov takes his predecessors to task, for the most part effectively, for their exaggeration of Bolshevik and denial of Menshevik influence on the working class, their refusal to acknowledge the degree of "chauvinist" or patriotic sentiment among southern workers, their neglect of the spontaneous, economic side of the labor movement, and their overly schematic presentation

of the rise of working-class consciousness under Bolshevik leadership from calendar year to calendar year. He is happy to concede that historians began to retreat from these onesided positions in the 1960s, but he modestly refrains from noting the important role that his own articles have played in this process. The present work incorporates most of Kir'ianov's previous publications in this area, but it draws upon a wider base of archival and published materials to strengthen his case. He has wisely chosen to organize his materials topically rather than chronologically. He has produced what will become, one hopes, the model for future regional studies of the wartime labor movement in the Russian Empire.

It is unfortunate that Kir'ianov's model is not emulated in Mukhin's study of the workers of Siberia. Mukhin's tone is more reminiscent of an earlier period of Soviet historiography, and his approach to statistical materials, often used as illustrations of a major generalization without regard to the global figures, is less than satisfactory. This is not to suggest that the book is useless. Its strength lies in those sections where the author concentrates on the formation and economic situation of the Siberian working class (primarily railroad workers and miners), its weakness in his discussion of the workers' struggle and the role of political movements, especially Bolshevism. The former sections illuminate the situation of Siberian workers within the context of the region's industrial backwardness relative to other parts of the empire; the motley character and widely dispersed location of Siberian workers are well depicted. The weaker sections roughly correspond to the second half of the book (pp. 171-335), which covers the years from the eve of the 1905 Revolution to October 1917. It is noteworthy that this long chapter, which relies much more heavily than the earlier sections on archival materials, is the most uncritical in its use of sources. Instead of closely analyzing the crucial documents, the author tends to draw from them just those quotations that serve his line of argument. It is impossible to judge from this approach whether conflicting evidence was evaluated and disregarded or simply ignored, but surely the author's conclusion that the workers of Siberia by 1917 constituted a "class for itself" has not been convincingly demonstrated. Indeed, his discussion of the war years, despite the presentation of interesting data here and there, is open to some of the criticisms that Kir'ianov applies to earlier Soviet historiography of the period.

Both of the books favorably reviewed here

would have benefited by dropping certain outdated terminology that is confusing at best, misleading at worst. Why apply the adjective "trade-unionist" to the spontaneous economic struggle of workers who do not even enjoy the luxury of a trade-union movement (Istoriia rabochego klassa)? Kir'ianov similarly detracts from his work when he mechanistically identifies the Mensheviks with the "petty bourgeoisie," especially when one considers how undogmatically he distinguishes the leftist, antiwar segment of the Mensheviks (and Socialist Revolutionaries) from the more moderate, prowar, and defensist tendencies.

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s. M. SAMBUK. Revoliutsionnye narodniki Belorussii (70-e—nachalo 80-hh godov XIX v.) [Revolutionary Populists in Belorussia (From the 1870s to the Early 1880s)]. (Akademiia Nauk Belorusskoi SSR, Institut Istorii.) Minsk: Izdatel'stvo "Nauka i Tekhnika." 1972. Pp. 244.

This small book is about intelligentsia revolutionists in the Belorussian provinces of the Russian Empire from the 1870s to the mid-1880s. On the one hand Susanna Mikhailovna Sambuk wishes to demonstrate that Belorussian groups were derivative of the Russian Populist movement, and at that time the dominant radical force throughout the Empire. Ideological and organizational leadership came mainly from St. Petersburg radical circles. She is inclined to work from gross and unsubtle generalizations about Russian Populism: for example, that all Populists adhered to the same socioeconomic outlook; almost no alteration or evolution occurred over the years; all Populists denied that the objective conditions for capitalist development existed in Russia; and not a grain of socialism resided in the Populist programs of the early 1880s.

On the other hand she wishes to demonstrate that particular indigenous circumstances shaped Belorussian radicalism and eased the transition to Marxist socialism there. The peasant commune (obshchina) hardly existed in these western provinces. Urban workers thus played a greater role in party programs than they did in other provinces. And, more important, Belorussian activists rarely established settlements among the people in the countryside. Instead they preferred to exert influence through intermediaries, usually workers and peasants in the larger cities.

The author is obligated to pass carefully through the treacherous question of Belorussian nationalism. Ouotes from the Lenin literary corpus provide judicious aid to navigation. But the wearisome and unhistorical passages where she "corrects" the "errors" of first one then another misguided Belorussian nationalist do not successfully remove those confusions and contradictions caused by her effort to strike the correct balance between Russian revolutionary internationalism and Belorussian national specifics. This is clearly a touchy business. Of particular interest in this regard is her discussion of a largely anonymous organization, Gomon, that was criticized at the time—unfairly, she says—for its narrow Belorussian nationalism.

The author devotes attention to the social basis of Belorussian activism. But one wonders about the pertinence of her categories and statistics. Nearly twenty-five per cent of her subjects could not be identified according to "social gradation," and thus appear in her tables as "unknown" or "without a defined occupation." The reader might still choose to applaud her effort. So little social history of this sort is done in the Soviet Union. Especially praiseworthy is her brief search for the factors that compelled individuals to take up the revolutionary cause. Concentrating on A. S. Boreisha, she discovers answers that derive at least in part from the immediate experience of the intelligentsia revolutionists themselves rather than exclusively from the moral outrage occasioned by the suffering of the folk.

Although the author works from a static and simplistic model of revolutionary Populism, she is more discriminating in her view of the historical evolution of Belorussian activism. She places accent on the Populist contribution to Russian Marxist ideology and tactics. Despite the "errors" of the earlier revolutionists, their efforts suggest that the Soviet state possesses what another scholar, G. G. Vodolazov, has called a "native lineage." This is a welcome departure from earlier Soviet historiography that insisted on an unconvincing, sharp disjuncture between Lenin and the traditions he inherited. This, too, is a touchy business.

Much of the author's information is derived from archives and would be more accessible to other researchers if the book possessed an index.

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P. F. LAPTIN. Obshchina v russkoi istoriografii poslednei treti XIX—nachala XX v. [Society in Russian Historiography in the Last Third of

the 19th and the Beginning of the 20th Century]. (Akademiia Nauk Ukrainskoi SSR, Institut Istorii.) Kief: Izdatel'stvo "Naukova Dumka." 1971. Pp. 297.

L. P. Laptin's book is an involved and tersely written polemic against several prominent nineteenth-century Russian medievalist historians: I. V. Lychintsky, P. G. Vinogradoff, M. M. Kovalevski, and D. M. Petrushevski. The author examines closely the major works of the above Russian historians that are devoted primarily to medieval and eighteenth-century studies. He attempts to show that these medievalists have erred in interpreting the European feudal experience, that they have been too greatly influenced by their own bourgeois prejudices, particularly their fear of the French Revolution and European revolutionary conditions in the late nineteenth and early twentieth centuries.

The author devotes several sections to copious and sometimes confusing quotes from Marx and Lenin in order to present the correct Marxist-Leninist interpretation of his polemic. In my opinion this book should be used for reference only.

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BRANKO LAZITCH and MILORAD M. DRACHKOVITCH. Lenin and the Comintern. Volume 1. (Hoover Institution Publications 106.) Stanford: Hoover Institution Press. 1972. Pp. xiii, 683. \$17.50.

Writing the history of the Communist International presents unusually onerous problems. First of all so many countries, Communist parties, and personalities are involved that few scholars have sufficient knowledge to deal with this complex subject. In addition the archives of the Comintern are securely locked away in the Institute of Marxism-Leninism in Moscow and are inaccessible even to Soviet scholars. Although a great deal of material published by the Comintern and the various Communist parties is available it is often deliberately misleading, or is written in an Aesopian language that is difficult to decipher. Many of the Comintern's activities, moreover, were secret, and information about them was systematically suppressed. Finally, both the revolutionary and the clandestine aspects of the Comintern naturally made it the subject of wild rumors and unsubstantiated stories. In view of these obstacles it is not surprising that few scholars have attempted to chronicle this intriguing world of conspiracies, demonstrations, uprisings, factions, purges, "united fronts," "popular fronts," and so on. And in fact the best general history of the Comintern, Franz Borkenau's World Communism, despite its faults, has not yet been superseded, even though it was published thirtysix years ago.

The present authors have devoted many years to research and writing about the Comintern: Mr. Lazitch is the author of Lenine et la IIIº Internationale, A Biographical Dictionary of the Comintern, and Les Partis communistes d'Europe, 1919-1955, while Mr. Drachkovitch edited The Revolutionary Internationals and (with Lazitch) The Comintern: Historical Highlights. They have also managed to gain access to many confidential, unpublished documents, such as minutes of meetings of the Presidium and Executive Committee of the Comintern, letters from Comintern officials, manuscript memoirs by important Communists in France and Germany, the personal papers of Boris Souvarine, and the archives of Paul Levi. In addition the authors have built up their own collection of rare Comintern documents, as indicated by the numerous footnotes that end with the comment, "in the authors' possession." (These include such items as original letters by Karl Radek and Clara Zetkin.) And they have benefited from the counsel of former participants in Comintern activities, including Pierre Pascal, M. Goldenberg, Boris Souvarine, and Bertram Wolfe.

It is not surprising, therefore, that this volume is richly documented and abounds in fascinating details. Of particular interest is the new information about some of the underground activities and organizations of the Comintern, including the short-lived Western Bureau, located in Amsterdam, and the more important Western European Secretariat in Berlin, which was scarcely even whispered about in Comintern literature and that has seldom been discussed before by scholars. Equally interesting are the accounts of the official and unofficial representatives that Moscow sent to spy on the leaders of the Communist parties in Western Europe. The long chapter entitled "The Initial Apparatus of the Comintern," which deals with these and other matters, is probably the book's most original contribution.

Less novel are the sections on the early history of the French and Italian parties—subjects that have already been explored in monographs by Annie Kriegel, Robert Wohl, and John McKay Cammett. Still, this is the most comprehensive work yet published on the Comintern as a whole for the years from 1914, when the Third International was just a dream in

Lenin's mind, to the spring of 1921, when he inaugurated an important change of course. The authors promise that a second volume, carrying the story to the death of Lenin in January 1924, will be forthcoming shortly.

THOMAS T. HAMMOND University of Virginia

K. V. GUSEV and V. P. NAUMOV, editors. Velikii Oktiabr' v rabotakh sovetskikh i zarubezhnykh istorikov [The Great October Revolution in the Work of Soviet and Foreign Historians]. (Akademiia Obshchestvennykh Nauk pri TsK KPSS, Kafedra Istorii SSSR.) Moscow: Izdatel'stvo "Mysl'." 1971. Pp. 226.

The October Revolution in Russia has never received the exhaustive and many-sided treatment accorded to the French Revolution of 1789. From this collection of historiographic essays by Soviet scholars it is all too apparent why. Attitudes toward the October Revolution tend to be polarized along political lines. In non-Soviet studies authors, who do not, as a rule, have access to the archives, tend to be hostile toward the event and question its legitimacy or lawfulness (zakonomernost'). Soviet scholars by way of contrast, who appear to have all the resources at their fingertips, write onedimensional political histories in which the Bolsheviks occupy the center of the stage and play all the parts.

There are five essays in this collection, all by leading Soviet scholars. Two of the articles deal with Russian research, the first on the October Revolution in Moscow, and the second on the split in the Socialist Revolutionary party. The other three articles survey foreign scholarship on the October Revolution in France, the United States, and Latin America respectively. Those articles dealing with Soviet works tend to be descriptive, rather than critical, and those treating non-Soviet research tend to measure it by ideological, rather than scholarly criteria.

The essays are interesting to the non-Soviet scholar because they display the trends in research around the world on the October Revolution, and because they put into bold relief some of the most important new directions. The Kachurin essay, for example, on the historiography of the October Revolution in Moscow shows how Soviet scholars since World War II, and especially after 1956, have given more attention to the Provisional Government, the non-Bolshevik parties, and to the local communities—trade unions and district soviets, for example—that made up the city. This is an

updated version of an essay Kachurin published in 1967. According to the editors it was included because there are still many unexplained and debatable issues about the interpretation of the Moscow phase of the Revolution. But Kachurin is not quite candid about the debates, which revolve around those Soviet scholars honest enough to admit that the Moscow Bolsheviks were sharply divided over the need for an insurrection and poorly prepared for the necessary military action.

M. M. Uzakov's essay on American scholar-ship on the causes of the October Revolution illustrates the general tenor of the three surveys of non-Soviet research. Uzakov correctly observes that most scholars in the United States seek to demonstrate the illegality or illegitimacy of the October Revolution, but he then undermines his case by chastizing Americans for using all the sources, Socialist-Revolutionary as well as Bolshevik. The scholars mentioned in this essay seem to be chosen somewhat at random, and Uzakov makes few qualitative judgments between them. Fainsod, Daniels, and Ulam are lumped together with Tompkins, Ethel Ewing, and Crankshaw.

On balance the two essays on Soviet scholarship are the most valuable parts of the book, chiefly for the convenient inventories of recent Soviet research that they provide.

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NAUM JASNY. Soviet Economists of the Twenties: Names to Be Remembered. (Soviet and East European Studies.) New York: Cambridge University Press. 1972. Pp. ix, 217. \$12.50.

RICHARD B. DAY. Leon Trotsky and the Politics of Economic Isolation. (Soviet and East European Studies.) New York: Cambridge University Press. 1973. Pp. 221. \$10.95.

When Naum Jasny died in 1967 he was the doyen of Russian emigré economists. His post-humous book on non-Communist economists is organized around the 1931 "trial" of the Mensheviks, though some who did not appear (notably V. A. Bazarov) are also included. This is a very personal book on Jasny's part, especially in its tribute to the memory of V. G. Groman. But its value would also seem to lie in direct proportion to the reader's prior knowledge about the period. It is marred by lapses of fact (such as the assertion that "the peasant land commune" was "introduced into legislation in 1861") that the author would presumably have corrected.

Richard Day has written an extremely interesting study of the political argument on economic policy during the 1920s among the Communists. He postulates alternative strategies for growth as dependent on the basic assumption whether the Soviet economy was to be seen as integrated with, or isolated from, the rest of the world, especially with the tool-producing capitalist West. In particular, Day argues that Trotsky's hard-line approach in 1920 on the mobilization of labor flowed from his belief in the economic isolation of the Soviet republic. During the 1920s Trotsky is supposed to have been converted to the notion that Russia could not be considered in total separation from world capitalism and that she could actually benefit by trade with the West. Day feels as well, quite properly, that "Socialism in One Country" was beneath Trotsky's contempt as a theoretical issue.

The novel framework that Day has established to re-examine the issues of the 1920s works well to explain many points of interest, such as the convoluted positions taken by Sokol'nikov, the commissar of finances. Day is also very good in explicating the real differences between Trotsky and most of the Marxist-Leninist opposition associated with his name. In particular the economic program of Preobrazhenskii is shown to be less a variant on Trotsky's own program than a substitute for it, with little in common between them. Further, while some of the contradictions inherent in "Socialism in One Country" have been evident since the slogan was formed in 1924, Day has made clear that the bloc of Bukharin and Stalin, which made the slogan respectable, was unstable from the very beginning because of the differences fundamentally embedded in the program and not merely because of the incompatibility of its adherents. While Bukharin took the isolation of Soviet Russia to mean the necessity of compromising with the peasant and gradualism, Stalin saw it as the justification for a forced drive of industrialization that far exceeded anything the "Trotskyite" opposition ever anticipated. Indeed, a major contribution of Day's book is precisely to stress the anxiety of Trotsky, in 1926 and 1927, to avoid the anticonsumptionist bent of stepped-up industrialization into which he saw even Bukharin slipping.

While Day's general scheme is conjectural it is also stimulating and enlightening. One part is questionable, namely the idea that Trotsky's pronouncements from exile on the theory of "Permanent Revolution" were a direct falsifica-

tion of his views in 1904-06. There is certainly a hiatus in Trotsky's own interest in the matter, from the Revolution until about 1924. Nonetheless Trotsky at sixty, with whatever fond corrections, could properly claim the stripling of twenty-five as a legitimate ancestor in his opposition to the Stalinist regime.

In any case Day's fine book should help stamp out the illusion that "Permanent Revolution" was an adventurist alternative to the sobriety of "Socialism in One Country." And it should as well generate a discussion to locate those issues in a more fruitful perspective than we have had until now.

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## NEAR EAST

OLEG GRABAR. The Formation of Islamic Art. New Haven: Yale University Press. 1973. Pp. xix, 233, 131 plates. \$17.50.

Islamic art is an elusive phenomenon. Since it does not strive for the spatial, the pictorial, the iconographic, or the symbolic density of Western arts, its meaning eludes us. Our problem is further compounded by the baffling diversity of Islamic arts, by the paucity of evidence and the element of chance in our knowledge of artifacts, and finally by the conceptual problems that attach to the term Islamic. What is Islamic art and what is Islamic about it? What are its origins? What does it mean to say? Oleg Grabar, in this book of exceptional subtlety and taste, surveys and extends his own important contributions to the study of early Islamic art history and works out an original and imaginative approach to the elusive and complex problems of understanding Islamic art.

Successive chapters unfold Grabar's way of thinking about the meaning of Islamic art. A historical survey sets Islamic monuments in the context of the Arab conquests and the quality of past civilization in each of the provinces settled by the Arabs. The new art, he hypothesizes, expresses the attitudes, the mentality, the wants, and the needs of a new people. Early Muslim arts represent a "conscious attempt to relate men meaningfully to the conquered world by islamizing forms and ideas of old" (p. 72). Such monuments as the Dome of the Rock and the city of Baghdad are symbols of Muslim supremacy over the religions and empires of the past and of the Muslim appropriation of the past in order to form a specifically Muslim identity as an outgrowth of the historic cultures.

To express their cultural situation the Muslims created a visual imagery in tension between the old and the new. Grabar reviews their religious and secular creations in thoughtful detail. He shows how the form of the mosque derived from a concept of space for worship that goes back to the time of the prophet Muhammed. The form was now organized by the use of architectural elements borrowed from Byzantine and Christian usage. The use of these elements, however, was governed by a Muslim concern to wrest the architectural motifs from their specifically Byzantine and Christian context of associations, to exclude the use of representations because they were central to the Christian art tradition, and above all to reject the notion that art as such was a symbolic mediation between man and ultimate reality. In Muslim hands the elements of Byzantine and Christian art were organized into a new Gestalt to create specifically Muslim associations and meanings. By contrast secular arts, as illustrated by the Umayyad princely palaces, borrowed Near Eastern motifs of power and luxury without thought of fitting them to Islamic attitudes, but they utilized these motifs as they were utilized in mosque architecture—without great attention to form or to iconographic programs. Functional and social rather than esthetic, formal, or symbolic concerns dominated the structure and decoration of the palaces.

In early Islamic art formal artistic concerns were subordinate to ornamentation. "Total covering, relationships between forms, geometric motifs, infinite potential growth, freedom in the choice of subjects, arbitrarinesssuch appear to be some of the salient characteristics of early Islamic ornament" (p. 202). What kind of art is this? Grabar sees it as the expression of an attitude toward reality itself. The superficiality, the arbitrariness, and the unreality of the visible world symbolize the transcendence of the divine reality, the Muslim's acceptance that God's ordinance for the order of the world is inscrutable, and his retreat from imitating his maker. Muslim art, he concludes, is essentially an expression of the Muslim attitude toward the very process of creation that underlies the natural universe and the world of man-made artifacts. Given the peculiar nature of Islamic art, the study of stylistic developments, formal problems within the specific genres of art, iconography, and traditions of symbolism—even the study of what place art has in a culture as a whole—can only be realized in conjunction with the study of the functional Near East 147

significance of art and the mentality that underlies its production and its conceptualization in the mind of the Muslim observer.

At this point the art historian becomes a cultural historian in search of the spiritual universe of Islam. If Grabar has sacrificed, on the one side, some of the art historian's love of specific objects of beauty and, on the other side, some of the cultural historian's inclusiveness of concerns, he has given us a stunning example of the creative possibilities that inhere in the tension between the study of concrete materials in accord with a specific discipline and reflection on the meaning of the specific subject for the civilization as a whole.

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OTHMAR PICKL, editor. Die wirtschaftlichen Auswirkungen der Türkenkriege: Die Vorträge des 1. Internationalen Grazer Symposiuns zur Wirtschafts- und Sozialgeschichte Südosteuropas (5. bis 10. Oktober 1970). (Grazer Forschungen zur Wirtschafts- und Sozialgeschichte, number 1.) Graz: Selbstverlag der Lehrkanzel für Wirtschafts- und Sozialgeschichte der Universität Graz. 1971. Pp. 366. Sch. 400 (\$16.00.)

Saying that a collection of essays by different authors in a single volume is "uneven" has become so commonplace that one is almost embarrassed to put the word on paper. In the present case the contrasts in quality are particularly striking. On the one hand, Professor Pickl must be commended for publishing some outstanding material. Especially good is his own meticulously documented, well-illustrated contribution, "The Effects of the Turkish Wars on Trade Between Hungary and Italy in the Sixteenth Century." The longest piece in the book, it suggests that financial rather than military considerations brought about a change in commercial patterns. On the other hand, one wonders why the editor even bothered to print half of the other articles included, some of which are only barely relevant to the theme. Was he personally obliged to participants in a symposium for which he was the host?

The prospective reader is perhaps best served by merely listing the titles (sufficiently descriptive per se) of those selections that I found really novel and informative. The honors go to the South Slavs and Magyars, if one excepts Pickl himself, and Hermann Kellenbenz (Nuremberg), who offers a useful, mainly factual summary of secondary literature in "Southeastern Europe Within the Framework of Europe's Overall Economy." Sergeji Vilfan's (Ljubljana) "Economic Effects of the Turkish Wars from the Standpoint of Ransom Payments, Taxation and Price Movement" is both brilliant and fascinating. Lajos Rúszás (Pecs) delineates the development of the market village in transdanubia under Turkish rule, while his conational, Ferenc Szakály (Budapest), examines the question of continuity in the economic structure of Hungarian market villages under Turkish control. István Kis (Budapest) turns to the human aspect of economic history in "Society and the Army in Hungary During the Era of the Turkish Wars (the Soldier-Peasants)." Finally, Fedor Moačanin (Zagreb) writes on "The Problem of the Landed Property of the Military Population Along the Croatan and Slavonian Frontiers." In summary, the worthwhile features of the book are enough to warrant its acquisition, especially by persons interested in the field but who read only German.

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STANLEY E. KERR. The Lions of Marash: Personal Experiences with American Near East Relief, 1919-1922. Albany: State University of New York Press. 1973. Pp. xxv, 318. \$15.00.

The Lions of Marash is an engrossing personal tale of adventure and violence written by a former American relief official and retired chemistry professor at the American University of Beirut. In January 1919 the slim young author left the United States Army Sanitary Corps and began service with the private organization, Near East Relief, at Aleppo, Syria. Kerr transferred to Marash in Cilician Turkey in autumn 1919. The contents deal primarily with events at Aleppo and Marash from January 1919 through July 1920. The book's dramatic focus, to which nearly one-half of its pages are devoted, is a bloody two-month period in the winter of 1920. In that time Turkish nationalists successfully confronted the French occupiers of Marash. Armenians resisted the Turks, having sought independence through French protection and American philanthropic support. During the confrontation Turks killed nearly fifty per cent of the twenty-four thousand Marash Armenians.

The most vital contribution of this work is its extensive narration of the French-Armenian experience with the Turkish uprising at Marash. The numerous specifics provide the

same kind of intensity as Truman Capote's In Cold Blood, minus the psychological probing. Kerr's accent is on the wave of destruction as it involves building after building. With the aid of a map of Marash and a portfolio of photographs, the reader witnesses Armenians seeking to defend their most secure houses, churches, schools, orphanages, hospitals, and the quarters of American and European missionaries and relief workers. One Armenian wrote what his Turkish neighbor told him had happened at the house of the Armenian's parents: "We finally broke in. A group of peoplefifty or more-were huddled together in one room. Your father asked permission to read from a book and to pray before surrendering . . . He stood up, read and prayed, and then we slaughtered them all-men, women, and children. We didn't waste any bullets on them! We killed them with axes and picks." Kerr quotes an Armenian's diary: "Two days ago the Turks set fire to the orphanage and burned the three hundred fifty orphans in it. Their cries still pierce my heart. We could not go to their aid because of the barbed wire and the enemy machine-gun fire." The partially armed Armenians often inflicted casualties upon Turks. The end of the Armenian resistance came during a frightfully cold threeday period in February 1920 when the French evacuated Marash and one thousand of thirtyfour hundred Armenian refugees froze to death in the mountain passes on the road out of Marash.

The title of the book inappropriately implies an evenly balanced impartiality. Lions, the author writes, stand for courageous Turks, Armenians, and French. There are a number of scattered attempts to present Turkish leaders and their views, the chief view being that the nationalist struggle in Marash was the important opening victory in the Turkish drive to eliminate foreigners and their Armenian clients from Anatolia. The author attempts to avoid repeating most traditional pro-Armenian propaganda, as found in such a work as that of his relief colleague at Marash, Mabel E. Elliot's Beginning Again at Ararat (1924). One of his problems is that his highly detailed data come largely from his own letters to his family and from memoirs and diaries of French, American, and Armenian people. Partly because he consulted almost no Turkish primary sources, his tale takes on many of the emotions of the non-Turkish materials. Usually avoiding negative stereotypes about Turks (e.g., the terrible Turk label) and simple martyr images

of Armenians, the author also loses much of the pathos found in Elliot's recollection. He might better have edited eyewitness sources on the Marash episode.

Kerr's chief frame of reference for the Marash incident is the military strategy of French and Turks. His thesis is that the French possibly would not have lost Marash if they had possessed a wireless. At the risk of his life the author helped begin negotiations for a French-Turkish armistice, which might have averted a French withdrawal. But just as the negotiations were about to succeed, the French acted on orders to pull back; these orders were not accurate, it later turned out. The inaccurate instructions could have been clarified, withdrawal suspended, and re-enforcements dispatched if there had been a wireless. A counter to this thesis is that the French probably would have lost Marash anyway because of the Glemenceau government's lack of will to defend it and because of the Turks' strength.

The leading difficulty of the book is that the Marash episode is not tied adequately to larger military and peacemaking processes during the First World War period. The complex intermingling of the communal system of the Ottoman Empire with Western intervention is touched upon too lightly. Often the story is a loosely connected set of summaries of different local incidents as recorded by various participants. Some attempt is made at filling contextual gaps by the introduction by historian Richard G. Hovannisian and by the first six chapters, which rather awkwardly prefix the primarily personal description that follows.

The book does not recognize how America's idealistic absorption with the Armenian question and the Wilson government's procrastination in handling it contributed to the Marash situation. Since Kerr directed American relief and married an American missionary he met in Marash, it is interesting that he did not use the latest scholarly works on Near East Relief and on the missionary role in Wilson's Near East policy: Robert L. Daniel's American Philanthropy in the Near East: 1820-1960 (1970) and this reviewer's Protestant Diplomacy and the Near East: Missionary Influence on American Policy (1971).

In sum, the book is valuable for the large number of French, Armenian, and American sources from which it draws, and for a poignant, well-phrased story.

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Near East 149

RIAZUL ISLAM. Indo-Persian Relations: A Study of the Political and Diplomatic Relations between the Mughul Empire and Iran. (Sources of the History and Geography of Iran, number 32.) [Teheran:] Iranian Culture Foundation. 1970. Pp. xxiv, 287.

This book is Riazul Islam's revised and amplified version of his doctoral thesis for the University of Cambridge, and it deals with the period from 1510, with the Mongol emperor Baber as yet ruling only the small kingdom of Kabul, to the middle of the eighteenth century. It covers a significant chapter in the history of the two nations when India and Iran became great powers under the rule of two outstanding dynasties, the Safawids of Persia and the great Mughals of India.

The intercourse between India and Iran was many-sided, and it covered politics, diplomacy, culture, literature, trade, commerce, and religion. In fact, as the author points out in his preface, "the Mughuls were not involved so deeply with any other foreign power, whether in friendship or otherwise." But the author concentrates only on the political and diplomatic relations of the two dynasties, and he provides an informative, thoroughly documented chapter in a captivating period in the history of two fascinating nations.

Historians have dealt with the Mughal-Safawid relationship, but from William Erskine on, all modern historians of the Mughal Empire have been primarily concerned with a particular period or a particular emperor, such as Sukumar Ray's work on Humayun's sojourn in Persia or the unpublished thesis by M. Jahangir Khan (The North-West Frontier Policy of the Mughuls) that deals with Mughal relations with Persia during the reign of Akbar, Jahangir, and Shah Jahan. Mr. Islam's study covers the entire period from Baber to Huhammad Shah, and in the author's own words, it "treats the subject independently in its own right and not as part of a broader canvas."

The author's use of primary sources, manuscripts, and archival materials is indeed very impressive. The book represents a thorough and painstaking research with some fresh insights and information for scholars in this field. It is not aimed at the casual reader or the layman, and thus one is indeed indebted to the Iranian Cultural Foundation for sponsoring its publication. It is a pity, however, that the author's Calendar of Documents on Indo-Persian Relations, often referred to in the text, is still unpublished. The Calendar is, from what the author describes, a collection

of numerous letters and documents—scattered in chronicles and manuscripts in various libraries and elsewhere—that have bearing on the political and diplomatic relations between the Mughal emperors of India and the Safawid shahs and Nadir Shah of Iran. The two works are indeed complementary studies, one making a historical study of the subject and the other bringing together the documents on the subject. One hopes that the *Calendar* will soon be published, too, for the sake of interested scholars.

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JON KIMCHE. There Could Have Been Peace. New York: Dial Press. 1973. Pp. xx, 359. \$8.95.

This is an opinionated and disjointed but enlightening volume by a London journalist who has already published extensively on the Arab-Zionist confrontation. It is not so much a coherent book as a series of "scoops" loosely strung together on the thread of the author's contention that there could have been peace if only Arab and Jew had understood one another and had not allowed themselves to be led down the garden path by the great powers of the day-the British and the French, especially the former, during World War I and its aftermath and by the Americans and the Russians since the Six-Day War of 1967. The first and more interesting section of the volume, "The First Chance" 1917-23, is divided into three chapters. The first retells the familiar story of British Middle Eastern policy with stress on the role of Sir Gilbert Clayton, the director of intelligence in Cairo and the chief political officer with Allenby's forces in Palestine. Kimche advances evidence from the Clayton papers at Durham University to support the argument that Clayton's "grand design" of a new deal for both Arabs and Jews under British tutelage had more substance than the better-known but erratic proposals of Sir Mark Sykes. The second chapter is based largely on revelations made to the author by Israel Sieff, a prominent British Zionist who worked closely with Chaim Weizmann. Kimche takes Weizmann to task for failing to realize, first, that the Zionists could not rely on support from Britain, which had many greater strategic concerns, and, second, that it was imperative to take seriously the burgeoning nationalism of the Arabs in Palestine. The third chapter sketches the hardenng opposition of the Palestinians and makes extensive

use of the files and recollections of Aref el-Aref, a close collaborator of Amin el-Huseini, whose political capacity in those early days, Kimche argues, has been unduly overshadowed by his later notoriety as the pro-Nazi mufti of Jerusalem.

After a rather naively muckraking "interlude" on oil diplomacy during and after World War II, part 2, the "Last Chance?" 1967-73, focuses on the domestic and international difficulties of Israel under the premierships of Levi Eshkol and Golda Meir. Kimche contends that both of them, like Weizmann, underestimated Palestinian nationalism and were too ready to cast Israel in the role of a client of the West, in this instance the United States rather than Britain. But the force of his argument is weakened by his reliance on unnamed, highly placed sources, by his hero worship of Moshe Dayan, and by the fact that the outcome of the Yom Kippur War has eroded the author's assumption of Israel's unqualified military superiority.

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## **AFRICA**

JAMES W. FERNANDEZ et al. Africa & the West: Intellectual Responses to European Culture. Edited by PHILIP D. CURTIN. [Madison:] University of Wisconsin Press. 1972. Pp. x, 259. \$12.50.

The overseas expansion story so familiar to all up to a generation ago was wholly onesided because it was told by the conquerors within their own frame of reference. It has changed drastically in our own time with subjectee evidence being increasingly admitted. But one essential element in evolving reappraisals has still been conspicuously lacking in many cases—the weighing of native thought. This must be given full consideration in every discussion bearing upon conquered peoples in all quarters, and, until this receives adequate attention, the revisionist movement must fall far short of its chosen goal. The simple fact is that because of environmental differences, the mental outlooks and thought processes of semi-isolated, underdeveloped groups differ drastically from those of their foreign rulers.

We have here an excellent work on crosscultural history aimed at establishing perspective and better understanding in studying Euro-African relations. The seven essay authors include anthropologists, historians, and a literary critic, all associated with the 1969 Conference on African Intellectual Reaction to Western Culture. Selected items will illustrate radically different viewpoints inviting strife between victor and his beaten foe.

Among the Bantu-speaking Fang, who are recent arrivals in the Gabon-Guinea-Cameroons country dealt with by James Fernandez, disputes had traditionally been handled by elders viewing each case as an individual one and rendering decisions accordingly. But alien administrators dispensed "justice" according to rigid lawbooks, a socially irresponsible approach from the local angle. The long resident BaKongo of the subequatorial grasslands discussed by Wyatt Mac Gaffey had completely variant theories of time and space from the Westerners, who concluded that these natives lacked powers of abstract thought and hence were an inferior people.

Africans along the central Atlantic rim were long in close association with outsiders. As Leo Spitzer and Jean Herskovits demonstrate, Europeans viewed the transplanted Westerneducated Sierra Leoneans as black Englishmen whose responses must naturally match those of their British counterparts. Yet, as the writings of these authors amply show, the black Englishmen continued under the sway of Yoruba assumptions both in Sierra Leone and in the Lagos settlement. The coastal Senegalese were under powerful French influence for over four centuries, those communally-born actually becoming French citizens. Nonetheless, G. W. Johnson, Jr. illustrates that their intimate ties with indigenous African culture prevented them from becoming fully assimilated Frenchmen. Senghor's poetry is employed by Harold Scheub to illustrate the tragic plight of black Africans seeking in effect to become white Europeans by mere adoption of Western civilization.

Editor Curtin's summarizing chapter holds that thought differences between masters and subject peoples elsewhere have been no less striking than African ones, and he suggests comparative studies on a world basis. This is indeed a provocative book opening a wide field of intriguing potentialities.

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PROSSER GIFFORD and WM. ROGER LOUIS, editors. France and Britain in Africa: Imperial Rivalry and Colonial Rule. New Haven: Yale University Press. 1971. Pp. xix, 989. \$30.00.

, Africa 151

France and Britain in Africa is a collection of papers presented at a conference sponsored by the Yale University Concilium on International Studies in March 1968. Like an earlier volume, Britain and Germany in Africa, produced by the same editors in 1967, it is a massive compilation. There are twenty-four contributors: ten from the United States, five from France, five from Britain, and one each from Belgium, Canada, Nigeria, and South Africa. Most of the current authorities on European imperialism in Africa are represented.

The volume is divided into three parts. The first, "Imperial Rivalry," begins with an overview by Henri Brunschwig of the causes and nature of French Anglophobia in the nineteenth century. This is followed by papers on Tunisia (Jean Ganiage), Egypt (Agatha Ramm), the Congo (Jean Stengers), the Berlin Conference (Wm. Roger Louis), the Fashoda incident (G. N. Sanderson), and the Anglo-French Entente of 1904 (Pierre Guillen). John Hargreaves supplies a short comparative survey of British and French imperialism in West Africa; Colin Newbury an informative and very detailed description of the two powers' tariff policies in the same region; and André Kaspi a discussion of French aims and ambitions in the continent during the First World War. A conclusion to part 1 is provided by Brunschwig, who likens the unfolding of the story of Anglo-French imperialism in Africa to a stage play-a charade, one gathers, rather than a drama-and sums the whole matter up in unequivocally Eurocentric terms: "It thus appears that the partition was completely achieved by Europeans living, thinking, and reacting within their European framework" (p. 405).

Most of the papers that make up "Imperial Rivalry" are exercises in traditional imperial history. We hear of the "official mind," of the "policy makers" among the politicians and bureaucrats in Paris and London, of "men on the spot" like Archinard and Lugard who acted first and sought permission afterward. Several papers draw attention to individual "permanents" employed by one or another home government department who, because of their control of the budget and of the inflow of information from Africa, were able to push through "forward policies" more or less singlehandedly, often keeping their colleagues in other departments in the dark about what was going on until it was too late for effective objections to be raised. Above all, the point is repeatedly made that nothing African was ever allowed seriously to jeopardize the essential unity of interest that joined France and Britain together in Europe. Some incidents, such as the British occupation of Egypt in 1882 and the Fashoda confrontation of 1898, provoked a certain amount of sabre rattling, but this was never allowed to go very far. Neither France nor Britain could permit their colonial rivalries to become really inflammatory because the shadow of a united and increasingly powerful Germany lay over them both.

All this is very much the stuff of which the history of European imperialism has been made for the past century or so. There is, it is true, a change of attituce. In contrast to the imperial, and anti-imperial, historians of an earlier day, the writers of these papers adopt a rigorously neutral posture, apportioning neither praise nor blame. Sticking closely to their sources, they are concerned more with chronology and narration than with analysis. As a result, while they supply a wealth of information, much of it new, on the "how" of French and British imperialism in Africa, they do not tackle in any coherent or sustained way the much more difficult question of "why."

Part of the difficulty is that the official sources, while providing a detailed picture of the decision-making process within the relevant metropolitan ministries, do not often indicate what outside influences were at work on the decision makers. When a minister made a public pronouncement on a projected course of action, for example, how much of what he said was rhetoric? And how much was rationalization? To what extent was "the national interest" (so often used as a justification) in fact the interest of a particular pressure group? How many decisions were taken because someone gained the ear of a government decision maker in private and at a crucial time? The absence in the official record of the "monopoly capitalists" of Hobson-Leninist theory does not mean that either they or their influence did not exist.

Part 2 of the volume, entitled "Colonial Rule," contains a set of comparative Anglo-French studies: of military operations in the Western Sudan (A. S. Kanya-Forstner), of the nature and results of economic exploitation (David Fieldhouse), of educational policy (Prosser Gifford and Timothy C. Weiskel), and of the workings of the mandate system (Ralph Austen); articles on the French (William B. Cohen) and British (Robert Heussler) colonial services and styles of government; and four papers that deal with French Africa alone: Boniface Obichere on "the African factor" in

the establishment of French rule in West Africa, Hubert Deschamps (at his best heremagisterial, witty, and anecdotal) on French colonial rule in general, D. Bruce Marshall on the Free French in Africa, and David C. Gordon on Algerian labor in France after the achievement of Algerian independence. The section concludes with a "perspective" by Leonard Thompson. This is a brief but stringent criticism of the approaches his colleagues of part 2 have taken to the topic of colonial rule. Thompson makes three main points: that it is time historians of the imperial era moved the locus of their inquiries from the metropoles to the colonies themselves, that what the French and British thought they were doing in Africa is less important than what they actually did, and that it is the effects of colonial rule—the results of the interaction between rulers and subjects—that should be studied nowadays in preference to "policy" and "method."

Part 3 consists of a bibliographical essay by David E. Gardinier on the literature of French colonial rule in Africa during the period 1914 to 1960. One hundred and sixty-four pages long, it is a comprehensive guide to the relevant source materials and will be of great value to researchers. It is a pity that the editors were not able to arrange for the 1870–1914 period to be similarly covered.

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MICHAEL CROWDER. Revolt in Bussa: A Study of British 'Native Administration' in Nigerian Borgu, 1902–1935. Evanston: Northwestern University Press. 1973. Pp. 273. \$13.50.

BONIFACE I. OBICHERE. West African States and European Expansion: The Dahomey-Niger Hinterland, 1885-1898. New Haven: Yale University Press. 1971. Pp. x, 400. \$15.00.

Within the past decade a number of studies have appeared that have illuminated considerably the practice of indirect rule in colonial Nigeria, but none have dealt with its effects in a specific area as fully as Professor Crowder's volume on the brief revolt in Bussa in 1915. The incident, of no great importance to Nigerian history as a whole, is nevertheless of substantial importance as an illustration of how badly a well-conceived theory of governing can work when it falls into the hands of incompetent or careless individuals.

Bussa, the capital of the relatively small

emirate of Borgu on the western boundary of Northern Nigeria, was an unprepossessing little town whose historical importance in precolonial times belied its appearance. Its king was an important traditional figure who commanded the allegiance of the rulers of substantially more important centers of the emirate.

With the settlement in 1898 of competing British and French claims to the area, the British portion of Borgu formally became part of the protectorate of Northern Nigeria in 1902, and a resident officer was appointed. Even under the best of circumstances the British presence in the emirate would have been shadowy, since the shortage of officers often meant that one man alone was in charge of the administration over varying periods, with only intermittent assistance. But to add to the problem, the caliber of officers assigned to Borgu province seems to have been frequently comparatively low. In consequence, disturbances occurred that might otherwise have been avoided had administrators of greater perspicacity made more of an effort to understand the people, their customs, and their his-

The author traces in detail the events that led to the rebellion; he makes clear that a combination of mismanagement, not anticipating the results of deposing a ruler, however incompetent he may have seemed in the eyes of the administration, and ignorance of traditional relationships made almost inevitable the violence that followed. Throughout the period the intriguing figure of the ineffective, drunken ruler, Kitoro Gani, appears as a constant theme. Removed by the administration and exiled for a number of years, he was at last brought back to his throne by wiser administrators who realized finally that a traditional chief venerated by his people would be more likely to bring political peace than would any successor selected as a result of British pressure.

Revolt in Bussa is more than a careful and painstaking reconstruction of a vignette of Nigerian history. The author makes of the events a fascinating story, but he also uses them for an examination of the inner workings of indirect rule. By liberal quotation from contemporary administrative reports he brings out the weaknesses of the officers assigned to Bussa during the early years, and he emphasizes the failure of their superiors in the protectorate administration to understand the dangerous course of the actions being reported to them. Crowder's account is all the more vivid to those

Africa 153

who have traveled in the area, but for any student of colonial administrative history it is an invaluable addition to a knowledge of this period.

Professor Obichere's work is the broad canvas of which Crowder has painted a part in detail. Obichere deals with the complex negotiations behind the settlement of British, French, and German territorial claims in the Dahomey-Niger hinterland during the critical period of European expansion between the Congress of Berlin and the Anglo-French Convention of 1808. The problem involved not only international rivalry for trade and territory, but a conflict of personalities in the Foreign Offices of the powers, to which were added the desires of ambitious expansionists of both sides. As Obichere points out, expectations of the wealth of raw materials in the hinterland were greatly exaggerated, often by the leaders of expeditions who sought by this means to reinforce the domestic pressures being brought on the national negotiators to claim as much territory as possible.

The key to development of the area lay in the control of navigation and trade on the lower Niger, and competing commanders sought to supply evidence of positive occupation of territory by means of treaties signed with the indigenous rulers—often with little assurance on either side of the extent of territory covered or the ability of the signers to comply with the terms of the document. But the African rulers were quick to realize the potential value of these treaties as protection against other European invaders, and they learned to play the European diplomatic game in short order.

Much of the material in Obichere's book is already available in other works, but he has gained fresh insights from new documentary sources. His interpretation, stemming as it does from the viewpoint of an African scholar, adds a new dimension to the dry facts of history. It is unfortunate that the volume has the defects which are the inevitable accompaniment of a reworked dissertation—less than vivid writing and an overwhelming burden of academic paraphernalia.

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BRIAN WEINSTEIN. Éboué. New York: Oxford University Press. 1972. Pp. xiii, 350. \$8.95.

This volume on Félix Éboué fills a long felt and very real need of the English-speaking

student of African history for materials in the English language on the African possessions and leaders of France in Africa. The need is twofold: first, there is a need for general studies on French West and Equatorial Africa and on the French philosophy of colonial administration, and second, there is a need for more specific studies on the leaders who have influenced the formation and direction of the French world order. It is in this latter category that this work on Éboué is a welcomed addition. However, it must be admitted that while the need is clear, it is a question as to whether Professor Weinstein's account is effective in filling in some of that void. Certainly his research was most thorough and his interviews with the members of the Eboué family and with close associates and friends were extensive. It is my fear, however, that Éboué may, in a sense, have escaped the writer. Perhaps a concluding chapter, designed to bring together the attributes, thoughts, and programs of this important French African administrator, might have been helpful.

Mr. Weinstein traces his subject, often with laborious detail, throughout his life, from early childhood in Guyane, through his education in France, through his long colonial administrative career in French West and Equatorial Africa and the French Caribbean, through the significant and tumultuous days of his actions in behalf of de Gaulle, through his appointment as governor-general of French Equatorial Africa, through the Brazzaville meeting, to his untimely death in Cairo in 1944. However, it would have been helpful if Weinstein could have given his reader a deeper insight into Éboué, his feelings, his frustrations, and his thoughts. Also it would seem that the work does not fully explore the pivotal role of the emergence of Éboué as a leader in the events that brought de Gaulle to effective power. A more penetrating study of the Brazzaville conference and its significance would have been especially helpful to the student of contemporary African

While this volume has some distinct omissions it is, nevertheless, a most valued addition to the literature of French Africa.

It is to be hoped that this book will encourage others to illuminate further the role of Éboué and other significant figures of French Africa. The book has a good bibliography and index attached.

VAN MITCHELL SMITH University of Texas, Arlington PHYLLIS M. MARTIN. The External Trade of the Loango Coast, 1576-1870: The Effects of Changing Commercial Relations on the Vili Kingdom of Loango. (Oxford Studies in African Affairs.) New York: Oxford University Press. 1972. Pp. ix, 193. \$14.50.

Fundamental questions regarding the course of African history in terms of precolonial trade have been raised in the past decade: the origin and function of domestic slavery and its relationship to the Atlantic slave trade; the pattern of initiative and response in the relations between African states and European traders; and the effect upon indigenous societies of organization of large-scale foreign trade. We are indebted to Professors John Hargreaves and and George Shepperson for another significant contribution in their Oxford Studies in African Affairs. The usefulness of Dr. Martin's book is in focusing these questions on the Loango coast, a relatively small area of western Central Africa north of the Congo River that was marked by political and social cohesion at the time of its first contact with Portuguese traders late in the sixteenth century. Three centuries later, at the point of being overrun by European claims, it was characterized by political fragmentation and social disintegration.

Expanding on her chapter in Pre-Colonial African Trade (Richard Gray and David Birmingham, eds., 1970), Martin has carried the story down to the nineteenth century to good effect. Utilizing the theoretical tools outlined by Gray and Birmingham and building on the base established by Jan Vansina in Kingdoms of the Savanna (1966), Martin begins with the traditional, hierarchical societies of Loango, Kakongo, and Ngoyo that existed when they asserted independence from the Congo political system south of the Congo River early in the sixteenth century. She then focuses on their commercial relations with one another and with the Europeans, who begin to appear on the Loango coast later that century, illuminating the processes of resulting change by an examination in particular of the Vili kingdom of Loango.

From oral tradition and contemporary observations made by Dutch, French, and English trading agents, officers in the later anti-slave-trade patrols, and missionary writings, a convincing argument is made that the initiative in Afro-European relations was frequently on the African side. Successful adaptation to new external trade demands—such as employing already existing skills in long-distance caravan routes—broadened the base of economic power

as the business of central government increased. The devolution of authority, however, initiated by the once autocratic Maloango, went further than was intended and in the end made his formerly all-powerful position irrelevant. From the beginning and until the mid-eighteenth century, during a long Vili ascendancy over trade on the Loango coast, both the Maloango and his trading manager, the Mafouk, were effective in playing European rivals off against one another and assuring that African control and African pricing for slaves were uniformly maintained.

By the nineteenth century the disintegration of a once-strong African government had dispersed political authority among local brokers, who became petty chiefs. Though this situation was itself a successful adaptation to new commercial needs, it resulted in a system of arbitrary exactions that discouraged economic effort above subsistence level among the common people, adding to the image of a stagnant, backward society that Europeans believed they saw in Africa. The dynamics of these changes are carefully examined in this book, and in an appendix Martin supplies a provisional list of Loango rulers and the sources upon which she bases her analysis. The author has made a substantial addition to the ever-deepening exploration of the African past.

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TADDESSE TAMRAT. Church and State in Ethiopia, 1270–1527. (Oxford Studies in African Affairs.) New York: Oxford University Press. 1972. Pp. x, 327. \$17.75.

Beginning with two introductory chapters on Ethiopia before Aksum and the rise and legacy of that state, to which all subsequent dynasties acknowledged their origin, Dr. Tamrat has produced a most perceptive and informative analysis of the rise of the medieval empire of Ethiopia. In striking parallel to feudal Europe in the same centuries, the process of empire building in Ethiopia has rested firmly on the twin pillars of the church and the crown. During the Aksumite period the dynamics of these two institutions produced a tradition of expansion by the book and the sword that continued for centuries until the decline of the empire at the end of the fifteenth century and its near demise between 1531 and 1543 because of the Muslim onslaught under the famous Ahmad Gragn.

Africa

The author's narrative and analysis begin in the thirteenth century with the increasing development of church and state under the new dynasty founded by Yikunno-Amlak in 1270. The author, with a combination of keen insight and skeptical skill, has extracted the reality, represented by a dramatic increase in the sources, mostly hagiographical, behind the paeans of praise habitually attributed to the holy and the great. The story unfolds with the majesty and mysticism of royalty and faith. The state evolves by the kings, who steadily assert themselves over their vassals, thereby increasing the wealth at the disposal of the crown. This trend toward centralization was accompanied by a literary awakening in the church that revived monasticism and inspired a series of reforms. Together soldiers and missionaries extended the territorial limits of the Christian

It comes as no surprise that the expansion of church and state was most active under the two outstanding military leaders of medieval Ethiopia, Awda"-Sijoy (1314-44) and Yishaq (1413-30), who pushed the frontiers of Christendom into the Muslim heartland to the south and east and into the Agaw and Falasha country to the north and west. These bursts of military and Christian expansion were followed by periods of consolidation characterized by intense literary and religious activities under such active kings as Zär'a-Ya'iqob (1434-68). A devout Christian, Zär'a-Ya'iqob sought to use Christianity to stabilize the conquests of his predecessors in the pagan and Muslin areas while seeking to bring unity to the diverse peoples and cultures of the empire through fidelity to the one true religion and loyalty to the crown. He failed, not through lack of effort, but by the succession of crises that plunged the empire into civil strife which after fifty years left the Christian empire helpless before the Muslim on-

This is rich history, thoroughly researched and critically presented. Here the reader can discover the continuity and sophistication of the literary, religious, and political development of medieval Ethiopia whose dynasty and traditions remain so much a part of Ethiopia today. Whether they shall prevail before the winds of change in Africa is doubtful, but whatever the force they will not be scattered like dust before the whirlwind.

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NORMAN H. POLLOCK, JR. Nyasaland and Northern Rhodesia: Corridor to the North. (Duquesne Studies, African Series, number 3.) Pittsburgh: Duquesne University Press. 1971. Pp. 576. \$15.00.

Britain's entry into the nineteenth-century scramble for African territories was prompted in part by Cecil Rhodes's dream of painting the map of Africa British red along a wide swath from the Cape to Cairo. It was the interfering expansionist tendencies of the Boer republics and Germany that caused Britain to enter and administer Nyasaland and Northern Rhodesia. Both territories are now independent as Malawi and Zambia.

Professor Norman H. Pollock, Jr., who teaches history at Denison University in Ohio, describes the British administration of these two territories from 1875 to 1925. He used mainly Philadelphia library resources plus other depositories to supplement material gathered during several months in Africa. His book is a detailed monographic study of colonial problems. As stated in the preface the author did "not give as much attention to Africans and their internal history as one might wish in an exhaustive treatment." What he does cover includes the historical setting, the establishment of the protectorates, the formation of the British South Africa Company, pacification of contending indigenous peoples, administrative problems, the economic base, and the growing political concerns of European settlers. Chapters are devoted to transportation, which the author sees as the heart of the administrative problem in British central Africa, labor, problems, missions and education, and medical care and public health. The author shows the causeand-effect relationship between administrative theory and what was achieved or not achieved. He tries to avoid harsh condemnation of men of that period from this current vantage point. He also tries to show the connections among climate, topography, political development, the economy, transportation, communication, and ideologies.

The book has a concluding summary chapter, an epilogue that brings the history of the areas up to and beyond independence, an appendix that describes geographical conditions under which the principal tribes live, and a second appendix that presents briefly the underlying causes of late nineteenth-century imperalism in Africa. The footnotes are full, the bibliography is useful as a guide for further study, and the index seems ample.

Professor Pollock points out that thus far in-

dependence has been disillusioning for many inhabitants because preindependence problems remain unsolved. He cautions Africans against retreat into an idealized past that cannot be resurrected or never was and against the tendency to find scapegoats for current ills. What he does call for is a frank look at current problems and cooperation among all citizens to solve them.

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ADRIAN PRESTON, edited with an introduction by. The South African Diaries of Sir Garnet Wolseley, 1875. (South African Biographical & Historical Studies, number 11.) Cape Town: A. A. Balkema. 1971. Pp. x, 293. \$11.00.

ADRIAN PRESTON, editor. The South African Journal of Sir Garnet Wolseley, 1879–1880. (South African Biographical & Historical Studies, number 12.) Cape Town: A. A. Balkema. 1973. Pp. viii, 359. \$15.80.

It was the practice of Sir Garnet Wolseley, "the ablest and most controversial soldier and colonial administrator of the late-Victorian Empire," to keep a diary record of his numerous campaigns and missions. His five extant journals (Ashanti, Natal, Cyprus, Zululand-Transvaal, and Sudan) constitute, as Professor Preston aptly asserts, a unique account of the conduct of military campaigns and colonial administration throughout the formative period of British imperialism. They reveal the motives and methods of that imperialism, "the almost paranoiac atmosphere of intrigue that suffused the making and application of British military and colonial policy," and the character, aspirations, beliefs, and methods of their author.

The Natal (1875) and Zululand-Transvaal. (1879-80) journals are undoubtedly crucial to an understanding of Wolseley and his decisive role in South African and imperial affairs. Their value is greatly enhanced by Preston's introductions. Based upon exhaustive research, his analyses of Wolseley's career offer an incisive revisionist interpretation that deserves more extensive review than is possible here. His initial presentation of this interpretation in his introduction to the Sudan journal, In Relief of Gordon (1967), was somewhat dissatisfying for its brevity and lack of documentation, but the introduction to the Natal journal provides a lengthy, thoroughly documented, definitive evaluation of Wolseley's pre-1875 career and limited consideration of aspects of its subsequent development. The introduction to the

Zululand-Transvaal journal briefly continues—though, unfortunately, without documentation—the evaluation of Wolseley's career, primarily as it developed within the context of the protracted Anglo-Russian confrontation of the late 1870s.

"No other soldier" of the age, Preston asserts, "had so sustained an impact upon the evolution of British military policy and professionalism"; for all his limitations "Wolseley, for good or ill, influenced every significant decision or development in British imperial strategic policy for over thirty years." But that influence was often negative, divisive, and fallible, and Preston's penetrating assessments of Wolseley as a military reformer and strategist and colonial administrator effectively destroy the uncritical hagiography constructed by earlier biographers.

Wolseley emerges not only as an able and energetic leader but also as an arrogant, authoritarian, superficially successful opportunist and intriguer driven by excessive ambition and chronic insecurity. In his performance in colonial administration—especially in his assault upon the Natal Constitution, his advocacy in 1875 of the annexation of Zululand, his postwar settlement of Zululand, and his intemperate response to Boer nationalism-he displayed imperious and arbitrary behavior, a proclivity for short-term expedients, and an enthusiasm for simple, forcible solutions to complex imperial problems in South Africa. The unfortunate manner in which those problems developed after 1875 was to a considerable extent the product of Wolseley's influence.

Having acknowledged Preston's pre-eminence as an authority on Wolseley, one must note that in regard to the Natal mission the author might have given more attention to the role of Theophilus Shepstone, to his relationship with Wolseley and its ramifications in native administration and Shepstone's subsequent service, and to the substance and significance of Wolseley's warnings that the Boer Republics would never accept confederation unless strangled by British acquisition of the Tugela-Limpopo seaboard. Preston missed opportunities to bring out these and other points with material from Wolseley's correspondence and Colonial Office records and to identify such figures as J. H. Brand, T. F. Burgers, and Robert G. W. Herbert. A half-dozen statements in the introduction are incorrect, and notes 72.51, 130.72, 135.95, 159.1, 187.5, 242.13, and 249.1 contain inaccurate or misleading information.

There is little to criticize in the Zululand-Transvaal journal. Its annotation is thorough,

though there are errors in notes 25.1, 26.8, 36.57, 40.73, and 48.15. There is confusion in the August 7 entry: in line 9 "Mr. T. Shepstone" should be "Mr. J. Shepstone" (Sir Theophilus's brother John). Finally, Preston's assertion that Wolseley was "technically subordinate" to Sir Bartle Frere's civil authority (p. 20) is incorrect. As high commissioner of the Transvaal, Natal, and adjoining eastern regions, Wolseley superseded Frere in those areas and exercised supreme powers, Frere's authority being confined to the Cape. All parties, including Frere, clearly recognized this intentional reduction of his authority.

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## ASIA AND THE EAST

SILAS H. L. WU. Communication and Imperial Control in China: Evolution of the Palace Memorial System, 1693-1735. (Harvard East Asian Series 51.) Cambridge, Mass.: Harvard University Press. 1970. Pp. viii, 204. \$7.50.

The present study is concerned with the "communication-decision" structure as it evolved in the Chinese government during the period in which the Ching dynasty (1644-1912) reached its high point of power. It is the sort of useful institutional history of premodern China that not only adds to our knowledge of the internal workings of the political system, but also effectively relates it to the social and ideological milieu in which the system functioned.

In nine well-documented chapters Professor Wu's query spans the years 1693-1735. It is significant that the initial date was just one decade after the complete suppression of the last armed resistance against the Manchu conquerors, and now the K'ang-hsi emperor was directing more serious attention to the problems of imperial control beyond the existing channels and methods. He initiated the practice of having selected officials, who were also his close associates, to report to him on local matters through the palace (that is, secret) memorials. By the end of the Yung-cheng emperor's reign in 1735 the practice had been expanded and institutionalized into a regular system that included the new office of the Grand Council (Chün-chi ch'u), an organ in the central government that continued to occupy a vital place in policy deliberations until the end of the dynasty.

The palace memorial system itself had evolved primarily out of the deliberative procedures of early Ch'ing, and under K'ang-hsi it remained largely personal and completely secret, the documents seen only by the memorialist and the emperor himself. The use of the palace memorials was extended in the last decade of the K'ang-hsi reign, as the emperor wished to gain more confidential information during the factional strife at court over the succession to the throne. The Yung-cheng emperor, having been designated heir to the throne at the end of this prolonged struggle, continued and expanded the use of secret memorials. Under him an informal coterie (Wu uses the term "committee") of inner Grand Secretaries was formed who assisted the emperor in dealing with confidential matters; at the same time, high provincial officials were ordered to send secret reports to the throne on a broadened scale. With the "overwhelming amount of raw information" pouring in through the new channels of communication (p. 79) the communication-decision system was not only expanded, but underwent structural changes as well. The final stage of this development was the merging of the functions of the Office of Military Supplies, in the course of the northwestern campaigns in the early 1730s, with the high-level advisory function of the inner Grand Secretaries, thus forming the Grand Council.

Based on a thorough examination of archival and published documents Wu's work provides us with valuable insights into the decisionmaking process in the Ch'ing political system. One cannot help but think that the value of the book would have been enhanced had the author ventured beyond the dynastic concerns and included within his conceptual scheme some reference to the impact of the Chinese scholar-official syndrome on the communicationdecision process: the Manchu emperors, in fact, were not the originators of regular procedures for policy deliberation by high officials; the latter was a fundamental aspect of the technique of government that had enabled a centralized monarchy to operate effectively, for the most part, along with a bureaucracy of educated elite to various extents since the Han times. Although the precise personnel and methods of communication and policy deliberation differed with the passage of time, the tempering of the monarch's absolute power by a countervailing power based on the concept of a meritocracy would seem to have played a large role in the ability of the Chinese system to survive until it was challenged by new problems in the modern era.

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W. E. WILLMOTT, editor. Economic Organization in Chinese Society. (Studies in Chinese Society. Sponsored by the Subcommittee on Research on Chinese Society of the Joint Committee on Contemporary China of the Social Science Research Council and the American Council of Learned Societies, 1971-72.) Stanford: Stanford University Press. 1972. Pp. xi, 461. \$16.50.

In many ways Economic Organization in Chinese Society is one of the best of the seemingly unending series of symposium volumes that are published in the field of Chinese studies. Although it suffers from variations in the quality of the separate essays that mark all such works, it deals with a relatively clearly demarcated period (the eighteenth to twentieth centuries), has a central theme (the social framework of economic modernization), and a distinctive disciplinary orientation (economic anthropology). The thirteen studies follow a continuum of theoretical sophistication. Thomas Metzger's discussion of the organization of the Ch'ing (1644-1911) salt monopoly gives a good deal of interesting detail, but it is marred by an awkward prose style and is characterized by an eclectic use of social science jargon. Susan Jones contributes a fascinating and well-written description of Ningpo banking during the nineteenth century, but she fails to treat one of the most critical roles of banks in early modern societies: the ability to provide an elastic money supply. As in all her work, E-tu Zen Sun provides a smoothly composed and carefully researched picture of the Ch'ing dynasty silk industry. And Craig Dietrich traces a carefully constructed outline of the technology and organization of cotton production during the eighteenth and nineteenth centuries. Although all four of these essays can be classed as traditional descriptive studies, and would therefore be considered pedestrian by proponents of the "new" economic history, the authors are clearly knowledgeable in elementary theory and avoid the kind of gross errors that frequently appear in earlier work on the pre-twentieth-century Chinese economy.

In his contribution Mark Elvin attempts to use a kind of pseudoeconomics to explain the absence of technological innovation in the early modern Chinese textile industries. According to Elvin, "over all growth was held back primarily by a long-established combination of low farm productivity per capita and very high productivity per acre," thereby precluding the development of surpluses in the agrarian sector that could have generated sufficient demand for cloth so as to stimulate invention. The condi-

tions governing this proposition, which he styles as the "high-level equilibrium trap," are so poorly specified that the hypothesis is difficult, if not impossible to test. At a minimum it assumes, as Elvin acknowledges, that "the country was not just a loosely-knit collection of local economies, but an economic unit effectively integrated by trade." This assumption contravenes common sense. If it were true, there must have been a mobility of resources that even the highly developed Western nations have only recently achieved with the help of modern transportation and communications. Indeed, it can be more logically argued that the absence of integration and barriers against interregional exchange were the chief obstacles to technological innovation. Elvin's piece contrasts markedly with that of Ramon Myers, who uses sophisticated theory to analyze the commercialization of agriculture through a comparative examination of the economic behavior of family farms in prewar China and Taiwan. His contribution is one of the best in the book.

The seven remaining essays are written by sociologists and anthropologists. There is a greater degree of theoretical input in these studies of contemporary local social situations than in the historical pieces, with the exception of Myers's article. But this is primarily because of the adaptability of sociology and anthropology to the interviewing techniques used in Taiwan and Hong Kong. Burton Pasternak contributes an absolutely fascinating analysis of the relationships between changes in agricultural technology in Taiwan and alterations in social structures. Lawrence Crissman is less successful in his attempt to test and invalidate central-place theory in an analysis of marketing on the Ch'ang-hua Plain, Tainan. And Stephen Olsen comes to somewhat inconclusive results in his study of the socialization of economic values. He surveyed students from different occupational groups in three Taipei senior middle schools, and the statistical results allow for varying interpretations.

The last four essays contribute to the debate as to whether noneconomic factors such as values make classical economic theory inapplicable to some past and present societies. Donald DeGlopper, Robert Silvin, and Barbara Ward study business activity in contemporary Taiwan and Hong Kong in order to test the idea that the particularistic values of the Chinese were an obstacle to their making economically rational decisions. All come to more or less the same conclusion as Silvin: "There is little concrete evidence to support the argument

that kinship and other prescriptive ties impair economic activity." Finally, John Pelzel examines economic management of a production brigade in Communist China and finds, once the legal limits are specified, a model of economic action quite in accord with what any economist would expect.

This volume, in conclusion, provides a fairly good sample of the kinds of work presently being done by historians, sociologists, and anthropologists on the early modern and modern Chinese economy. In a cheaper paperback edition the book could provide an excellent text for courses on economic history and development.

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JEAN CHESNEAUX. Secret Societies in China in the Nineteenth and Twentieth Centuries. Translated by GILLIAN NETTLE. Ann Arbor: University of Michigan Press. 1971. Pp. xii, 210. \$7.95.

LUCIEN BIANCO et al. Popular Movements and Secret Societies in China, 1840–1950. Edited by JEAN CHESNEAUX. Stanford: Stanford University Press. 1972. Pp. xii, 328. \$11.95.

Chinese secret societies may be traced back for more than two thousand years, but Professor Chesneaux and his colleagues are only concerned here with the role of these societies in the revolutionary social changes in China during the century following the Opium War. Chesneaux uses a familiar theme in characterizing the period as one of "intense, modern-style revolutionary struggles . . . aimed at eliminating an ancien regime that was degenerate, corrupt, and incapable of standing up to foreigners or of leading the country in the path of progress" (Secret Societies, p. 11). He breaks new ground, however, in seeing the history of secret societies in the nineteenth and twentieth centuries "not as a subset of the history of secret societies since antiquity, but as a subset of the general history of China during the nineteenth century" (Popular Movements, p. 3). This approach requires its own operational definition of secret societies as "associations whose policies are characterized by a particular kind of religious, political, and social dissent from the established order"that is, as a discrete subset of the genus "Chinese secret society." The principal merit of this approach is that it adds a new dimension to the historiography of modern China by demanding that the testimony of popular, grassroots participants in the revolutionary process be sought out aggressively, admitted to consideration, and fully weighed. In as many words, Chesneaux denounces (unnamed) Western Sinologists for "their overriding concern with the Confucian establishment, of which they were in a sense unconscious heirs as a result of the mandarin image imposed on China by the Jesuits" (Popular Movements, p. 4). As distinguished from "impeccable 'archive material,' " Chesneaux's sources include official documents of the societies, such as proclamations, manifestoes, catechisms, oaths, and rituals; recollections and evewitness accounts of former society members; surveillance reports by the mandarinate and police in China and in Southeast Asian countries where many society members were active; reports of other observers with firsthand information (including some journalists); and commentaries by Chinese political personalities from Hung Hsiu-ch'üan to Mao Tse-tung. Such material is used with discriminating respect for its limitations, and the resulting history is fresh, suggestive, and challenging.

Secret Societies is Chesneaux's own monograph, but the text is so heavily interlarded with some sixty substantive extracts from major sources that it also becomes the principal source book of its subject. In Popular Movements Chesneaux and fifteen other scholarly collaborators of various nationalities-Lucien Bianco, Lilia Borokh, C. A. Curwen, Lev Deliusin, Guillaume Dunstheimer, Winston Hsieh, Georges Jidkoff, Ella S. Laffey, Charlton M. Lewis, John Lust, Mark Mancall, Boris Novikov, Guy Puyraimond, Roman Slawinski, and Frederic Wakeman, Jr. —offer specialized article-length studies of specific aspects of the secret society phenomenonbiographic, cultural, economic, political, regional, and religious, among others. The supplementing apparatus of the volume includes four valuable glossaries: first, a bilingual name list of some 250 secret societies (pp. 291-97); second, a list of some 265 persons connected with secret societies, including Sun Yat-sen, Chiang Kai-shek, and other familiar names (pp. 298-305); third, a secret society terminology of some 231 terms (pp. 306-14); and last, a list of Chinese and Japanese authors whose works in their respective languages are included in the extensive bibliography (pp. 315-16).

Among Chesneaux's generalized conclusions (Popular Movements, pp. 16-21), the following deserve brief mention: first, secret societies have been "forces of opposition" to established authority; second, their recruitment of coolies and other manual laborers was aided by their own economic activities in salt and other com-

modities, control of public markets, and other profit-making activities, including piracy and brigandage, so that "the history of the secret societies is [also] the history of the formation of the illegal petty bourgeoisie" (as summarized by Feiling Davis, a contributor to the French edition of Popular Movements); third, they were closely linked to the "old China" of regionalism and other traditional forces and understood its language; and fourth, the main support for the secret societies came from poor peasants, the urban proto-proletariat, and rural and urban fringe elements (largely displaced peasants that migrated to burgeoning towns and cities). If the societies had no interest in class struggle as such, Chesneaux suggests that they "embodied the two main lines of class struggle that we find in France in 1789 and most other preindustrial societies: the struggle of the bourgeoisie to free itself of feudal economic restraints, and the struggle of the peasants against their feudal masters." As a modernizing force, the secret societies formed a new type of voluntary association for the uprooted peasantry and freed them from lineage, village, and other ascriptive connections, with important consequences for social loyalties. By offering these generalizations in a highly tentative sense, Chesneaux actually appears to be suggesting a new syllabus for the future historian of modern China.

Each of the two volumes is complete in itself, but the full impact of Chesneaux's style and method is best felt when both volumes are read as a unit.

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JESSIE GREGORY LUTZ. China and the Christian Colleges, 1850–1950. Ithaca: Cornell University Press. 1971. Pp. xiii, 575. \$16.00.

SIDNEY A. FORSYTHE. An American Missionary Community in China, 1895–1905. (Harvard East Asian Monographs 43.) [Cambridge, Mass.:] East Asian Research Center, Harvard University; distrib. by Harvard University Press, Cambridge, Mass. 1971. Pp. viii, 146. \$4.50.

Many college teachers today may still remember the not too distant past when books on missionaries were written by missionaries for missionaries in none too critical fashion. The studies of modern China have since progressed. One piece of evidence is the two books under review; each brings forth a wealth of data and is written with a keen sense of analysis.

In Professor Lutz's broad but detailed account of the Christian colleges from their inception to their disappearance under the Peking regime (perhaps the best part is her description of the inner workings of the missionary groups and their home boards) we learn that, in spite of the great weight of education in the total activities of missionaries in China, teaching was never more than a subordinate means to the end of converting China to Christianity. We also learn that the mission schools had a very low priority in fund allocation and that one reason for not teaching the Chinese classics was their incompatibility with Christian doctrine. None of this was known to the Chinese public even though the topic of missionary education commanded a great deal of attention. Professor Lutz also presents well the specific contributions of the mission colleges to agriculture, medicine, library science, and education for women. Although these are well known the author's documentation is very useful.

One might, however, query the implications of some of the author's remarks. After demonstrating that the missionaries turned to education only after they had failed in winning converts and were in need of a different entering wedge Lutz states flatly that "the colleges originated out of the need of Westerners, not as a result of Chinese need." But does this not come very close to the anti-Christian charge that the schools were tools of Western cultural imperialism?

Again, Lutz insists that the Western presence was a catalyst of Chinese nationalism and that the mission schools "aided the Chinese in defining themselves and in defining the West." The crucial point is what constituted Chinese nationalism. Lutz seems to have a complex view of this matter, for she speaks of a variety of things including national unity, anticapitalism, and the desire to accord power to the state. Yet one might adopt a much simpler view and say that Chinese nationalism came essentially as a reaction against Western encroachment and that unity and state power were merely considered appropriate means for national self-strengthening. If this view is correct then the catalytic role of the missionaries was correspondingly negative. To put it simply, the missionaries might have merely fueled Chinese antiforeignism by their own bad behavior. This possibility is not examined critically by Lutz but emerges clearly from Professor Forsythe's monograph, which is a sociological analysis of the attitudes toward China on the part of 103 Protestant missionaries.

By a careful examination of their own writings Forsythe succeeds in drawing a clear profile of the attitudes of these men and women. The picture that emerges is unflattering by any standard. The missionaries were both bigoted and shallow. They had little knowledge of or interest in China beyond the immediate concern of their work. They had a pervasive stereo-type of "the Chinese mind" that was overwhelmingly negative and derogatory. Probably because of their failure as evangelists they could see no possibility of any desirable change emanating from within China; therefore they applauded such events as Japan's victory over China but lamented the relatively peaceful policy pursued by the United States. In a crowning act of cupidity the missionaries used force to collect reparations from rural communities in 1900 and 1901, and according to the eyewitness account of an American newspaperman some preachers even participated in the looting of palaces in Peking.

Writing within the framework of a monograph Forsythe does not deal with the broad significance of his findings. Yet these certainly affect our understanding of the roots of contemporary Sino-Western relations. Unless we assume absolute insensitivity on the Chinese part, it is impossible to deny that the missionaries created ill will among the people they supposedly served. Moreover, it is safe to say that this antagonism far outweighed any good will that came from the contributions of individual missionaries to specific fields, since the public attitude toward a group is always based on its stereotyped behavior rather than on the deeds of a few individuals. If this interpretation holds, then many new avenues of research open up. How much harm did the missionaries do? Did they really change their attitudes after 1905? Above all, what are the factors that govern the success or failure of a missionary movement? In spite of the enormous American investment in missionary efforts, there seems to have been no scientific study of these problems. Professor Forsythe is to be congratulated for his pioneer endeavor.

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E. A. BELOV. Uchanshoe vosstanie v Kitae (1911 g.) [The Wu-ch'ang Revolt in China (1911)]. (Akademiia Nauk SSSR, Instytut Vostokovedeniia.) Moscow: Izdatel'stvo "Nauka." 1971. Pp. 249.

The task of this book, Mr. Belov carefully explains, is to show the reasons for the success of the Wu-ch'ang uprising on October 10, 1911 (the decisive event of the Hsin-hai Revolution that soon led to the collapse of the Ch'ing dynasty in China), to clarify its moving forces and its aims, to analyze the policy of the Wu-ch'ang revolutionary military government, and to suggest some reasons for the defeat of the bourgeois-revolutionary movement by Yuan-Shih-k'ai and his feudal-compradore class. Mr. Belov, after opening with a brief treatment of the historiography of his subject, organizes the book in a chronological manner: from the beginnings of armed struggle against the Manchus in 1906, the preparations for Wu-ch'ang, and the railroad protectionist movements, to the Wu-ch'ang uprising itself, the support it received from other provinces outside of Hupei, and the end of the revolutionary movement in Hupei in 1913. There is much detail, and much interpretation of it, throughout. The book is an impressive historical monograph.

Many of the conclusions that Mr. Belov reaches will not trouble American historians, despite his evident wish to force upon bourgeois historiography the view that Wu-ch'ang was quite accidental. Such a construction of the events of 1911 no longer holds up in the West, if indeed it ever did. Belov's history emphasizes the mass participation in the uprising, particularly by the lower echelons of the army. He makes a convincing case for the programmatic weakness of the bourgeois-revolutionary leaders. He confirms that the T'ungmeng-hui was completely out of the picture in Hupei, and goes on to deplore its moderate and elitist leanings. If the leaders were more in touch with the masses, if their forces had not been partially disbanded after 1911, the story might well have been different. But might-have-beens are absurd, especially to a Marxist historian, and Belov does not dwell on them. The very fact that the revolutionary leaders were bourgeois can suffice to explain their naiveté, their ties with landlords and feudal-bureaucratic elements, their fear of the people, and their talk of "one big Chinese family." The revolutionary movement was, in Belov's word, an "ill-assorted" camp that as a whole could overcome its acute class contradictions only insofar as the primary job of dumping the Manchus was concerned. After that it understandably disintegrated.

Mr. Belov's treatment of the declared neutrality of the foreign powers in 1911 is rightly suspicious of their pious invocation of inter-

national legal principles. But he does not consider the possibility that the powers, like the Chinese people, were now ready to see the Manchu dynasty come to a well-deserved end. And Yuan Shih-k'ai, entering in the last pages of this book (as is his custom), is figured by Belov as someone, not who was chosen for his ability to handle foreign powers, but who chose himself on the basis of his ability to handle the capitulationist bourgeois leaders of the revolution. Here there is room for a great deal more study and speculation.

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ROGER V. DES FORGES. Hsi-liang and the Chinese National Revolution. (Yale Historical Publications, Miscellany, 99.) New Haven: Yale University Press. 1973. Pp. xviii, 274. \$12.50.

K. S. LIEW. Struggle for Democracy: Sung Chiaojen and the 1911 Chinese Revolution. Berkeley and Los Angeles: University of California Press. 1971. Pp. ix, 260. \$8.75.

A comparison of these two books validates the old aphorism that the more we know the less sure we are. Each adds materially to our knowledge of the last decade or so of the Ch'ing dynasty and the revolution of 1911–12. But both authors re-examine fundamental questions—who the revolutionaries were, what they contributed to the decivise events of 1911–12, what place the revolution has in the course of Chinese history—and they disagree with each other and challenge existing interpretations

Roger V. Des Forges has taken the refreshingly novel approach of studying the 1900-12 period through the career of a high Ch'ing official. Hsi-liang (1853-1917) held a variety of key posts and earned the respect of his superiors, who included Chang Chih-tung and the empress dowager, and his enemies, including foreign imperialists and anti-Manchu revolutionaries. Loyal, hard-working, and honest, Hsi-liang grappled courageously with the major issues of the day, struggling to reconcile his love of the past with the needs of the present, his respect for tradition and hatred of foreigners with China's need to change, his loyalty to the state with his devotion to the people. Des Forges demonstrates beyond dispute that right to the very end of the Ch'ing there were officials who possessed vision, integrity, and a large capacity for growth and innovation. Less and less can we thing of late Ch'ing officialdom as corrupt and reactionary.

But can we go so far as to consider men like Hsi-liang radical? Des Forges finds that Hsi-liang's life was characterized by "three strategies of resistance, expansion, and radicalism." These developed in three phases from 1900 to 1909, after which "he adopted elements of all three strategies and blended them in a most complex and significant way" during the last eight years of his life. Hsi-liang's many-sided career and personality do not fit very comfortably into this rather contrived pattern, and none of the three "strategies" seem sufficient to define entire phases of his life, but of the three overworked and ill-fitting terms, the least suitable is "radicalism." Des Forges applies this term to so many varied activities and policies that it becomes seriously diluted. It comes to include not only reforms that in the context of the 1900s have generally been considered moderate, but also as passive an act as retirement from public life. There is merit in Des Forges's very interesting attempt to find in Chinese tradition some basis for a modern Chinese radicalism, but Hsi-liang was no radical. Perhaps the clearest evidence is that when the time came to choose in 1911, he acted without the slightest doubt or hesitation: he chose wholeheartedly to defend the dynasty and save the throne.

The different perspectives from which K. S. Liew and Des Forges view the revolution can be indicated by their references to Chang Chien. Chang provides an interesting point of contact between Hsi-liang and Sung Chiao-jen, the revolutionary who is the subject of Liew's book. A leading supporter of the Manchu regime's reform movement, especially its effort to introduce by slow stages a limited form of constitutional and representative government, Chang switched sides two and one-half months after the revolution broke out. Thereafter he acted effectively both to force the abdication of the Manchus and to moderate the revolution. Des Forges finds that Hsi-liang's support for Chang Chien in 1910 is one of the clearest examples of Hsi-liang's growing radicalism; Liew places Chang even in 1912 among those "conservative and non-revolutionary elements" who undermined the short-lived Nanking provisional government. Liew appreciates that the single most tangible achievement of the revolution, the overthrow of the Manchu dynasty, could not have occurred without the participation of men like Chang Chien; but the author also realizes that their participation in the revolution as late as the end of 1911 did not make them radicals. For by the end of 1911

the republic was already being born. In January 1912, when even the reactionary Manchu prince and chief grand councilor I-k'uang had come to favor abdication, Hsi-liang was still proposing drastic last-ditch measures to save the throne.

Des Forges has written a very valuable portrait of an able but essentially traditional late Ch'ing official, a man of no ordinary qualities but well short of greatness, who swam in confusing crosscurrents. One misses in Des Forges's book sharper attention to those currents. Fuller discussion of Hsi-liang's relationship to K'ang Yu-wei and Liang Ch'i-ch'ao would have been especially welcome. Des Forges stresses the man and shortchanges the times. Swept this way and that, Hsi-liang spent a frustrated life (I am less "struck by his achievements" than Des Forges) that ended in 1917 with a melancholy "deathbed memorial" to the child-emperor in his palace exile. Sung Chiao-jen, struck down by an assassin's bullets at the age of thirty, sent from his deathbed a telegram, equally melancholy, to China's new ruler. Readers of both will sense similarities as well as differences between the two men. but I was impressed by the contrast between the one urging his child-sovereign to work hard at "the kingly way" five years after he had been deposed, and the other enjoining Yüan Shih-k'ai to "promote democracy so that Parliament can produce an everlasting constitution." It comes as no surprise that Des Forges sees the revolution as "merely a collapse that brought an end to a decade of ferment and change" that was in turn the product of "a long radical tradition" and "a long rebel heritage." Liew, however, sees 1911 as "a momentous event" that not only was the climax of a century-long process, but was the beginning of the last sixty-plus years of revolution. Liew is the more persuasive, largely because he achieves a better balance between the man and the times. Sung's growth and his effort to plan for postrevolutionary construction, even as he tried, despite his youth and inexperience, to lead a revolution at the same time, receive highly sympathetic treatment. But Liew's focus upon Sung does not preclude some excellent wide-angle shots of the larger movement. Liew occasionally allows himself to become defensive about partisan criticism of Sung, but, on the whole, he provides a very good analysis of the complex and contradictory forces that made up a highly diverse and even fragmented revolutionary movement. Sung's preoccupation with the introduction into China of Western-style institutions is discussed with particular force. Indeed, one major issue that these books raise in different but equally effective ways concerns the uneasy coexistence between Chinese antiforeignism and China's imitation of foreigners. The foreign powers as both threatening imperialist overlords and admired models loom over the lives of men such as Hsi-liang and Sung Chiaojen and flit still too silently through the history of the Chinese revolution.

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KENNETH E. SHEWMAKER. Americans and Chinese Communists, 1927–1945: A Persuading Encounter. Ithaca: Cornell University Press. 1971. Pp. x, 387. \$10.00.

Shewmaker has provided us with a well-researched and carefully written study of the first round of encounters between Americans and the Chinese Communists. Appearing on the eve of renewed contacts the book was a timely reminder of the twisted paths we had followed in our attitudes toward Mao and his followers. Almost all the men and women of whom Shewmaker writes found the Chinese Communists attractive and were sanguine about the prospects for the Chinese people under Mao's leadership. But by the 1950s reporters who had found Yenan more to their liking than Chungking were accused of conspiring to deceive the American people, of trying to disguise the Red hordes as "agrarian reformers." In the 1960s scholars praised Tang Tsou for raising the status of these American observers from that of traitors to that of fools who did not understand the insidious nature of international communism. Shewmaker, in addition to recounting the visits and analyzing the reports of his subjects, considers both conspiracy and Tsou theses, offering useful correctives.

Shewmaker focuses on the Westerners, mostly journalists, who visited Yenan in the years from 1937 to 1945. Edgar Snow is the most familiar of these travelers and he analyzes Snow's work with precision and balance. He demonstrates the absurdity of charges that Snow sought to deceive the American people about the nature of the Chinese Communist movement without ignoring the fact that Snow's writings in the 1940s were colored by his commitment to Mao's cause. He treats Agnes Smedley—an archetype "infantile leftist"

—with warmth and Freda Utley, who provided grist for McCarthyite mills, with understanding. T. A. Bisson and Anna Louise Strong come off less well. Others discussed include James Bertram, Evans Carlson, Haldore Hanson, Philip Jaffe, Gunther Stein, and Theodore White.

Referring to recent studies of Communistcontrolled areas in the Yenan period, Shewmaker concludes that his subjects reported accurately and that their understanding of Chinese communism was closer to reality than that of their later critics. On the central question of why Americans were drawn to Mao and his vision he offers several valuable insights. Their disgust with Chiang and conditions in Kuomintang China derived not only from the corruption, repression, and indifference to human suffering they observed, but also from a reaction to Chiang's style of rule—which they perceived as neo-Confucian feudalist. In Yenan they found not only honest and humane government, but also modern men with Western values. Mao, Chou En-lai, and Chu Teh were open, informal, direct-and appealed to Westerners repelled by the reserve and ritual they found in Chungking. Forced to choose between the Communists and the Kuomintang, American observers found the Communists closer to their image of themselves: the men most likely to create a modern China in which the hideous suffering of the lao pai hsing would end. In 1974 who would gainsay them?

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HUNGDAH CHIU. The People's Republic of China and the Law of Treaties. (Harvard Studies in East Asian Law, 5.) Cambridge, Mass.: Harvard University Press. 1972. Pp. xv, 178. \$10.00.

Law in general reflects political reality and objectives. Although much the same is true with other states, the political determination of legal principles is particularly pronounced in the case of China.

The author argues that the People's Republic of China accepts the general principles and practices of international law adhered to by the West and the Soviet Union in particular, at least up to the early 1960s. It can be maintained, however, that due to its numerous and politically inspired qualifications, reservations, and outright disagreements, there can be no definite delineation of a uniform Chinese attitude toward international law. This im-

portant point has not been sufficiently high-lighted.

The author belabors the legalistic approach, as if China and the West share a sufficient body of common perceptions dealing with treaty law and the existing differences do not constitute a major rift. Although the book presents a thorough compendium of Chinese legal thought and practices through the 1960s, given the knowledge of Chinese history and its basic policies and problems, simple political inference would be sufficient to discern what the actual views of Peking would be in most cases. For example, and all perfectly understandable, are, first, China's legal argument to invalidate the Sino-Indian boundary; second, Peking's shifting position on the legal status of Soviet treaties with Outer Mongolia; third, the treatment of the most-favored-nation clause; and fourth, China's view of the Taipei-Washington Mutual Defense Treaty as "void" being contrary to the United Nations Charter. In this context Chiu's statement that "[Chinese] practice seems to indicate that revolutionary groups have the capacity to conclude treaties" (p. 13) is simply inane.

China's views on the most-favored-nation clause and the "unequal" treaties are primary indicators of its attitude toward the question of treaty law. The examples of "unequal" multilateral treaties (Nuclear Test Ban and Non-Proliferation) that the Chinese consider "null and void" are enlightening as to China's political considerations. On the other hand, the strange silence of the People's Republic on the Big Five's "veto" in the Security Council seems a clear case of political flexibility in its legal positions (p. 68). Yet the most dramatic fluctuation of Peking's perception is seen in the radically different reactions to the Hungarian uprising (p. 88) and the Czech crisis (p. 66).

Indeed, depending on the nature and intensity of its disputes with the Soviet Union and the détente in general with the West, the legal position of the People's Republic would further change: a radical departure from the Soviet position on treaty law, a gradual convergence with the West's, and increasing support for international organizations.

Given the dramatic changes in China's foreign policy and the relatively moderate domestic atmosphere, it seems reasonable to state that the publication of this book has been too hasty. Its serious failure to consider China's present attitude in the postdétente period severely limits its worth. Besides, the author

tried too consciously to investigate China's "legal" views, while they either do not exist in established form or are in a state of rapid evolution contingent on shifting political perspective. Peking's perception of self-interest, national security, historical views, ideological commitment, differing interpretations of Marxism-Leninism in the context of violent dispute with Moscow, and propaganda motives vis-à-vis the third world merit greater attention.

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BERTOLD SPULER. The Mongols in History. Translated by GEOFFREY WHEELER. New York: Praeger Publishers. 1971. Pp. ix, 166. \$8.50.

J. J. SAUNDERS. The History of the Mongol Conquests. New York: Barnes and Noble. 1971. Pp. xix, 275. \$10.00.

BERTOLD SPULER. History of the Mongols: Based on Eastern and Western Accounts of the Thirteenth and Fourteenth Centuries. Translated from the German by HELGA and STUART DRUMMOND. (The Islamic World. Published under the auspices of the Near Eastern Center, University of California, Los Angeles.) Berkeley and Los Angeles: University of California Press. 1972. Pp. x, 221. \$8.00.

The three books under review were intended by their authors to be read by nonspecialists. It will not be inappropriate, therefore, to consider them from that perspective. Bertold Spuler's The Mongols in History is based on a series of lectures presented in France in 1959-60. The translator, Geoffrey Wheeler, has provided a few textual notes and an epilogue. Spuler views the Mongols from the standpoint of the history of Iran, Central Asia, and Russia. The Mongols enter his account with the formation of Chinghiz's steppe empire and the establishment of the khanates. They soon vanish from the scene, however, with the incorporation of their western settlements into the Islamic and linguistically Turkish society of the western steppes. The subsequent history of these largely Turkish heirs of the Ilkhanate and Kipchak and Chagadai khanates is then pursued into the twentieth century. Iran is largely excluded after the beginning of the Safawid dynasty, as is Mongolia from the time of the transfer of the grand khanate from Karakoram to Peking. The East Asian conquests of the Mongols are not discussed. The author has thus chosen to focus his attention on those fields of history that his own research has done much to develop. He writes with

economy and assurance, and he has provided a lucid introduction to his subject. His main interest is in political history: the internal politics of the khanates, Eurasian international politics, and the changing position of the Turkish peoples within the Russian Empire and the Soviet state. Economic factors (chiefly trade), the rivalry of religious establishments, and state religious policies are skillfully integrated into the political history.

J. J. Saunders's The History of the Mongol Conquests is differently defined in time and space. The author introduces his work with an essay on Eurasian nomadism and an outline of Turkish history from the sixth to the thirteenth century. His treatment of the Mongol Empire ends with the rise of Tamerlane in Central Asia and the founding of the Ming dynasty in China. In contrast with Spuler, Saunders has undertaken to emulate the continental geographic scope of Rene Grousset's l'Empire des Stepbes (1948) by discussing the Empire in toto. Like Spuler, Saunders appears to be more at home in Islamic West and Central Asia than in Eastern Asia, but he has attempted here to transcend that limitation. He has organized his account around the conquest of the "civilized" poles of the Empire-Persia and China-and the manner in which Mongol rule was terminated in these societies. Russia and Central Asia are offered as counterexamples in which the "semicivilized" population of the one and the oasis and steppe peoples of the other offered less resistance to the formation of ethnically complex states and therefore remained longer under Mongol or Turkish domination.

Saunders's History offers certain advantages to the student looking for an introduction to the Mongol Empire: copious annotation links the text to a very large body of secondary literature, and the bibliography is large enough to be useful for Western-language materials. On the other hand, certain shortcomings of Saunders's book appear in direct proportion to its more ambitious scope. His mode of analysis sometimes seems less a help than a hinderance. His assertion that the Khitan and Jurchen "aped" the "superior culture of China" (p. 43) does not contribute to an understanding of the process of acculturation. Moreover, he does not encourage an intelligent interest in Buddhism when he writes that it "has oscillated between the loftiest theosophy and the most debasing superstition" and that it is "beyond the comprehension of most Westerners" (p. 179). Is Mahayana's trikaya, the doctrine

of the three bodies of Buddha, any less intelligible than the Christian Trinity? Also, in the author's consideration of the effects of the Mongol conquests on the history of religions, he introduces a kind of revolvingdoor principle: Buddhism was "suppressed" in China in 845 and "went underground" (p. 179), "the Buddhists" were "expelled" (p. 181), and "under the barbarian dynasties which succeeded the T'ang, Buddhism reentered China" (p. 179). Buddhism, favored by Kubilai. similarly "seemed destined . . . perhaps even to return to its Indian birthplace, whence it had been expelled" (p. 180). Apart from the fact that Buddhism was not "expelled" from China nor, strictly speaking, from India either, such mechanical reification does not lead to analysis, but inhibits it.

The third title under review, Spuler's History of the Mongols, is a collection of thirteenth- and fourteenth-century Mongolian, Persian, Arabic, Syrian, Latin, and French source materials, many of which have not appeared before in English translation. The materials are so divided as to provide a neatly articulated documentary account of the Mongol Empire and an easy reference to specific topics. This volume usefully complements the translations presented in Christopher Dawson's Mission to Asia (1955).

Welcome as these contributions are, their limitations call attention to the still unmet need for an integral history of the Mongol Empire. Their methodology is better suited to the study of single civilizations than to the simultaneous examination of several civilizations within one great polity. The fact that the Empire was divided into several khanates and that these soon asserted their independence does not require that they be treated separately. Saunders's distinction between the "civilized" and the other regions of the Empire suggests one step toward a comparative analysis: how does a pastoral-nomadic society interact with a bureaucratic empire? The hypotheses of Owen Lattimore and Wolfram Eberhard, among others, still await application to the history of the Mongol Empire as a whole. Finally, it should be noted that both Saunders and Spuler speculate briefly on the possible relationship between the formation of the Mongol Empire and the subsequent relative imbalance between Western Europe and the rest of the world with respect to wealth and power. Here is a fertile field in which the

formulation and testing of hypotheses have scarcely begun.

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ROBERT H. G. LEE. The Manchurian Frontier in Ch'ing History. (Harvard East Asian Series 43.) Cambridge, Mass.: Harvard University Press. 1970. Pp. 229. \$8.00.

"Cradle of conflict" is the description Owen Lattimore applied in the 1930s to Manchuria. Now Robert H. G. Lee has picked up Lattimore's trail and given us an account of the contest for control of the broad, rich, and sparsely settled Manchurian frontier zone, roughly defined by the provinces of Kirin and Heilungkiang, immediately south of the Amur River. The contest Lee describes is not the familiar international one. He is instead preoccupied with the more obscure yet, for the ultimate fate of the region, the more decisive domestic struggle of Chinese settlers against both a rugged frontier environment and a long-hostile Manchu court.

Lee deals in detail with the Ch'ing dynasty's policy of isolating frontier Manchuria from Chinese influence, the gradual abandonment of this policy under pressure of immigration and Russian and Japanese penetration, and the resort to a policy of colonization, a time-honored tool of Chinese frontier defense. Lee offers no clear judgment on whether this reversal of policy should be interpreted as another of those creative responses credited to late Ch'ing statesmen or merely a belated reaction to a situation already out of hand.

The real protagonist of Lee's story is the anonymous, land-hungry Chinese peasant, who contributed to a tenfold increase in the population of Kirin and Heilungkiang between the mid-nineteenth century and the turn of the twentieth. This process of Sinification is the pivot on which the history of modern Manchuria turns and is fittingly the subject of the best chapter in this book.

Lee has culled from Chinese gazetteers, official documents, and travel accounts the details for an often engrossing perspective on frontier life. He excells in evoking the atmosphere of raw frontier towns and wild mining camps peopled by colorful characters—exiled scholars, convicts, bandits, merchants, and poachers. It is unfortunate that this monograph, revised for publication from a Columbia University Ph.D. dissertation, retains some of

the flaws common to that genre. At points the author obscures major themes in a forest of facts, or he resorts to the unimaginative practice of summarizing documents mechanically and in tedious detail. He has also failed to consider his topic in a larger context by comparing it to China's other frontier areas. We can nevertheless be grateful for this noteworthy contribution to our knowledge of Ch'ing China.

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w. g. Beasley. The Meiji Restoration. Stanford: Stanford University Press. 1972. Pp. vi, 513. \$17.50.

DAVID WURFEL, editor. Meiji Japan's Centennial: Aspects of Political Thought and Action. (Studies on Asia, Second Series, volume 1—1968.) Lawrence: University Press of Kansas. 1971. Pp. xii, 105. \$5.00.

The Meiji Restoration of 1868, like all complex historical events, has defied easy interpretation. Was it the central occurrence in Japan's modern revolution? Did Japan in fact experience a revolution in the French or Russian sense? If not, why not? And if so, what were its causes? What motivated its leaders? And what were its results? Historians in and out of Japan have debated these questions, sometimes with great heat, during the last hundred years. For the Japanese the answers to these questions have had deep political and emotional implications. For to them it matters a great deal whether the Restoration can be looked upon with pride, as the start of a national effort at modernization dedicated to the common good, or with shame, as the beginning of a course of statist development based on repression at home and expansion abroad, leading ultimately to war in the Pacific. When in recent years the Japanese faced the prospect of celebrating the hundredth anniversary of the Meiji Restoration, the nation divided sharply over whether the last hundred years had witnessed a "successful" modernization or whether "the revolution" could only be said to have been achieved in 1946 with the adoption of a new "democratized" constitution.

It is inevitable that the Meiji Restoration should be interpreted in a wide variety of ways. For an event so complex can only be "explained" by the use of formula (a preconception of what is meant by revolution) or by simplification (the selection of one aspect as explanation of the whole). The great prewar

debate among Marxist scholars over whether the Restoration represented an absolutist counterrevolution (the kōza argument) or the first stage of a middle-class revolution (the rōnō argument) was in large part a controversy over definition of what constituted revolution. The differences that have divided Western writers were mainly over whether the primary motive force was of external or internal origin, whether the modern history of Japan was a story of Westernization or, as E. H. Norman so dramatically described it, the working out of class interests vested in the lower levels of the samurai aristocracy, propertied merchants, and rural landlords.

W. G. Beasley's thoughtful and eminently readable book has the primary virtue of not neglecting the complexities and controversies over interpretation of the Restoration at the same time that it presents a detailed and coherent narrative of how Japan moved from the Tokugawa condition of 1830 (weak, divided, unstable, feudal) to the Meiji condition of the 1870s, by which time a new society was being built upon institutions drastically different from those of the old regime. Yet, as Beasley points out again and again, no revolutionary leadership exercised a conscious and continuous influence on events between these dates. The old order was not pulled down in the name of social equality or specific revolutionary goals, but rather in the name of national defense against pressure from Western powers. There were radical leaders, to be sure, as Marius Jansen and Harry Harootunian have so eloquently demonstrated in their respective studies. But the radical ideologues and men of violent action, while they may have helped to arouse the political consciousness of their compatriots, had burned themselves out long before 1868. The Restoration itself was the work of conservative-minded leaders, "realists, pragmatists, bureaucrat-politicians . . . men convinced that national defense required national unity." These men were of no single class, no single station within the hierarchy of wealth, in fact not even necessarily anti-Tokugawa.

But victory over the Tokugawa made these men responsible for government and above all "for implementation on a national scale the policies that would bring Japan 'wealth and strength.'" In pursuit of this policy the Meiji leaders were pushed increasingly to revolutionary measures. Since feudalism was "an obstacle to national strength, it had to go. . . . Since land tax was an essential resource . . .

landlords got confirmation of their landed rights." Samurai privilege was abolished not in the name of social equality but in order to make possible a national conscript army. And so on.

Did all of this add up to revolution? Beasley answers cautiously. Certainly the Restoration lacked the "avowed social purpose that gives the 'great' revolutions of history a certain common character," and modern Japanese society was one "in which 'feudal' and 'capitalist' elements worked together in a symbiosis dedicated to acquiring national strength." If revolution it was, it could best be called a nationalist revolution, "thereby giving recognition to the nature of emotions that above all brought it about."

This conclusion is not new, to be sure, but Beasley leads us to it with a sure hand and through a great wealth of detail along the way. Based on a comprehensive reading of works in Japanese and English, sensitive to all the nuances of interpretation, Beasley's is a synthetic study of great depth and perception. Yet synthesis is not its only quality. The middle chapters that deal with the politics of the Restoration, and the later ones that trace the post-Restoration policy formation, are based on letters, memoranda, diaries, and observations of and about the main protagonists in the Restoration drama, providing a new intimacy in our understanding of the thoughts and motives of these men. A further dimension is given through the use of British records from Foreign Office archives.

Future historians are not apt to quarrel with the balance and thrust of this work. There is, to be sure, a current trend to make more of the civil war that flared in the aftermath of the Restoration. And the whole subject of popular unrest at the end of the Tokugawa period will need re-examination. One of these days we will know more about what merchants and farmers thought in the 1860s and 70s, and this can then be added to the picture. But until then, Beasley's work can stand as a mature summary of where we stand today in our understanding of the Meiji Restoration.

The second book under review takes us back to the question of the meaning of the Restoration as image and as model for the contemporary Japanese and for the rest of Asia. The volume consists of a series of papers prepared for the Midwest Conference on Asian Affairs in 1968. Taken together, they serve as a reminder that the Restoration must always hold an ambiguous place in history. Marius

Jansen's reflective piece explores the special qualities of the Restoration leaders-turned Meiji oligarchy and why they did not institutionalize their influence beyond their own life spans. Sidney Brown describes the continued use of political assassination in Meiji Japan and how the murderers of Okubo became folk heroes. Barbara Teters provides a case study of the difficulties that beset one judge in his effort to uphold the rule of law and judicial independence as part of the newly created constitutional government. Two final papers by Frank Wong and Thadeus Flood deal with the ineffectuality of the Meiji pattern of modernization as a model for either China or Thailand. The Japanese of the Meiji period were themselves too nationalistic and too concerned with their own problems to be effectual exporters. The Meiji Restoration was to that extent confined to its time and its place. Unlike the "great revolutions," it provided no idealized universal message for either the people of Japan or their continental neighbors.

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NOBUYA BAMBA. Japanese Diplomacy in a Dilemma: New Light on Japan's China Policy, 1924–1929. Vancouver: University of British Columbia Press. 1972. Pp. 440. \$10.50.

While Professor Bamba does shed some new light on Japan's changing China policy during those overshadowed interwar years from 1924 to 1929 he fails to elucidate much beyond the public statements and behavior of the two foreign ministers of the period. Actually he is groping for a sociopsychological rationalization for his conception of prewar Japan as a case of "national-cultural schizophrenia." This psychosis appears to him to be preordained for nations like Japan that modernize from Western impetus. The very success of modernization, he explains, simultaneously produces antipathetic sentiments based on traditional values and a renewed sense of nationalism. The emotions are then articulated by reactionary leaders bent on asserting to the nation and the world a unique nationalcultural identity. They implement illogical policies that, as in Japan's case, culminate in a "dilemma of diplomacy." The nation could neither advance nor maintain its interests, especially in China, short of the ultimate irrationality-war. Blindly these traditionalists led Japan to national suicide.

The bulk of this book is concentrated on the

China policy conflict between the traditionalists personified by a frenetic, reckless premier and concurrently foreign minister, Baron General Tanaka Giichi, and the modernists portrayed by a rational, internationally disposed foreign minister, Baron Shidehara Kijūro. In the introduction the author repudiates hitherto used partial approaches for understanding national policy and claims an awesome, if inclusive, methodology for this study. In the succeeding three parts are discussed the historical background to the rise of the 1920s traditionalists, biographical and ideological, backgrounds of Tanaka and Shidehara and the antagonistic goals and methods of implementing China policy pursued by both men. A concluding section emphasizes the confusion of a "double diplomacy" that brought suspicion and war to Japan, and it boldly sketches the Dr. Jekyll nature of Shidehara diplomacy and the Mr. Hyde character of Tanaka diplomacy. Indeed Baron Tanaka is depicted as "the vital link between Meiji traditionalists and Showa ultranationalists" (p. 367).

Bearing the marks of the Ph.D. dissertation from which much of this work was derived its strengths carry concomitant weaknesses. The promise of an exciting presentation based on such intriguing social science concepts as Professor DeVos's adaptation of selective permeability is not fulfilled. This is so partially from poor handling of ideas and partially from the author's penchant to inundate the reader with details, many of which are irrelevant. Yet there is the excitement in this study of embarking on new theoretical paths for explaining such complex phenomena as interactions between actors' subjective perceptions, as well as the conflict between cultural legacy and the will to progress along nonindigenous lines. This book requires further mulling, considerable pruning, more careful attention to accuracy of facts, and a greater sense of detachment by the author. In the vernacular on our campuses Professor Bamba "hasn't quite gotten it all together yet."

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HAN WOO-KEUN. The History of Korea. Translated by LEE KYUNG-SHIK. Edited by GRAFTON K. MINTZ. Honolulu: East-West Center Press. 1970. Pp. xii, 551. \$15.00.

WILLIAM E. HENTHORN. A History of Korea. New York: Free Press. 1971. Pp. xiv, 256. \$7.95. There has long been an acute need for bal-

anced, comprehensive, and scholarly books in English on Korean history that may be useful to historians, political scientists, and other students of cross-cultural studies. The two significant books by Professors Han and Henthorn go a long way in filling this need, certainly from my standpoint as a political scientist. There have of course been since World War II a fair number of Korean history books published in Korea and Japan, among other countries, but they were written in Asiatic languages and have remained untranslated. Relatively brief discussions of Korea in a number of excellent books, including East Asia (1970) by Professors John K. Fairbank, Edwin O. Reischauer, and Albert M. Craig, could not be comprehensive enough for many Asianists.

Han's work, originally written in Korean for Korean readers and expertly translated and edited for Western readers, proposes to present "a new history of Korea," as contrasted with unbalanced accounts, and promises to emphasize "the social structures of the past" as distinguished from a "dynastic-centered description of history" (p. v). Henthorn introduces his work modestly by stating that it is intended for "university undergraduates studying the history and culture of east Asia" and that "the structural format" for his work, "such as the use of topical categories and the inclusion of terms in the original and considerable institutional information, has been dictated largely by considerations of the audience for whom this work is intended" (p. vii).

In terms of the periods covered, Han begins with "The Primitive and Tribal Societies," but he subsequently pays considerable attention to the "modern" and "contemporary" periods (pp. 203-509). Han's study covers the period up to 1960 when the "old President Syngman Rhee] had no choice but to step down" (p. 509) in the wake of the student uprising. Henthorn begins his study with "The Archaeological Record" and brings it to the end of traditional Korea, or roughly the end of the Yi dynasty in 1910. Henthorn's four appendixes are most fascinating, particularly the postscript in which he spells out the "theoretical considerations" underlying his assumptions and interpretations of key features of Korean history (pp. 237-40). Henthorn's summary bibliography (pp. 241-50) also provides a valuable guide to further research on Korean history. Han's bibliography (pp. 516-26) is also valuable, and it contains a fair number of works in Korean as well as Japanese. His index (pp. 527-48) is thorough, and Chinese characters are also given for most of the indexed items. Henthorn's book is also indexed (pp. 251-56).

Han, a Korean who studied in Korea and Japan before he was a research associate in the Harvard-Yencheng Institute in 1961, has a comfortable and thorough familiarity with Korean history. His book abounds in keen insights and incisive analyses of some characteristic features of the Korean past. His generalizations, however, sometimes tend to be quite sweeping: "Since Buddhism is not an exclusivist or dogmatic religion like Christianity or Islam, these different [Buddhist] sects managed to coexist [in Korea] fairly amicably most of the time" (pp. 69-70). Han's book also contains some ambiguous and unsubstantiated statements. For instance, while discussing the tribal leagues, he writes that "the ruling class probably had somewhat more elaborate houses. We know that they dressed in silk, wore leather shoes, and necklaces and earrings for precious stones. Commoners wore clothes of hemp cloth and straw shoes. The characteristic Korean topknot had appeared, and our sources also mention the custom of tatooing" (pp. 36-37; the italics are mine).

Henthorn's work is far more tightly and precisely written and pronouncedly topical in structure. His translations of classical Korean poems, excerpts from ballads, Confucian and Buddhist texts, and the like are skillfully used in many parts of his work (for example, pp. 74, 77, 100, 105, 107, 124-25, 126, 135, 148-49, 180, and 190). Not only do they make the book more readable, but they also highlight his professed emphasis on Korean culture, literature, and ideas. Henthorn furthermore lets Koreans themselves succinctly capture the mood, interest, and "intellectual outlook" of many periods. Henthorn writes, for instance, that "the eminent Koryo Confucian scholar Ch'oe Sungo (927-89) expressed it [the intellectual outlook] in this way in 982: 'Carrying out the teachings of Buddha/Is the basis for cultivation of the self./Carrying out the teachings of Confucius/Is the source for regulating the state./The cultivation of the self is necessary for the future world;/Ruling the state for affairs of the present" (p. 105).

The assumption upon which Henthorn's book is based "emanates from the view that a concern of the study of history is the analysis of societies, cultures, and human relations through time" (p. 237). On this assumption Henthorn expertly interweaves his discussion of the early Koryo governmental structure (pp.

89-91) with, for instance, brief sections on social structure (pp. 95-96), life in the capital (pp. 101-02), and commercial activities of the elite (pp. 102-03). It might be added, however, that these short topical sections occasionally make the work appear fragmented. Henthorn is ever mindful of "a persistent theme which runs through Korean history, revealed in practically every facet of the national life . . . namely, the native and the foreign, sometimes in harmony, sometimes in conflict" (p. 239). His clear awareness of the larger environment for Korean history is manifested effectively through his chronological overviews that are given at regular intervals. Henthorn's original "interpretation of the meaning of Korea's relations with other states" has been influenced by "the notion of functionalism." He also found it useful to view the Chinese influence on Korea as "a process of acculturation, with the critical variables being language, religious and ethical systems, and, to some extent, technology" (p. 239). I would have liked to read his elaborations on these attempts at original interpretations that are so briefly intimated in his postscript. Henthorn writes that events after the reign of King Kojong (1864-1907) "belong to another era and are properly the subject of another book." It is earnestly hoped that he will write another book on Korean history very soon.

Both books are highly valuable additions to a slowly growing number of scholarly studies on Korea. Both should be on "must" lists for any serious student interested in Korea, and both will definitely benefit all students of Asian affairs.

JOHN K. C. OH

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M. A. LAIRD, editor. Bishop Heber in Northern India: Selections from Heber's Journal. (The European Understanding of India.) New York: Cambridge University Press. 1971. Pp. vii, 324. \$19.50.

"I never saw such prospects before, and had formed no adequate idea of such. My attention was completely strained, and my eyes filled with tears, every thing around was so wild and magnificent that man appeared as nothing, and I felt myself as if climbing the steps of the altar of God's great temple." Such views "most sublime and beautiful" moved Heber as his pony took him toward the snowy peaks of "the glorious icy mountains" of Himalaya. But even more noteworthy was Heber's "strong sense of

the fundamental one-ness of mankind." Except for places and names, one can hardly tell when he was writing about England or India. Thus, of one local community he could write: "They are a modest, gentle, respectful people, honest in their dealings and as remarkable for their love of truth as the Puharees of Rajmahal and Boglipoor"; or, of a Brahman Pandit: "He is evidently a man of considerable talent and extremely desirous to improve whatever opportunities of knowledge fall his way."

Such an observer was Reginald Heber. His impressions of India and its people, along with reflections into his own attractive character that these reveal, are what make Heber's Journal so important. An incorrigible romantic who relished traveling in the wilds, Heber could remark, "my life has been that of a Tartar chief, rather than an English clergyman." Yet his tours were undertaken not just for sake of curiosity, but out of a sense of professional duty. He looked at the Empire, not as an imperialist, but as a person always responsible for what was in the interests of the people. Thus he did not hesitate to say what he thought of British failings, especially about arrogance and lack of consideration for the feelings of peoples in India. Heber was an Anglican of moderate, Evangelical sympathies, whose elevation doubtless came in part from his managing the unusual feat, as Dr. Laird aptly puts it, "of pleasing men of most diverse opinions." Heber was a supporter of missionary activity at a time when Protestant work, so long limited to German efforts in Tranquebar and Serempore, had hardly begun its large expansion. But he supported such activity only so long as it was carried on with utmost courtesy and sensitivity to the feelings of local inhabitants. Indeed, what so marked Heber's Journal at its first appearance and led so quickly to its republication was its deep interest and sympathetic understanding of human concerns in India. At the same time, by its acute probings, it gave a glimpse of a country that was changing, both in social and economic conditions and in responses to ideas from the West.

Thus the reprinting of this splendid classic is a notable service both to scholars and to general readers. First published in 1828, a year after Heber's sudden death in Tanjore, the Journal is a compilation of letters and notes sent to his wife. Selections in this edition, which reduce the text to half its original length, contain choice accounts of the bishop's days in Calcutta (1823–24) and of his travels across Northern India to Bombay. In the excellent

introduction Dr. Laird succinctly surveys early British missionary and Anglican Church history in India and gives a brief biographical sketch of Heber and his significance. Explanatory notes, a short glossary, and a select bibliography are also helpfully provided. Doubtless, Americans will only know of Heber, if at all, by his hymns, some of the most popular in the English language. Yet for scholars Heber on India might be compared to Tocqueville on America.

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DHARMAVIRA. Lala Har Dayal and Revolutionary Movements of His Times. Foreword by R. C. MAJUMDAR. New Delhi: Indian Book Company; distrib. by South Asia Books, Columbia, Mo. 1970. Pp. vii, 9–363. \$7.50.

In Lala Har Dayal and Revolutionary Movements of His Times Dharmavira explores the life of a Punjabi intellectual whose techniques of building a cadre of political workers through. the experience of living in an ashram, of using Indian dress to express respect for his own culture, and of disassociation with the government as a means of protest foreshadowed Mahatma Gandhi's use of similar political instruments two decades later. Har Dayal first worked to mold students in north India into political missionaries around 1908, next moved into Indian revolutionary terrorist circles in Europe, then helped to organize Sikh laborers, Hindu intellectuals, and German money in the United States into the abortive Ghadr conspiracy during World War I, and finally, at thirty-five, moved permanently into exile in Sweden and later England and turned to teaching, lecturing, and writing philosophical works.

While the subject is both little studied and fascinatingly complex Dharmavira's work manifests the strengths and weaknesses usually found in the voluminous genre of life and times biographies of Indian nationalists: The major defect is the lack of a definite focus on Har Dayal or of a specific thesis about his relationship to the political development of modern India. The inevitable result is repetition and occasional confusion as the narrative wanders from chronological biography to discussions of political events in India to thematic summaries of Har Dayal's ideas on religion and politics and women. As a consequence the author fails to investigate fully key relationships, decisions, and periods in Har Dayal's life such as his activities in Germany and Turkey during World War I. Erratic documentation might also possibly be a result of the undetermined orientation of the work. At times Dharmavira buttresses generally accepted points with long quotations from memoirs or interviews with contemporaries and references to files of the Home-Political Department of the Government of India. In other places, however, he denies the assertions of a close associate like Gobind Behari Lal without giving any indication of the evidence on which he bases his conclusions.

The assets of Dharmavira's study include a warm evocation of an unusually colorful individual who exerted a strong personal influence over students and those he reached by the spoken word. Dharmavira is at his best in two areas. His description of the personalities, activities, programs, and linkages of Indian revolutionary terrorists in India, Europe, and North America is crowded with illuminating detail not readily available in one source. His survey of Har Dayal's years in exile cover a period of growing isolation from Indian politics. Though Dharmavira does not always explain satisfactorily the contradictions between the thought and actions of his subject his book reveals a man most worthy of further study.

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JUDITH M. BROWN. Gandhi's Rise to Power: Indian Politics 1915–1922. (Cambridge South Asian Studies, number 11.) New York: Cambridge University Press. 1972. Pp. xvi, 384. \$19.50.

FRANCIS C. HUTCHINS. India's Revolution: Gandhi and the Quit India Movement. Cambridge, Mass.: Harvard University Press. 1973. Pp. ix, 326. \$14.00.

There is much to praise and criticize in these two books by young scholars. Praiseworthy is their use of unpublished private papers and government records in the New Delhi and London archives. Brown also makes good use of recent Ph.D. theses done at Australian National University, Cambridge, and Oxford, while Hutchins was able to interview participants in the 1942 Quit India Movement. Both books, however, show only a partial understanding of Gandhi's motives, goals, and methods, and they seem to me flawed by interpretive schema that distort the realities the authors wish to describe and explain.

Brown, writing as a political historian interested in the interplay between provincial

and national leaders, displays an exemplary attention to detail and an admirable skill in weaving together evidence from British and Indian sources. After an unfortunately brief chapter on Gandhi's twenty-year career in South Africa, closer attention to which would have helped her grasp better what Gandhi was trying to do in 1919-22, she shows how odd he seemed to the Western-educated political elite during his first years back in India (1915-19). Her next chapters demonstrate Gandhi's use of satyagraha (in the form of noncooperation with British-dominated institutions and nonviolent disobedience of selected laws and official orders) to rectify local injustices in rural areas of Bihar and Gujarat and to protest the repressive Rowlatt Bills in 1919. Lieutenants, variously termed "mobilizers," "subcontractors," and "henchmen," were recruited and trained during these struggles in provinces previously little involved in nationalist politics, and these men became the nucleus for one of Gandhi's three "bases of power" when he came forward in 1920 with a program for nationwide satyagraha.

Brown portrays convincingly the crucial role of the second large group supporting Gandhi (because he supported them)—Muslims anxious to protect the holy places of Islam and the status of the defeated Ottoman caliph. The author's detailed narrative ends with Gandhi's victories at the Congress sessions of September and December 1920, the defeated, old-style Congress leaders becoming his temporary allies and the third "base" of his "power."

So far, so good. But in her two concluding chapters Brown seems to me a prisoner of a mechanistic view of politics as the quest for power rather than for justice, national selfrespect, or the general welfare. Succumbing to a naive empiricism, she believes her portrayal of the 1920-22 noncooperation movement, based on abundant reports and memoranda of worried British officials and the writings of Indian leaders critical of Gandhi's methods, to be more "realistic" than the accounts of unnamed writers of "nationalist mythology and hagiography." Small wonder that her final paragraph describes Gandhi as "a political failure." What moved Gandhi to come forward when he did, why so many of his countrymen rallied to his support, and what the sacrifices they made in 1920-22 meant to them then and thereafter-such questions are not answerable within Brown's limited framework.

Hutchins's India's Revolution rests on a narrower base of evidence but ranges much more widely in space and time than Brown's Gandhi's Rise to Power. Hutchins is at his best in his earlier chapters, which are philosophical essays reminiscent of Aristotle's in that they are gracefully written and studded with both brilliant insights and improbable generalizations. Sensitive to states of mind, Hutchins traces a pattern of change from manipulating one's foreign rulers, to selectively assimilating their values, to rebelling against those values. The fourth stage he posits, revolution, is too broadly defined to be usable as an analytic tool. There is a difference between ousting a foreign power and overthrowing an indigenous ruling group.

In the latter half of his book Hutchins tells us, somewhat disconnectedly, about the 1942-43 outbreaks of lawlessness triggered by the arrests of Gandhi and the Congress leadership. The author argues that what both Gandhi and the Viceroy called "rebellion" (pp. 195, 282) was a "spontaneous revolution" (chapter 9), a "final showdown with the British" in which the Congress "gambled-and won" (p. 273). Just what changes these events produced in the minds and hearts of the people of India and their rulers is not clear from Hutchins's scanty evidence on this question, but his conclusion that they "brought British rule to an end . . . the revolution had triumphed" (p. 283), seems improbable in the light of Jawaharlal Nehru's verdict that "it was a foolish and inopportune challenge."

Both books thus present much interesting material, but they are marred by simplistic or faulty generalizations. Brown seems to underestimate the psychological and political effects of Gandhi's leadership; Hutchins overestimates them. Neither book has an adequate index with respect to the themes and concepts deployed (for example, imperialism, nationalism, power, rebellion, revolution). What is most valuable in both books is the focus on a limited period in Gandhi's career. Brown's has the added merit of discovering detailed evidence of his changing relations with lesser-known provincial and local leaders.

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NALINI RANJAN CHAKRAVARTI. The Indian Minority in Burma: The Rise and Decline of an Immigrant Community. With a foreword by HUGH TINKER. New York: Oxford University Press, for the Institute of Race Relations, London. 1971. Pp. xxiv, 214. \$10.50.

The story of the Indian minority in Burma as

it fared from 1852 on is fascinating and melancholy. A small alien minority makes truly outstanding contributions to the economic development of a remote area of a far-flung empire and in time that minority community is inevitably rejected and expelled. The Indian immigrant, as transient or permanent resident, came to Burma at British behest to take up the modern tasks that were not being done by the local population, tasks that lacked prestige in the traditional society. He came as a miserable coolie, soldier, trader, administrator. He stayed on and prospered, and so did the country. Almost never was he in competition with the indigenous peoples of Burma: the Burmans, Shans, Karens, Kachins, and Chin, to name the most important of the ethnic communities.

The immigrant the world over follows this pattern. Forced from his home by economic hardship or some sense of oppression and attracted by economic opportunity, he starts at the bottom of the social ladder. He works his way up in the community through diligence and hard work, and he is ultimately absorbed into his adopted society. The failure to accept this assimilation has been the flaw in the colonial migrations of the nineteenth and twentieth centuries, and the flaw in the life-style of the Indian minority in Burma. The Indians came to the country at foreign request in numbers dictated by foreign authorities, political and economic. They chose, even those who stayed on to become important in the colonial community, to be Indians first and Burmese last. That is to say, the Indian minority with which Dr. Chakravarti's study is concerned never became Burmese. Others from India have done so over the centuries-the Burmese Muslims, Arakanese, and the like-and are now Burmese.

Chakravarti's study is excellent, drawing together a wealth of information for the student of Burmese and Indian affairs. Painstaking in its detail, gentle though sad in criticism of the schism that persists between alien and indigenous, it stresses the point that the Indian community throughout the period from 1852, the annexation of Lower Burma, to 1941 was only a small portion of the total population. The Indians at their height made up only 6.9 per cent of the total. Moreover, while portions of the Indian community prospered—as did the Burmese at the same time—major parts of this population remained desperately poor. The Indian coolie did the labor that Burmans would not undertake. "He suffered silently from long hours of hard work, scanty wages, rotten food and wretched shelter." Rich and poor, the Indians were a highly visible alien people.

Chakravarti quite properly notes that from the start of British rule there was a significant Burman protest against the inflow of Indians. The young Burmese nationalists were speaking out on this issue. There was, however, very little intercommunal strife until the 1930s. As always, the problem came to a head at a time of economic privation when the continued unrestricted immigration of Indians was adding to the poverty and unemployment of those Indians already living in Burma. It is impressive, nonetheless, that all through the British era Indian immigrants rarely displaced Burmans from positions they held. In this area Chakravarti concludes that "if it was required to choose out of many evils the one which did greatest harm to Indian interests and contributed most to the unhappiness of Indians in Burma, the choice should unreservedly fall on the erroneous immigration policy, assiduously and obstinately followed by the Government of India, for decades, in the teeth of Burman opposition." Of course this was true. The unrestricted migration was sure to rouse resentment, particularly when the immigrant remained identifiable and aloof. A moneylenderbanker is a moneylender-banker, and there were plenty of them among the Burmans. A Chetyar moneylender-banker, however, is an alien.

Perhaps the problem of the Indian in Burma is best presented in Chakravarti's summary of a 1930 Legislative Council debate. An Indian member of the Council, who insisted that the Burman race and the Burman nation were not identical, protested when Burmese nationalists raised the spectre of the "Indian Menace." A Burmese nationalist replied that "we will be glad if you come to this side of the House and put on a goung boung and paso [Burmese dress] and call yourself Maung Ni. We will receive you with open arms."

The Indian minority in Burma would not then and still has not taken this advice. And in the period of intensely xenophobic nationalism that follows the ouster of an imperial ruler—even though the British left peaceably and voluntarily—this minority has been forced out of any meaningful role in Burma. Chakravarti's study is thus neatly and precisely confined. It is unlikely that the phenomenon of a steadfastly alien immigrant community will occur again, but in this work the reader has an excellent study of such a community's un-

questioned contributions, and its inevitable demise.

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DAVID JOEL STEINBERG et al. In Search of Southeast Asia: A Modern History. Edited by DAVID JOEL STEINBERG. New York: Praeger Publishers. 1971. Pp. xii, 522. \$12.95.

There is irony in the fact that In Search of Southeast Asia should have become available to students and teachers of Southeast Asian history at a time when this area of historical studies faces so many problems, particularly those of funding. This excellent textbook was published in 1971, but its conception dates from the middle sixties when the study of Southeast Asia seemed set on an uninterrupted and ever-upward path. By the time of publication the fallacy of a belief in progress was again revealed. Southeast Asian studies programs have been cut back, courses limited, and numbers of students curtailed. All of this has happened when, for the first time, a historical text became available that genuinely reflected the scholarly advances that had been made in the study of Southeast Asian history.

Yet if this situation is a matter for regret, no such feeling may be associated with the book itself. As the product of collaborative effort, the text is notable, not least, for the way in which six specialists have been able to produce a coherent and unified whole. Even from a stylistic point of view this unity is seldom drawn into question. Only an occasional flatness in the prose suggests some of the quite remarkable problems that were overcome in the course of the book's production.

Style, however, is of much less importance than the way this book provides an authoritative review of the major issues in Southeast Asian history from the eighteenth century to the mid-twentieth-century world. Few will agree with all the interpretations, and individual historians concerned with particular countries may find particular points of details where there are grounds for disagreement. Such areas for debate are to be expected in all but the most bland, and therefore unsatisfactory, of texts. In the case of this book the extent to which the joint authors have striven to take due account of all major questions of interpretation is the point requiring both stress and admiration.

The book is divided into five parts: "The Eighteenth-Century World," "New Challenge's

to Old Authority," "Frameworks for Nations," "Social Changes and the Emergence of Nationalism," and "The Preoccupations of Independence." This arrangement, with its emphasis on conceptual analysis, does seem to assume that the reader already has some basic knowledge of the Southeast Asian region and its history. In this respect, and having used the book as a text for both undergraduate and graduate students, I am inclined to judge that its greatest value is for the already informed, if not necessarily advanced, student. In making such a judgment there should be no suggestion that this book is unsuitable for undergraduates. But In Search of Southeast Asia is, and this should be recognized, a sophisticated text. Offered to an uninformed audience it does require a certain commitment on the part of students, particularly a commitment to pursue the suggested additional readings that are amply and carefully selected.

Indeed the bibliographic service provided by the extended bibliography is a further important contribution made by this volume. One minor word of qualification might perhaps be entered here. The presence in the notes and bibliography of a substantial number of references to non-Western materials is an admirable reflection of the authors' linguistic capabilities. A student, on the other hand, might ask if some of these references could be deleted in favor of more readily available, if not necessarily more valuable, Western-language references.

This, however, is a minor matter, as is the further observation that one might expect the authors, when they come to consider changes for a second edition, to rethink, at least in part, some of the approaches followed in part 5 of the volume.

Given the book's value, even to end with minor criticisms is inappropriate. The publication of In Search of Southeast Asia may not mean that the search for well-written texts has ended. It certainly does mean that a new and very high standard of quality has been set, and by authors well-attuned to the vital issues of Southeast Asian historical scholarship.

MILTON OSBORNE
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JEAN CHESNEAUX et al., editors. Tradition et révolution au Vietnam. (Sociologie et tiersmonde.) Paris: Éditions Anthropos. 1971. Pp. 499.

In the quarter century following World War II

there appeared in a number of journals in France and occasionally elsewhere essays on Vietnam written by members of the Franco-Vietnamese community of Paris. These writers, quite varied in their approaches, took it upon themselves to educate the Western world on the reality of Vietnam. Jean Chesneaux and his students have drawn upon this scattered production to compile a volume that reflects the concerns of these specialists. Interpretive essays, scholarly pieces, journalism, and exposés, all exist among the eighteen presentations, forming a collection that provides a vivid pastiche of Vietnam in the modern age.

In showing how Vietnam has moved from the traditional situation to the present (as seen especially in the DRV) these authors have properly stressed the spirit of Vietnamese civilization in its resistance to the encroachments of the outside world. The result is successful in the sense of action that is given. Ideologically, politically, militarily, and economically, the dynamism of those seeking a free and independent Vietnam emerges as the main theme of its modern history, working toward an ultimate success. Ideology and tactics succeed only as they fit into the fundamental cultural pattern and creativity of the Vietnamese. Foreign manipulations fail because they create artificial situations and stimulate the inherent resistance of the Vietnamese.

Still the degree of success in this work must be qualified by the relative lack of penetration into the Vietnamese scene itself, the stage on which the action takes place. The descriptions of the indigenous patterns of belief are too general, and a much more comprehensive analysis of Vietnamese society is needed before we can gauge the full meaning of the variety of events presented here. The one piece that begins to give us this sense of depth is that on the great Vietnamese epic, The Tale of Kieu, where Chesneaux and Boudarel describe its political and social dimensions in the DRV of the 1960s.

By contrast, the strength of this piece reflects the weaknesses of the collection. Only three essays are based directly on research in Vietnamese language material. Vien has portrayed well the modern Vietnamese experience (including his own), the interviews by Lacouture, Devillers, and Dranber came directly from Vietnamese figures (Ho, Giap, etc.), Condominas and Dumont made their descriptions from first hand observation, but only Son and Boudarel have worked extensively in Vietnamese writings. The remaining essays, many of them quite good, still draw upon Western documentation.

Overall, however, we can be glad that Chesneaux has made the effort to compile these pieces. The authors are men with a greater experience in Vietnam and a greater sensitivity for the Vietnamese than the American writers of the same period. By both its strengths and weaknesses this work shows how we must proceed in our own studies, and Chesneaux's initial statement can only be commended: Vietnam must be seen in Vietnamese terms. With this in mind, this collection can certainly be recommended as a good beginning for the study of Vietnam in the twentieth century.

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ANTONIO DE MORGA. Sucesos de las Islas Filipinas. Translated and edited by J. s. cummins. (Works issued by the Hakluyt Society, Second Series, number 140.) New York: Cambridge University Press, for the Society. 1971. Pp. xi, 347. \$16.00.

Antonio de Morga's Sucesos, originally published in 1609, was the first formal history of the Philippines and their conquest to be written by a layman. A high official of the government at Manila from 1595 to1603, Morga witnessed and even participated in many of the episodes he described, and he had access to survivors from the colony's earliest days. His account is a primary source of unusual value for the study of early Hispano-Philippine history. Previous editions—most notably those by H. E. J. Stanley (1868), José Rizal (1860), Blair and Robertson (1904), and W. E. Retana (1909)—have markedly influenced modern thought about the subject both by making available Morga's information and also by using it as a foundation for the editors' moral and national interpretations.

The Sucesos is, in effect, two books: an episodic, disjointed narrative history of wars, intrigues, diplomacy, and evangelization and an essay (chapter 8) on the culture of the Filipinos, the resources of their islands, and the economics and institutions of Spanish rule. The first of these is of limited interest by now, although it is occasionally dramatic in the telling and suggests the role of clericalism, geographic remoteness, and strategic vulnerability in shaping the Spanish regime. The second is the heart of the work for modern readers. Morga was a historical anthropologist of some subtlety. His coverage is impressive—Stanley discreetly left

untranslated a paragraph on sexual practice—and his perspective humane. Morga took Indian culture seriously and recorded not only the skills, ethics, and social patterns that existed, but also those that had been lost or compromised under the impact of Spaniards and Chinese.

Given the paucity of sources for this period and subject, it was time for a new edition of Morga that would be more accurate, more objective, and more readily available to students and new libraries. Cummins has met the need. His translation is lucid and pleasingly unobtrusive, and it is accompanied by a glossary, maps, erudite annotation, and a useful bibliography of modern scholarship. A careful introductory essay provides biographical perspective on Morga and a historiographical discussion of the book in its earlier editions.

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PHILIP LOH FOOK SENG. The Malay States, 1877–1895: Political Change and Social Policy. (East Asian Historical Monographs.) New York: Oxford University Press. 1969. Pp. xvi, 288. \$9.75.

A large number of books and articles on modern Malay history has appeared in the last decade and a half. Such studies are limited by the nature of the source material available, and, therefore, to British activity both in the Straits Settlements as well as in British Malaya. One major focus of the scholarship of the early sixties was British intervention in the Malay States in 1874, which ended the policy of nonintervention of the previous half century. Professors C. Northcote Parkinson, John S. Galbraith, C. D. Cowan, David MacIntyre, Kim Khoo Kay, and myself examined the question from varying angles. Meanwhile, the University of Malaya at Singapore and Kuala Lumpur produced a number of bright young scholars whose interest, characteristically enough, was in the Malay states proper. The volume under review, a very sophisticated and extended version of a master's thesis, deals with principles underlying British policies, not imperial but local, in their application to southern Malay states before the latter were brought into a federation in 1895. The author's focus is not the Colonial Office in London, where policies could be assumed to have their origins, but British administrators in Malaya and their "attitudes and motives which had a part in determining both the political formation and the social framework" of the Federated Malay States—Perak, Selangor, Pahang, and Negri Sembilan.

Seng's study begins with 1877 in the aftermath of the Perak War, which was until World War II the only challenge to British authority in Malaya. The period before the arrival of Sir Frederick Weld in 1880 is treated as "transition years of political adjustment" during which time "the British commitment was to restore the form if not the substance of a Malay Sultanate" in each of the Malay states brought under the residential system. Seng has assumed that serious political thinking on crucial questions-such as how the Malay states should be governed, what the nature of the relationship between the ruler and the ruled should be, and other related issues—was undertaken for the first time by Weld. Much of the work is consequently devoted to Weld's term as governor.

However important Weld's governorship was, and it was important, residents like Hugh Low and his able assistant William Maxwell had laid down principles of administration in crucial areas like land ownership before Weld's arrival. In fact, among the best sections of this book is the one dealing with land policies. In Perak Low's numerous policy ends included the maximum exploitation of tin mines for increasing state revenues and also for permitting the introduction of British capital in mines already owned by the Chinese. This was achieved through a 1879 regulation that "no land should be allowed to lie idle if anyone is willing to work it, and the owners of metalliferous land must submit to its being worked" (p. 112), as long as the government royalty was paid. A proliferation of fresh European concessions also came about as lands for "agricultural industries" were granted to European concessionaires, often friends of the resident (p. 118). Such a policy of favoring British capital became much more evident during the governorship of Weld, an archimperialist, during the period of the so-called "economic imperialism."

In bringing out Weld's thoughts on administration Seng has perhaps paid greater attention to the clash of personalities—governor and residents and Colonial Office—than is warranted. It would instead have been more instructive in terms of the author's self-chosen task of studying the interaction between political change and social policy to afford some insights into the dynamics of local politics and the changes brought by the introduction of modern economic devices and technology. Hardly any consideration is given to such issues because they are "but faintly perceived in the corpus of English documentation utilized here," thus underlining the serious limitations on

scholarship in modern Malay history alluded to earlier.

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H. C. BROOKFIELD. Colonialism, Development and Independence: The Case of the Melanesian Islands in the South Pacific. New York: Cambridge University Press. 1972. Pp. xvi, 226. \$16.50.

A distinguished geographer with much experience writing about Pacific island areas, especially New Guinea, discusses here the economic history of Melanesia by means of a conceptual frame. He centers most of his attention on the period since 1945 and skillfully demonstrates the widely differing economic development of various parts of the area.

This is a difficult book for a historian to evaluate. It lacks notes, it abounds in social science jargon, and it attempts to build a model applicable to studying the development of heterogeneous peoples scattered over a wide area of the Pacific and whose dissimilarities presumably outnumber their similarities. On the other hand the author makes sound economic judgments, demonstrating a thorough if not intimate knowledge of the area, and presents valuable economic evidence to support his thesis.

Professor Brookfield begins by declaring that he writes "not so much as a geographer, but rather as a social scientist who is also an observer of contemporary trends" (p. x). This may be valuable because of his background and experience, but when he turns to his model, that is another story. He uses the terms "colonialism" and "independence" in a much broader than political sense to explain "opposing drives which persist throughout the whole period of interaction between local forces and invading forces from without" (p. xi). Not only are there inherent problems in attempting to define these terms, which the author concedes, but the definitions do not begin to fit the range of possibilities. For example, the roles of culture and of individual behavior are mentioned, but they are not considered important enough to contribute substantially to the operation of the model.

In addition, some of the areas surveyed, such as the Solomon Islands and West New Guinea, do not fit the system during long periods of their recent history. Further, in discussing the problem of pluralism in Melanesia—and what

a problem it is!-the author admits that to fit it into his model becomes "a gross simplification" (p. 146). One better alternative to such an approach is individual histories of topics or areas that represent cohesive studies that are valuable by themselves but that may be used together. For example, Dorothy Shineberg's study of the sandalwood trade between 1830 and 1865 and R. A. Derrick's history of the Fiji Islands to the 1940s are both more pertinent than this work because they combine sound methodological technique with competent evaluation of a limited subject. Many of the social scientist's techniques may be valuable to the historian, but in this case the generalizations and the continual oversimplifying make this book too uneven and too unreliable to induce the reader to spend the time necessary to mine its buried nuggets of economic data.

W. PATRICK STRAUSS Oakland University

## UNITED STATES

JOHN E. POMFRET. Colonial New Jersey: A History. (A History of the American Colonies.) New York: Charles Scribner's Sons. 1973. Pp. xix, 327. \$10.00.

This first volume of a projected series of thirteen dealing with the original colonies is not completely free from the narrow provincialism often associated with state histories. Eight of the twelve chapter headings in this study of colonial and Revolutionary New Jersey indicate an emphasis on political history, and actual perusal of the contents reveal an even greater commitment to narrative political and religious history with little actual attempt at the interpretation promised on the dust jacket.

Dr. Pomfret's text substantiates his contention that New Jersey's bipolarity and lack of a sense of identity contribute to the complexities of the narration, but this is no excuse for inadequacies such as the sketchy and dated material on the Indians and on the pre-1664 colonies or the social and economic backdrop limited to furtive statistics and almost painful platitudes. The politically derived, detailed coverage of colonial currency and exchange problems illustrates an imprecision in the handling of terms such as fiat money, legal tender, land banks and loan offices, and a lack of knowledge of recent scholarship in areas of paper money and the Currency Act of 1764. The statements that New Jersey's £347,000 paper money emission was colonial America's largest and that New Jersey paper passed at a premium (actually at par) are easily checked inaccuracies as is the omission of mention of the mother country's 1774 approval of a New Jersey land bank.

In a work with such narrative detail, the omission of many New Jersey personalities such as Peter Hasenclever, Caspar Wistar, and Mollie Pitcher is startling. The ideological significance of New Jersey as the battleground of Old Lights and New Lights and the related dynamics of college founding are largely glossed over or ignored in favor of interminable tenure listings of ministers.

The bibliography reveals an unfortunate emphasis on traditional and regional works and few of those recent interpretative works dealing with class, caste, institution, power, and ideology that are remolding our understanding of colonial America. It should be possible, even when seeking a general audience, to enliven such a work with current conceptualizations without getting involved with the obfuscations of some contemporary methodologists. The restatement of the two revolutions theme of Becker and Jamison and the judgment that New Jersey's revolution was conservative and consensual seem to be an afterthought and are less than convincing given the limited evidence.

This volume better meets the needs of neophyte students and the general public than does any other existing single volume, but a definitive interpretative history of colonial New Jersey is still wanting.

ALBRIGHT G. ZIMMERMAN Rider College

BRUCE E. STEINER. Samuel Seabury, 1729-1796: A Study in the High Church Tradition. [Athens:] Ohio University Press. 1971. Pp. xiii, 508. \$13.50.

This is a strange book. Dr. Steiner has stirred together a lumpy brew, mixing extremely important and needed narrative history of the early years of the Protestant Episcopal Church in America, biographical data of Bishop Samuel Seabury, and trivia strained fine indeed. We have long needed a thorough study of the life of Seabury, and this work, in part, fills the void. The best portions of the book explore Seabury's failings as a parish priest (which will be an eye-opener for many Episcopalians), his prominent role in the pamphlet war over King's College, the controversy over the authorship of the A. W. Farmer essays,

and Seabury's appointment as bishop by the Episcopal Church of Scotland after he had been refused by the Church of England.

Yet the author never seems clear as to which part of his material is important and which is not. Thus the reader encounters page after page of information about parishes grown more constant and devout. We are told of the graduation dates from college for nearly every character who appears in the text (William White [College of Philadelphia, 1765], the Reverend Bela Hubbard [Yale, 1758], William Smith [Aberdeen, 1774], et cetera ad nauseum). We learn such potent facts as that late in 1742 Robert Jenney, who had for many years been minister at Hempstead, accepted a call to Christ Church in Philadelphia. Naturally enough we learn that Jenney graduated from Dublin in 1709. Surely much of this information is nonessential and some could be of interest only to the most antiquated of antiquarians.

The author's style lacks both grace and clarity. Lengthy quotes are frequently included, too easily passed over and forgotten. Finding a page without several parenthetical expressions is a monumental occurrence. Obscure terms are used repeatedly without definition, a small difficulty perhaps, but it would have been easy for the author to tell his readers that "glebe," for example, was cultivable land owned by a parish or benefice, the income from which was used primarily for the support of a cleric. An early and continuing difficulty in Steiner's book arises from the plethora of Seaburys named "Samuel" with which he deals: a great-grandfather, a great-uncle, a father, a son, and a grandson of Bishop Samuel all bore the same Christian name. The author's style makes it difficult at times to know which man he is referring to.

It is unfortunate that Dr. Steiner chose to refer to the Congregationalists as "Presbyterians," which they surely were not. As they themselves consistently and continually made clear, the polity of each group was very different. One ponders the reasons for Steiner's choice in this matter. Furthermore, as a work subtitled A Study in the High Church Tradition, it makes little attempt to assess the theology, ethics, metaphysics, dogmatics, or Christology of either Seabury or the Episcopal Church. Of ecclesiology we are given a little, and in one chapter—the last save the epilogue —we are introduced to Seabury's view of the Eucharist. Surely more space could have been devoted to such topics and a little less space given over to informing us of graduation dates.

I have written of disappointments in the book, yet it has many good qualities. It contains everything anyone could possibly desire to know about Seabury and his daily activities, including a great deal of unfamiliar material that will add considerably to our knowledge of the man. It is based upon the solidest kind of research, and supernumeraries constantly remind the reader of the sources from which the material has been drawn. The notes, placed in the rear, cover almost one hundred pages! The bibliography is both extensive and inclusive, and the index leaves out no important name or topic treated in the text that I could discover.

CECIL B. CURREY
University of South Florida

DIRK HOERDER. Society and Government 1760-1780: The Power Structure in Massachusetts Townships. Berlin: John F. Kennedy-Institut, Freie Universität Berlin. 1972. Pp. ii, 200.

We can learn much from Dirk Hoerder's structural description of five Massachusetts towns. For the towns themselves—Amherst, Weston, Worcester, Plymouth, and Boston—Hoerder supplies some new data and, more importantly, a restrained statistical framework that allows genuine comparisons. Half the study is devoted to a detailed set of appendixes and a remarkable bibliography.

By beginning his description with Amherst (whose very newness blurred the expected correspondence between status, economic wellbeing, and political expectation) and then moving along a continuum of increasing age, size, and economic complexity Hoerder argues that colonial Massachusetts's egalitarianism was more the product of incompleteness than any radical redefinition of the social order. Settlements, which grew and survived, readopted reigning notions of social deference just as soon as their size and internal complexity allowed the identification of a coherent group of social notables. Once in power, these men looked after their family interest with all the tenacity one expects of a self-important middling gentry.

Hoerder's failings are equally instructive for they mirror—and occasionally caricature—our present preoccupations. Trained at both the University of Minnesota and Berlin's Free University, Hoerder's theoretical perspective is a curious amalgam of the European's notion of social class and structure and the American behavioral scientist's concern with political power and community typologies. Applied in a historical setting such a perspective leads Hoerder—just as it has so often led the rest of us—down blind alleys and into meandering diversions. Unfortunately our language for social analysis still lacks the suppleness a creative melding of history and numbers requires.

ROBERT ZEMSKY
University of Pennsylvania

MARVIN KALB and ELIE ABEL. Roots of Involvement: The U.S. in Asia, 1784–1971. New York: W. W. Norton and Company. 1971. Pp. 336. \$8.95.

Marvin Kalb is a diplomatic correspondent for CBS News in Washington: Elie Abel, once the same for NBC News, is now the dean of the Graduate School of Journalism at Columbia. They are judicious analysts and skilled writers, and they have put together a book that will provide the general reader conceptual continuity in making sense of the Vietnam trauma. In so doing they offer future historians much detail based on interview that will help illuminate the documentary mass already available, as well as the mountains that will some day be declassified. Instead of Roots of Involvement: The U.S. in Asia 1784-1971, however, the professional historian might wish to call it Vines of Entanglement: The U.S. in Indochina, 1945-1970. The title promises more than the book delivers.

The authors do not consider root causes closely. They do assemble a plausible narrative toward demonstrating that "the inexorable progression—from Yankee clipper to Yangtse gunboat to helicopter gunship—suggests that Vietnam was but a terrible moment in America's swashbuckling adventures in Asia." The book, however, is much better than the sentence quoted. An excellent work of narrative journalism, it soundly concludes that "even the most powerful of nations can exhaust and discredit themselves by overspending their resources in a dubious cause."

The book is not footnoted, nor are sources listed. The authors appear to be familiar with the most revealing of books with a Washington focus by participants and observers through 1970, but the Pentagon Papers were published at about the same time as their own book. They have not consulted works with primary focus on modern Vietnamese history, such as McAlister's The Vietnamese and Their Revolution (1970). The strength of the work of Kalb

and Abel lies in their accumulated knowledge as Washington correspondents, their two dozen acknowledged interviews with high-ranking officials, and other interviews with persons preferring anonymity.

What do they say? They say that Truman should have followed Roosevelt's lead in Indochina policy rather than Acheson's policy of putting European allies first, with its corollary of support of neo-colonial ventures. Eisenhower let Dulles draw a moralistic line against communism that led to a commitment to Vietnam -Eisenhower's Gettysburg speech of 1959-in the name of national interest. Kennedy tragically let slip the opportunity to take a critical new look at the involvement in Vietnam. Johnson, with a gravely narrowed list of options, plunged unreflectively into the most dangerous of them, and only by withdrawing from national life was he able to begin to reduce an "Asian obsession" to a "nuisance." The book was apparently finished some months after the Cambodian incursion and nearly two years before the Kissinger-Tho agreement; it ends on a cautiously trustful note that Nixon would continue to "winch down" the war. In my opinion-an opinion obviously subject to significant change as time proceeds and documentation accumulates—the book undervalues Eisenhower's conditioned refusal to engage in overt military support of France in Indochina in 1954, and overestimates the value of his Gettysburg speech. More important, it underestimates the gravity of the steps that Kennedy took in 1961 in response to the report from Taylor and Rostow.

There are, I believe, some points in this history when a different decision would have made a great difference. The authors are sensible enough to believe so, too, as their thoughtful handling of particulars frequently shows. Nonetheless, by insisting that Vietnam was not an "aberration" but a logical consequence of long-held American attitudes and assumptions, they proceed greatly to overstate the "inexorability" of American entanglement. They do not accept the assumption that the war was waged for motives of economic imperialism, and the lack of putative or real economic benefits supports their argument. But by speaking of romance and self-righteousness going back to the voyage of the Empress of China in 1784, they do not present a compelling case for inevitability.

A series of allusive connections woven into a narrative of disaster does not constitute the inexorable. George Ball was correct when he cautioned President Kennedy that by agreeing to the small increases in manpower asked by Taylor and Rostow in 1961 he would have to go to 300,000 men in a few years. Further increases took the commitment beyond half a million men. In 1968 Clark Clifford grew apprehensive that "someone is going to want to round it off at a million." The change came: a slow winching down of what had been slowly cranked up in the seven years previous.

Mechanical metaphor, however, is weak. This is a story of how some illusions became obsessions, and how the obsessions became intolerable to the people who held them. Some day there will be enough perspective on the story for it to be told as a major tragedy of American history, and of the Vietnamese, Cambodian, and Laotian histories upon which American actions impacted.

THEODORE FRIEND Swarthmore College

RICHARD R. BEEMAN. The Old Dominion and the New Nation, 1788–1801. [Lexington:] University Press of Kentucky. 1972. Pp. xiv, 282. \$11.00.

This book would have made a good, longish article or perhaps even a monograph of fifty or seventy-five pages had the new and useful information in it been separated from the considerable space devoted to surveys of foreign affairs (as in discussions of Jay's Treaty and Franco-American relations in 1798), oversimplified accounts of national politics (Hamilton's financial policies, for example), and misunderstandings of theoretical issues (such as efforts to extend religious liberty in Virginia). Beeman shows us in illuminating detail how the Virginia press reacted to national issues during the period 1788-1801, how public meetings in many counties were used by both Republicans and Federalists to clarify issues and to consolidate support, and how the traditional system of control by "leading families" remained substantially in effect within both parties in local and state government. Such a descriptive monograph would have made available data valuable to subsequent studies of the politics of the Federalist era.

As it stands, though, the book is simply too often dubious or misleading in its interpretations. In "proving," for example, that James Madison switched from being a nationalist before 1790 to being a Virginia parochial after that date, in order to retain his power in the traditional state political structure, Beeman

fails to mention Madison's two years as a member of the Virginia Governor's Council and his assiduous attention to state problems during his years in the Continental Congress—a stance abundantly clear in recently published volumes of The Papers of James Madison. Just as injudicious is the author's assertion that Madison's opposition to Hamilton's funding and assumption plans rested on the Virginian's state bias. Madison's position was indeed mindful of state interests, but his statesmanship was to see how these local needs were consistent with a plausible, attractive, perhaps more noble conception of national growth than that of the secretary of the treasury. Furthermore, Beeman's argument that Virginia anti-Federalists opposed grand titles for the president in 1789, entirely overlooks Richard Henry Lee's firm support for them in the Senate, another example of the author's distressing tendency to ignore facts unsuited to his thesis.

The major misconception marring the book, though, is that Jeffersonian Republicanism was basically hypocritical because it did not repudiate the traditional political system in Virginia, extend the franchise, free the slaves, establish a public school system, and virtually dismantle the federal government. Since no one in Virginia during the 17903 (and certainly not John Taylor of Caroline whom Beeman often uses as an example of a "radical" Republican) advocated such a program, it is very easy, of course, to "prove" that the Jeffersonians did not! Hence the author incessantly topples straw men and puts down the Jeffersonians for not being 100 years ahead of their time. Unfortunately such uniruitful analysis largely submerges the useful information Beeman has gathered about reaction in Virginia to national political issues.

RALPH KETCHAM
Syracuse University

NATHAN REINGOLD et al., editors. The Papers of Joseph Henry. Volume 1, December 1797—October 1832: The Albany Years. Washington: Smithsonian Institution Press; distrib. by George Braziller, New York. 1972. Pp. xxxix, 496. \$15.00.

The name of Joseph Henry is well known to many American scientists because of his association with the Smithsonian Institution (of which he was the inaugural secretary) and because the unit of self-inductance is called the "henry" in his honor. But he is a shadowy figure, even for historians of physics who may not have a special interest in American science. A central problem is that while Henry was unquestionably the foremost American physicist in the years between Benjamin Franklin and J. Willard Gibbs, it is difficult to determine just how good he was. Credited with some major experiments and concepts in electromagnetism he was all but eclipsed by the great Faraday, and he never achieved the eminence of being elected a foreign fellow of the Royal Society or a foreign associate of the French Academy of Sciences.

In mid-career Henry gave up an active life as professor at Princeton and vigorous experimenter to serve the larger community of science and the nation as secretary of the Smithsonian Institution. To this day the Smithsonian follows in broad outline the series of activities Henry conceived to be its mission, in terms of James Smithson's will: "to increase and diffuse knowledge." The introduction of the first volume of Henry's papers sees him also as "a founder of the American scientific community."

A major event in Henry's career was his appointment to Princeton, in 1825, the end point of the present volume. At that time he had achieved a significant position in Albany and was well launched on his career in experimental electricity. There exists very little manuscript material on Henry's early life, education, or family background. The editors have decided that, "aside from documenting Henry's personal career, a major task of this volume is to describe the Albany milieu as an influence on Henry's scientific growth." A footnote refers to "a brief family history as told by Henry's aunt, Elizabeth Selkirk," which contains Henry's own "notes on Elizabeth's account," but this document is not printed. Its omission evidently represents an editorial point of view concerning the significance of a scientist's family and ancestry. This conclusion is substantiated by the editors' own declaration of the "working resolutions" by which the volume was put together: "The first is to document Henry's research and professional career for an understanding of both science and the growth of the national scientific community. The second is to use the life of Joseph Henry as an occasion to present a broad documentary history of a period and a place, not merely a narrow recital of events in a career. The life becomes the thread upon which the beads of history are strung." Thus the aim of the editors is to demonstrate that science is "integrally part" of "the national culture" and to achieve the goal of merging "imperceptibly the scientific work and the general background," and thus "to present few sharp edges between the 'internal' life of science and the 'external' milieu." In any event there appears to be very little surviving personal documentation of Henry's personal life or even experiments; accordingly, there is "very little to say about Henry before his twenty-fifth year." Again, because "so few manuscripts survive," Henry's "Albany work on electromagnetic induction is barely touched upon."

The first volume of these Papers is a monument of editorial industry. The editors have gathered together a mass of documentary information concerning the scientific, cultural, and social life of Henry's Albany: concentrating on two institutions with which he was associated, the Albany Lyceum of Natural History, of which he was one of the curators, and the Albany Institute. Albany in those days was the eighth or ninth largest city in the United States, a major center for state government, industry and finance, and culture. While we may regret the lack of information concerning Henry's own education we must applaud the editors' assemblage of documentary information on so many aspects of the cultural life of the Albany community of his intellectual formation, especially the sequence of public scientific lectures that he might have attended. Indeed, Henry apart, the editors have provided a model documentary history of the intellectual life of a major American city in the second and third decades of the nineteenth century that-inter alia-shows the great extent of interest in science. Thus this volume should become a primary source for cultural historians.

The editors are especially to be congratulated on thus reconstructing the background of Henry and his education for the years in which no documentation is available. Indeed, Henry himself comes alive in these pages only in his early maturity, with his correspondence with Harriet Alexander, whom he married in 1830, and with his notes and correspondence on scientific matters in the years 1830–32. These concluding documents of Henry's Albany period whet the scholarly appetite for the succeeding volumes.

The level of scholarly editorship of this volume is exceptionally high—not only in finding documents but in presenting them with a full panoply of editorial comment and ex-

planation (or amplification). There are very few readers, if any, even specialists, who will not find much that is new in these pages. It should be added that the volume is beautifully printed, in a manner worthy of the subject, and is reasonably priced.

I. BERNARD COHEN
Harvard University

FRANCIS N. STITES. Private Interest & Public Gain: The Dartmouth College Gase, 1819. Amherst: University of Massachusetts Press. 1972. Pp. 176. \$9.50.

STANLEY I. KUTLER. Privilege and Creative Destruction: The Charles River Bridge Case. Philadelphia: J. B. Lippincott Company. 1971. Pp. 191. Cloth \$5.95, paper \$2.95.

Stites offers a case analysis of the famous Dartmouth College v. Woodward decision, which extended the contract clause of the Constitution to charters of corporations. Making few promises, and regrettably keeping his word, he states that the purpose of his study is simply to pull together the abundant material on the case that we already have and present it as a composite whole. The results of his research are even more disappointing than expected. The author fails to add measurably to our understanding of the decision and, what is worse, falls woefully short of the modest goal he has set for himself.

Drawing principally from the records and correspondence contained in the Dartmouth College Archives, Stites tells us more than we want to know about the tactics of the rival factions at the college and of the lawyers involved in the litigation; at the same time, the reader is left wondering whether anything worthy of mention took place beyond college walls. The author's research concerning events at the college is extensive and at times fastidious; but he scrupulously avoids such basic sources as newspapers of the day and most of the primary and secondary materials on Justice Story, a principal figure in the case; the concurring opinions of Justices Story and Washington are likewise passed over in his superficial analysis of the decision itself. He merely alludes to the decision's beneficial impact on business corporations and private colleges, missing his last opportunity to redeem the book. As a consequence of these and countless other errors and omissions, the author is unable to place the decision in its proper setting or relate it clearly to the political, legal, and cultural history of the country. What we have here, at best, is a fatiguing article masquerading as a book. Happy is the library that did not purchase this cheaply printed, overpriced monograph.

Turning to Kutler's balanced and readable study of the decision that qualified the Dartmouth ruling, we leave the marshes, reaching high ground. "The Charles River Bridge case," he perceptively observes, "may well mark the origins of dissent within the Supreme Court." Mindful of the larger issues, Kutler paints a rich background, depicting the pecuniary interests at stake before the decision was handed down; he then completes the picture by showing the effects of the decision on American legal and economic history. The importance of the case stems from its legal rationale justifying "creative destruction"—that process whereby new enterprises and inventions create new goods and services and destroy existing ones in the name of progress. "It is this aspect of the case that transcends the relatively limited conflict over bridges in the Boston area, making it a landmark involving the interrelationship of public policy, technological change, capital development, competition, and law.'

But "creative destruction" is not a legal rationale Kutler applauds in his final evaluation of the merits of the decision. Donning the hat of the early nineteenth-century conservative who criticized the application of the Utilitarian ethic to capitalism, the author indulges the "privileged" proprietors of the old bridge and their spokesman on the Court, Judge Story. Standing behind Tocqueville, who stressed the need of protecting economic minorities, Kutler condemns the expediency of the Massachusetts legislature; for when it authorized the new, free bridge and refused to compensate adequately the proprietors of the old one, it violated "in the noble name of 'community rights'" the "rights of others within that community." And it may be true that Taney's decision supported the public interest, but this interest should not always be determined by counting heads. "Surely," he concludes, "it is not always just-or even useful-to rescue that interest from the altar of privilege, only to sacrifice it in a holocaust to immediate popularity."

JAMES MCCLELLAN
Hampden-Sydney College

RONALD P. FORMISANO. The Birth of Mass Political Parties: Michigan, 1827-1861. Princeton: Princeton University Press. 1971. Pp. xii, 356. \$12.50.

MARVIN E. GETTLEMAN. The Dorr Rebellion: A Study in American Radicalism: 1833-1849. New York: Random House. 1973. Pp. xxii, 257. \$8.95.

Since the pioneering work of Lee Benson on New York (1961) and William W. Freehling on South Carolina (1965), microcosmic studies have provided a montage of conflicting conclusions about Jacksonian America. These two books add to this growing corpus.

Ronald P. Formisano is Benson's most impressive student. Going far beyond his mentor in the utilization of quantitative techniques and in the sophistication of analysis, his case study of Michigan party development will remain a landmark in the historiography of this "era of the common man." Using election returns and contemporary testimony as evidence, as well as expanding upon Alexandra McCoy's excellent analysis of elite behavior, and testing all this with analytical techniques borrowed from the behavioral sciences, Formisano argues that political controversy in the Wolverine State revolved not around socioeconomic divisions but rather around ethnocultural ones. Whatever his weaknesses (a plodding literary style, difficult organization, occasional inconsistencies, and excessive repetition are among them), Formisano lays bare the problems of an economic conflict interpretation of early American politics. Far more important than economic status in determining political preference was region of birth (the Yankee versus Yorker battle continued on the western end of Lake Erie), religious preference (nonevangelical Catholics, Episcopalians, Methodists, and Lutherans versus evangelical Presbyterians, Congregationalists, and Baptists), and national origin (native whites versus immigrants). But in destroying the class conflict thesis, he does not adopt a consensual school theme of political infighting merely for the fruits of office. There were differences between Whigs and Democrats. The former's evangelical tradition considered partisanship to be secular and profane and allowed individual conscience to determine party allegiance, while at the same time Whigs sought "an organic society with specific ethnocultural and religious traditions" (p. 127) achieved through governmental enactment and enforcement. The democracy's adherents valued individualism over governmental control of ethical standards and supported a tolerant attitude regarding the immigrants' role in politics while subordinating individuality to party regularity. As long as the Protestant majority remained divided over various issues, the Democrats normally won.

A series of events combined to destroy the Democratic monopoly during the 1850s. The rapid increase in foreign, largely Catholic, immigration, the entrance of the Catholic hierarchy and Mormons into politics, a revulsion at Southern dominance of national politics, a growing intransigence relative to such issues as slavery extension, temperance, public education, and voting registration, and an animosity toward conventional politics and politicians all contributed to political independency and support of the Republican party. Particularly significant was the shift by the Methodists and New British immigrants into the evangelical, Republican camp. While this is a somewhat captious criticism, it would have been interesting if Formisano had correlated mass voting behavior with legislative voting behavior, which could add significantly to our understanding of partisan solidarity and the persistence of antipartyism.

While Formisano makes significant additions both to the understanding of Jacksonian politics and to historical methodology, Marvin E. Gettleman's analysis of the quest for broadened suffrage in Rhode Island does neither. One wonders if the kind of analytical techniques used by Formisano would contradict or confirm the geographic-economic cleavage Gettleman portrays. When Gettleman tries to use Robert Merton's "The Self-Confirming Prophecy" to support a contention, it is stuck in a footnote and not integrated into substantive argumentation. The failure to use such paradigms as Harry Eckstein's on internal wars limits the value of this monograph. Moreover, because of the author's sympathy with Thomas Wilson Dorr (1805-54) and his most avid followers, he fails to acknowledge that the Suffragists secured relief from their major dissatisfactions with the Charter of 166g upon the approval of the "Algerine" Constitution of 1845. Despite his attempt to show how "the nation's revolutionary beginnings transmitted an undercurrent of genuine radicalism" to "antebellum American political life" (p. xx), it is equally apparent that the same revolutionary tradition provided intellectual sustenance to the charter's supporters. And 'this "tradition" with its resort to violence and justification by popular sovereignty appeared elsewhere in Jacksonian America—the nativist riots of Eastern cities, antiabolitionist mobs, California's vigilante

groups, bleeding Kansas, and the anti-Mormon riots. Any tie to the Revolution is unproven. Finally, in discussing the external causes of the Dorrites' failure, Gettleman omits an obvious conclusion of his own evidence—timely concessions by the opposition. Though his writing style is lucid and felicitous, it is filled with loaded words such as "cruel," "brutally," "impassioned," "passionate," "haughtily," and "learned." Thus, while providing the best available description of the Rhode Island affair, The Dorr Rebellion makes little substantive contribution to our understanding of Jacksonian America or of any radical tradition.

DAVID CURTIS SKAGGS

Bowling Green State University

ROBERT J. PARKS. Democracy's Railroads: Public Enterprise in Jacksonian Michigan. (National University Publications, Series in American Studies.) Port Washington, N.Y.: Kennikat Press. 1972. Pp. 261. \$12.50.

Until the seminal works of Louis B. Hartz and Oscar Handlin appeared nearly three decades ago, historians, often conditioned by an ethic of private enterprise and lacking tools of economic analyses, gave short shrift to the role that state governments played in the nineteenth century in the development of transport facilities and to the economic meaning of these and other forms of social overhead capital. Now scholars are gradually constructing a body of literature giving a new account to governmental enterprise and economic progress. In this study, an outgrowth of a dissertation based primarily on public records, Robert Parks effectively narrates and analyzes the ways in which the territorial and state governments of Michigan, usually dominated by Jacksonian Democrats, planned, built, and operated railroads in the 1830s and 1840s.

Seeing the ideology of progress, the involvement of the national government in the promotion of internal improvements, and the national credit structure as external forces at once supporting and inhibiting railroad projects in Michigan, Parks asserts that essentially a growing population, its fortunes dependent on the marketing of agricultural produce, prevailed in a call for the state, which it saw as an agency to protect the people against monopoly, to build a publicly owned railroad system. Local interests, appearing to compose sectional groupings, then compelled the state legislature to approve the construction of three main railroads, the Southern, Central, and

Northern roads, and ancillary railroads and canals. Unfortunately expenditures for multiple projects deprived the one really viable railroad, the Central road, of funds for more motive power and rolling stock. Indeed, even though the state managed the Central road reasonably well and realized profits from its operation, the state legislature finally decided to sell it to private interests rather than increase the public debt or taxes to finance a rebuilding that might have enhanced the road's profits. Neither the Southern nor the Northern lines, the author concludes after a careful financial analysis, met the test of profitability.

Looking at the broader economic import of the state railroad system Parks finds that the Central and Southern railroads had "Smithian" effects in promoting agricultural specialization in the areas they traversed. He ascribes only nominal importance to the railroads in generating manufacturing linkages. Arguing that the Central and Southern roads ran through sections where the density of population in the 1830s could yield satisfactory returns he challenges Albert Fishlow's assumption that these railroads were constructed ahead of demand. More stridently he dismisses Robert Fogel's contrafactual proposal for a canal system in Michigan as an adventure in abstraction. Rather, insists Parks, one single railroad from Detroit to St. Joseph or Chicago, with appropriate feeder lines, would have more productively served the state. Some state legislators saw the wisdom of such a project, but economic realities were not compatible with representative democracy.

Unquestionably, this study is a solid contribution to the literature bearing on the relationship between state governments and economic development. If there is some general criticism to offer here it is that owing in part to his compartmentalization of economic and political issues, Parks does not sufficiently tie political ideology to economic decisions, especially in respect to the sale of the Central railroad. The title of the book is a bit misleading: readers without a specialized knowledge of the political terminology of the period may assume that the author is referring to democracy in a generic sense when, of course, he is using the old denomination of the Democratic party. He is not convincing, incidentally, in portraying the initiation of the state system of railroads as a special triumph of the Democrats and indeed acknowledges that the Whigs were not opposed to the initial program for the system. Historians not well versed in economic analyses, particularly with the concepts of external economies and the technical indivisibility or "lumpiness" of social overhead capital, may occasionally struggle for understanding, but they should find the effort rewarding.

CARL M. BECKER
Wright State University

WILLIAM GERALD SHADE. Banks or No Banks: The Money Issue in Western Politics, 1832–1865. Detroit: Wayne State University Press. 1972. Pp. 328. \$15.95.

This is a detailed study of the bank issue in the states of the Old Northwest—Ohio, Indiana, Illinois, Michigan, and Wisconsin—in the thirty-odd years from Andrew Jackson's "Bank War" to Appomattox. Using impressive research as well as quantitative analyses of voting in the legislatures, constitutional conventions, and referenda to the electorate, the author seeks to add a new perspective and to prove the major thesis that the antebellum fight over banks was not "simply a conflict between the haves and the have-nots," but "an aspect of the broader conflict between political subcultures that structured partisan controversy within the area" (p. 18).

The book traces the shaping of attitudes of two opposing coalitions. "Agrarian-minded" Democrats, who by the mid-1840s dominated the party's thinking, moved from the exoneration of Old Hickory and the blame of the banking system for the coming of the panic and depression of 1837 to a "hard" position of opposition to all banks. Banks corrupted the legislative process and violated the Constitution, equal rights, historical experience, and "true philosophical principles." To this group —a heterogeneous compound of Germans, Irish Catholics, and upland Southerners within the Democrat party—banks became a symbol of and a focal point for resistance to an increasing commercialization of American life and culture fostered by Yankee-Protestant Whigs and after them Republicans, who came to regard free banks under general incorporation laws with notes based on government paper as tools vital to economic development and general prosperity. These two antagonistic points of view would be sustained in bitter debates in constitutional conventions in all five states between 1846 and 1851, in arguments over freebanking legislation proposed (and adopted) by four of the states in 1851 and 1852, and again, in discussions after the fiscal debacle of 1857, and especially during the political crisis of 1860-61, out of which national banking laws would ultimately come.

This is a thoughtful, carefully executed book. Yet its main thesis is at times fragile and elusive. Thought and rhetoric on the bank question are not always separable. Factor analysis is not completely reassuring, and one has to wonder why all vote maps and tables are concerned only with Illinois. At the same time one must admire Banks or No Banks as a serious attempt to view both the real and symbolic dimensions of an issue in the broadest social context:

CLARK C. SPENCE
University of Illinois,
Urbana-Champaign

ALAN DOWTY. The Limits of American Isolation: The United States and the Crimean War. Foreword by HANS MORGENTHAU. New York: New York University Press. 1971. Pp. xii, 272. \$9.50.

Alan Dowty distinguishes between American isolation during the nineteenth century and the isolationism of the 1920s. In the 1800s United States policy toward Europe combined neutrality with selective probing for political advantages. The interwar twentieth-century imitation was merely a passive aloofness unworthy of a great nation.

To document this thesis Dowty has minutely traced the diplomacy of the Franklin Pierce administration, which he considers a representative model. The essence of his interpretation is that President Pierce, Secretary of State William L. Marcy, and their diplomats perceived the Crimean War as a crisis that would draw European attention from the Western Hemisphere. The United States then could seize the opportunity to expand politically into the Caribbean, Central America, and Hawaii. These goals were not realized because the president and his State Department indecisively vacillated between bluff and the use of force, never successfully blending the two into an effectively expansionistic compound.

The theme that Europe's disputes work to America's advantages is not new, but Dowty has added the important qualification that at least one administration failed to gain from a major European political and military conflict. A quite different conclusion can, however, be reached from the evidence presented in this book. Passage after passage of quoted diplomatic dispatches to the State Department assert that the primary purpose of American statecraft is support of overseas American commerce. Marcy's man in Moscow, Thomas H.

Seymour, succinctly warned of the economic significance of the Crimean War. American commerce had "recently outstripped that of every other nation," Seymour wrote. "It would not be strange, if the continuance of a war, which manifestly is to be carried on for the purpose of destroying the trade of Russia by sea, and the destruction of her sea-port towns, should seriously affect us." In other words the Crimean War was yet another test of the viability of America's policy of neutrality, a strategem designed to maximize profit from oceanic trade during European wars.

Dowty admits that his documents demonstrate a keen interest in American foreign trade within the State Department and among diplomats, but he insists this concern was secondary to thoughts of political and territorial expansion. This insistence probably flows from a set of assumptions held by those students of international affairs who consider themselves "realists." To them politics and power are the primary preoccupations of governments. The seeking of economic advantages is a decidedly secondary objective.

Students who comprise the William Appleman Williams school of diplomatic history would reverse that order. Should one of them study Dowty's evidence he would probably conclude that Pierce failed to expand politically into the Caribbean, Central America, and Hawaii simply because that goal was not uppermost in his mind. The president and his State Department dedicated themselves instead to the real function of nineteenth-century American diplomacy, enlarging the sphere and profitability of overseas American economic activity.

KENNETH J. HAGAN
United States Naval Academy

ANNE L. AUSTIN. The Woolsey Sisters of New York: A Family's Involvement in the Civil War and a New Profession (1860–1900). (Memoirs of the American Philosophical Society, volume 85.) Philadelphia: the Society. 1971. Pp. xv, 189. \$3.00.

Specialists in women's history and the Civil War as an institutionalizing force will find material in this chronicle of the private lives and public activities of Abby, Jane, and Georgeanna Woolsey. Since the author's purpose is somewhat narrowly defined, however, as a desire to win proper recognition for the Woolseys' contribution to the professional nursing movement and other forms of social welfare, the

book is difficult to assess because, while achieving her stated purpose, she leaves undeveloped significant possibilities inherent in the material spread out before the reader.

The Woolseys were an affluent, cultivated family of ancient lineage and distinguished connections, living in New York before, during, and after the Civil War. Seven of the eight children of Charles and Jane Woolsey were daughters, born between 1828 and 1839, and coming to adulthood in the 1850s when feminine assertiveness reflected the broadened education and rising expectations of some urban middle-class women. The author draws on an amply filled storehouse of family letters, diaries, journals, and privately printed memoirs for a sketchy portrait of the educational background, social environment, intellectual, social, and political interests of the seven sisters. Their individuated life styles reflect the measure of their liberation.

After the fall of Fort Sumter, Abby, Jane, and Georgeanna Woolsey were totally committed to war work. Abby was associated with Dr. Elizabeth Blackwell in organizing the Women's Central Association of Relief, forerunner and source of the initiative out of which came the U.S. Sanitary Commission. Jane and Georgeanna pioneered in the emergency hospital camps near Washington, finding ways to make themselves useful, as self-appointed angels of mercy amidst the hapless and unhygienic confusion during the formative months of the Sanitary Commission's hospital-related activities. Their letters and reports interpreted the needs, for Abby's use on the home front, and offered fleeting glimpses of Frederick L. Olmsted, Dorothea Dix, and assorted samples of obtuse officialdom. They documented the incredible medical and dietary procedures that made hospitals far more dangerous than battlefields and furnished a record of the development of nursing service and hospital management from Ball's Bluff to Gettysburg.

The experiences of the Woolseys, in many respects parallel to those of Florence Nightingale in Crimea, gave rise to the conception of the need for professionalized nursing education. Florence Nightingale provided the model for the schools of nursing education established by the Woolseys in New York and New Haven in 1873, with stiff criteria for entry and high standards for training, and for the subsequent development of nursing as a professionalized occupational sphere for women.

LOUISE M. YOUNG Washington, D.C.

MILTON PLESUR. America's Outward Thrust: Approaches to Foreign Affairs, 1865–1890. DeKalb: Northern Illinois University Press. 1971. Pp. vii, 276. \$12.50.

Among the growing volume of recent works on the origins and history of American imperialism, we are fortunate to have the present study on the last third of the nineteenth century. When so much of the literature is nationally deprecating and (in the name of economic determinism) preoccupied with proving the influence of special interest groups upon foreign policy, it is refreshing to find a scholar seriously exploring a range of determinants in a more balanced approach.

Plesur's main theses are that the United States did not suddenly and almost unwittingly burst onto the imperial scene (a myth highly improbable on its face); that the molders of public opinion had as much to do with promoting a more aggressive world policy as did exporters and shipping lines (the evidence is substantial); and that indeed, ideology frequently overcame palpable economic self-interest in moving the nation toward overseas territorial expansion over the objection or misgivings of the commercial establishment. The technique is what the author calls an "attitudinal study," generalizations being made upon random samplings of newspapers, trade and professional journals, and other indices of public opinion. What the traditional—and the more radical student of diplomatic history will find missing is archival documentation on the one hand, and statistical correlation on the other.

Familiar ground is covered in such topics as American territorial interests in Santo Domingo, Cuba, Hawaii, Samoa, the Congo, and the Orient. The exploration of the same theme in the Middle East and in the international conference movement is more novel. The Spanish-American War is treated as a culmination of a chain of precedents, not as "the great aberration" of foreign policy. One could argue with the allotment of space to the various topics, and the volume unfortunately projects the image of a collection of somewhat unrelated chapters. However, it is a valuable counterbalance, and it should be useful in updating such works in the field as those by David M. Plecher and Walter LaFeber. But there is yet more light to break forth upon American foreign relations in the late nineteenth century.

o. LAWRENCE BURNETTE, JR. Stratford College

ALWYN BARR. Reconstruction to Reform: Texas Politics, 1876-1906. Austin: University of Texas Press. 1971. Pp. xiv, 315. \$8.50.

For many years the literary vehicle most commonly used for late nineteenth-century United States political history was either biography or monographic studies of Reconstruction, Populism, Greenbackers, and the like. In either case writers had a natural tendency to exaggerate the importance of the man or the movement about which they were writing. Recently, excellent reviews of state politics have emerged that take a somewhat broader and more comprehensive view of the period and that successfully meld the major and minor personalities and movements into an understandable whole. Alwyn Barr's Reconstruction to Reform is such a book about Texas politics, and the author is to be commended for a job well done.

Barr's point of departure is 1876, the year the adoption of the Bourbon constitution closed the door on Texas Reconstruction. Vestiges and overtones of Reconstruction racial politics remained for years to come, of course, but Barr is essentially correct in declaring that after 1876 economic and sectional disputes eclipsed race as the major issues dividing people and politicians. The study terminates with the adoption of the Terrell election laws, approved in piecemeal fashion shortly after the turn of the century, which noticeably affected the contest for governor for the first time in 1906.

One wishes Barr had begun his study with 1865 rather than 1876. His two short introductory chapters dealing with the election of 1876 infer an understanding and knowledge of Reconstruction that would have erased, perhaps, many of the Dunning school assumptions under which Texas history of that period still suffers. However, the post-Reconstruction period provides a stimulating challenge to historians, and Barr grapples with those less emotional, but fundamentally more important, issues of commercialization of agriculture, growth of business monopoly, and the closing of the frontier. The agrarian revolt and third-party movements that developed in response to these economic forces are reviewed in a thoroughly professional and competent fashion. We are apt to learn more about the role black people played in those years when monographic studies now underway are completed, but for the moment Barr is thoroughly conversant with the most recent writings on that point and, except for this one area, his book is apt to be the standard for the period for many years to come.

JAMES A. TINSLEY
University of Houston

CARL H. CHRISLOCK. The Progressive Era in Minnesota, 1899-1918. (Minnesota Historical Society, Public Affairs Center Publications.) St. Paul: the Society. 1971. Pp. xiii, 242. \$7.50.

DAVID P. THELEN. The New Citizenship: Origins of Progressivism in Wisconsin, 1885–1900. [Columbia:] University of Missouri Press. 1972. Pp. 340. \$12.00.

Scholars of the early twentieth-century Progressive era in the United States have often dreamed a dream. It seemed likely that once research and writing moved from the area of national politics, biography, and generalized intellectual history to state and local studies many of the glaring disagreements on fact and interpretation among established scholars in the field would be resolved. At present there are about three dozen published studies of state and municipal social and political developments during the era, together with ten times as many unpublished ones. The long awaited time for historical synthesis should be at hand.

Carl H. Chrislock's The Progressive Era in Minnesota, 1899-1918 is unlike most state studies in that it is not a doctoral dissertation; his unpublished dissertation completed at the University of Minnesota in 1955 was "The Politics of Protest in Minnesota, 1890-1901: From Populism to Progressivism." The present book is a well-documented work by a mature scholar of not inconsiderable literary talents. As the author concedes, it belies its own title by skipping lightly over the period when Progressivism was at its height (so as not to duplicate published biographies of Progressive governors) and focuses on the circumstances and consequences of the movement's decline.

David P. Thelen's The New Citizenship: Origins of Progressivism in Wisconsin, 1885-1900 is much more like previous state studies. A Ph.D. dissertation initially, it is literate, though hardly literary, and rests on prodigious research. The work developed from Thelen's prize-winning master's essay at the University of Wisconsin, The Early Life of Robert M. La Follette, 1855-1884 (1966), the research for which convinced him that Wisconsin's Progressivism could not be understood in terms of La Follette's leadership, quite the contrary. But, unlike many state and local studies, Thelen's book is not just a survey of social and economic developments melded vaguely with political administrations; it is brilliantly, even belligerently interpretive. Chrislock, on the other hand, begs off the kind of interpretation that might aid synthesis. After a brief, witty summation of interpretive controversies about national Progressivism, he concludes: "My position is flexible. To a greater or lesser extent . . . [Progressivism] may have been all of these things."

Though the books deal with Progressivism in individual states before it existed or after its peak, both authors believe their conclusions to be relevant to Progressivism generally. As such the works suggest how difficult the eventual work of synthesis is going to be. Take, for example, as most previous studies have insisted, the peerless Progressive leader, Robert M. La Follette. Chrislock continues to regard him as such, but Thelen sees the Wisconsonite as an opportunist whose Progressivism was largely an "out politician's" response to the rising progressive mood. Chrislock uses La Follette, declaring that the "name was almost synonymous with radical progressivism," as a foil for disputing the view that Minnesota's governor, John A. Johnson, was "a reformer par excellence." Yet Chrislock's criticisms of Johnson in contrast to La Follette are virtually identical to Thelen's charges against the Progressivism of the Wisconsin leader.

Chrislock faults Johnson for, unlike La Follette, not dedicating "his career to . . . crusades for righteous causes," for having "consistently adjusted to his surroundings," for too much of a "capacity for establishing rapport with all kinds of people," and for having earned the "harshest criticism . . . from a group of advanced progressives." Anyone searching for synthesis would have to conclude that if Thelen is right about La Follette (I believe he is not), then Chrislock is wrong about Johnson, or vice versa.

More of an obstacle to synthesis even between two Midwestern states so similar as Wisconsin and Minnesota is the scholars' views of what large city, small town, and rural groups contributed to the movement. Thelen puts great emphasis upon the leading role of Wisconsin's largest city, Milwaukee, while at the same time arguing that "size of home town" had little, if any, relevance to the making of a reformer. Chrislock, on the other hand, declares categorically: "The main push came from the nonurban middle class. Small-town Minnesota supplied most of Progressivism's visible power structure." The contradiction may be partly resolved by an interesting proposition that Thelen generalizes for several states other than Wisconsin, a proposition that Chrislock's data suggests may have been true of Minneapolis and St. Paul. Urban Progressivism is held to have prospered more in states like Wisconsin with a single, dominant metropolis than in states where two or more urban centers competed for hegemony. Even so, such sharp contrasts between the movements Chrislock and Thelen describe exceed by far the differences their analyses make evident between the two states.

Chrislock's failure to attempt interpretive rigor saves him from criticism for much beyond that failure; Thelen's indefatigably rigorous effort at interpretation opens him to challenge. He feels that established scholars have failed to see the importance of consumer and taxpayer protest at the local level. The early Progressives' orientation as consumers and taxpayers led them to doubt "whether the existing political economy could ever meet their needs" and led them, through an admirable resort to "direct democracy" and such devices as the income tax, to try to "produce fundamental changes." One boggles a bit, with so bold a thesis, at not finding consumers mentioned again until a third of the way through a 312page book. They receive throughout, in fact, rather sparse attention. Thelen actually is far more preoccupied with a corollary to his main thesis, that it was the depression of 1893 that brought producer, ethnic, and what-have-you groups in Wisconsin together as beleaguered consumers and taxpayers to open the way to Progressivism. He concludes bluntly: "Without the depression of 1893-1897 there would have been no Wisconsin progressivism," certainly a strong enough Q.E.D. for a clearly stated hypothesis. But one wonders why Thelen failed to consider alternative hypotheses. It is possible to argue, as some have, that financial panics and depressions repress reform desires in the very urban, middle-income groups that Thelen emphasizes. A large part of his assiduously collected evidence can be used to support that very proposition. Had Thelen weighed his evidence in the light of such a hypothesis, as well as his own, his case might not be, as it is now, unproven. In sum, both books, for all their excellence, are disappointing as works of interpretation.

CHARLES FORCEY
State University of New York,
Binghamton

YONATHAN SHAPIRO. Leadership of the American Zionist Organization, 1897–1930. Urbana: University of Illinois Press. 1971. Pp. xi, 295. \$9.50.

SAUL S. FRIEDMAN. No Haven for the Oppressed: United States Policy toward Jewish Refugees, 1938–1945. Detroit: Wayne State University Press. 1973. Pp. 315. \$15.95.

Despite differences in tone, method, and sub-

ject matter Friedman's impassioned study of the relationship between American Jews and their government in the era of the Holocaust and Shapiro's cool assessment of American Zionist leadership are intimately bound. The virtual annihilation of Europe's Jews during World War II and the emergence of the state of Israel have crystallized now, a generation later, a new revisionist American Jewish historiography of which these two significant books are representative.

A Columbia-trained Israeli teaching historical sociology at Tel Aviv University, Yonathan Shapiro brings the sophisticated perspective of the gifted outsider-insider to his study of the American Zionist movement to 1930 when what he calls "Palestinianism"-"all Jews should help in the upbuilding of Palestine as a national Jewish home"-replaced earlier aspirations for Jewish cultural solidarity and became the official ideology of the American Zionist Organization. Shapiro asserts that "Palestinianism" became the price of American Jewish acculturation, "the cornerstone of a specific American Jewish culture" (p. 6), and the continuing basis for Jewish group integrity. A student both of Sigmund Diamond and Robert K. Merton, Shapiro gives particular attention to "the changes in the status-set and role relations of the members" (p. 4) of the ZOA and the American Jewish community that brought this about. Within the bounds of his own sociological parameters Shapiro's analysis is most persuasive. The historical sociologist, however, stumbles badly when the ventures into the realm of biography to cement his argument. For Shapiro, Louis D. Brandeis is the prototype of the marginal man turned ethnic politician, the inauthentic leader "from the periphery" who singlehandedly transformed Zionism into Palestinianism. Shapiro insists that the outstanding leader of American Zionism "around whom all Zionist activities in the United States revolved from 1914 to 1921" (p. 99) almost barefacedly co-opted the Jewish electorate to advance his own public career. Frustrated in his ambition to become attorney general of the United States in 1912, Brandeis promptly and conveniently became a political Jew and Zionist to ensure his appointment four years later to the United States Supreme Court, the first Jew to be so designated. The circumstantial evidence for Shapiro's conclusion is a classic illustration of the post hoc, ergo propter hoc fallacy. Alpheus Mason, Ben Halperin, Stuart Geller, and Melvin Urofsky have demonstrated that this proud scion of a well-known Jewish

family was driven to champion the powerless in movements that coincided with his own austere set of liberal-populist values. Brandeis, a son of immigrants from Bohemia, a native of Kentucky, and a student in Germany, changed his middle name from David to Dembitz in 1882 upon his appointment as lecturer to the Harvard Law School. This was a tribute to the only professedly Jewish member of his family, his uncle, Lewis Dembitz, a Lincoln elector, an honored attorney, an orthodox Jew by conversion, and an early Zionist. As "the People's Attorney" Brandeis was a remarkably authentic if unconventional Jew, his opaque style and demeanor veiling the fiery historical consciousness of a born loner. A biographical study of American Zionist leaders is sorely needed to lend depth and subtlety to Shapiro's provocative exercise in historical marginality.

For Saul Friedman, who teaches ancient and Near Eastern history at Youngstown State University, there are also no heroes. Coming after studies by Arthur Morse, David Wyman, and particularly Henry Feingold, No Haven for the Oppressed is a soul-searching and dramtic if fruitless effort to identify American Jewish culpability for the Holocaust. Like his "anomic" fellow Jews, Brandeis's stentorian successor and the nation's most well-known rabbi is portrayed as a quietly desperate victim of the isolationist tornado of the 1930s, of his friendship for FDR, of the deep-seated parochial divisions among American Jews, and of general indifference in an era of catastrophe to the tragic fate of his co-religionists in Europe. In September 1942, upon learning of the impending Holocaust, Stephen S. Wise privately despaired to his longtime friend, John Haynes Holmes, but publicly remained silent. "Think of what it means to hear, as I have heard, through a coded message—that Hitler plans the extermination at one time of the whole Jewish population of Europe, and prussic acid is mentioned as the medium" (p. 142).

These two books are valuable because they focus on the breakdown of a larger Jewish and of course Christian consciousness in the face of ultranationalism gone mad. Their relentless moral probing of the liberal assumptions that proved powerless to prevent the destruction of two-fifths of the world's Jews, however, leaves too much margin for innocence even as their episodic methods limit their value as fully realized history.

MOSES RISCHIN
San Francisco State University

ROBERT B. CARSON. Main Line to Oblivion: The Disintegration of New York Railroads in the Twentieth Century. (National University Publications, Series in American Studies.) Port Washington, N.Y.: Kennikat Press. 1971. Pp. ix, 273. \$12.95.

This is a needed, informative, and thought-provoking (if not always convincing) book. It is needed because it fixes the spotlight on the railways' modern problems of excess capacity and competition. Although these are by no means the only problems (one might mention overregulation, discriminatory taxation, outmoded working rules, and the like), they are certainly at the center of the stage. Dr. Carson diligently traces the onset and growth of these twin problems in the state of New York. But he refers frequently enough to out-of-state and federal developments to place his microcosmic study in its proper setting.

This book is not only informative but, in the best sense, encyclopedic. There is nothing like it as a brief account of the big roads. Whereas it does not pretend to be all-inclusive, it provides not only facts but interpretations that force the reader to think. For example, are today's problems largely of the industry's own making as Dr. Carson says they are? After the Civil War were roads built "without much regard for the routes" or for costs? (p. 20). Was regulation invoked primarily to control competition and (contrary to what Sharfman says) largely by the roads themselves? Is it true that prior to 1920 railroads "rarely" used sinking funds, refunding programs, or depreciation allowances? (p. 58). Did the ICC subordinate itself to the policies of the railroads in the 1030s and thereafter? Has "no large American industry ever had the special access to the public treasury that the railroads . . . enjoyed?" (p. 113). Were the railroads, as late as 1945, "virtually a protected public monopoly?" (p. 130). Is it true that railways, in abandoning hopelessly money-losing lines, "never calculated the community or social costs they created?" (p. 220).

I would hardly answer these questions as does Dr. Carson; there is a great deal to be said on both sides of most of them. Yet the author has done a great service in raising the issues, for they do go to the heart of the present confused and serious situation. Despite his evident distaste for the balance-sheet policy making of the roads, he strives mightily at least to recognize both sides of any given question. This is all to his credit.

In the volume's very first sentence the author

asserts that rail history has been written "almost to the complete exclusion of twentieth-century developments" (p. 3). Not so. At least a third of the printed works now available are wholly or in part concerned with the twentieth century. Had Dr. Carson consulted some of the many key works omitted in his bibliography (e.g., Sharfman's The American Railroad Problem), he might well have avoided some of the oversimplified generalizations in the text. On the other hand, it is again to his credit that he has avoided counterfactual nonsense and meaningless models; neither would have been appropriate in a work of this sort. This is, rather, an inductive, "tell-it-like-it-is" account. It will, I think, evoke a few painful catcalls along with some well-deserved plaudits, but, in any event, we will all be the wiser for it.

RICHARD C. OVERTON
Burr & Burton Seminary

ARNOLD H. TAYLOR. American Diplomacy and the Narcotics Traffic, 1900–1939: A Study in International Humanitarian Reform. Durham, N.C.: Duke University Press. 1969. Pp. viii, 370. \$11.00.

While concentrating upon the first four decades of the twentieth century Taylor traces American interest in the international control of narcotics from the Opium War of 1839-42 to the late 1960s. Missionaries and reformers urged the American government to initiate diplomatic moves to end the opium trade with China, and the government was often responsive to their wishes. The United States took an active role in the Shanghai Opium Commission of 1908, and an American diplomatic initiative resulted in the Hague Opium Conference of 1911 and 1912, which concluded a far-reaching convention. After the First World War the United States and other Western powers came to realize that opium was not the only narcotic in need of international regulation; cocaine and Indian hemp were becoming problems. And the Western powers came to realize that the Far East was not the only part of the world in which narcotics were claiming their addicts. The United States and several European countries also had problems with drug addiction and the importation of narcotics. During the 1920s and 1930s the United States cooperated closely with the League of Nations to extend international controls.

This book is the result of intensive research in State Department documents and other records. It offers no unusual thesis or interpretations, but there are provocative discussions of connections between economic problems in narcotics-producing regions and the diplomacy of narcotics control, and the accounts of American cooperation with the League make a considerable contribution to an understanding of relations between the United States and the League. This book merits the attention of students of American diplomacy, and it should find a place in the library of everyone engaged in present-day efforts to control addictive drugs.

CALVIN D. DAVIS

Duke University

ZOSA SZAJKOWSKI. Jews, Wars, and Communism. Volume 1, The Attitude of American Jews to World War I, the Russian Revolutions of 1917, and Communism (1914–1945). New York: Ktav Publishing House. 1972. Pp. xxvii, 714. \$20.00.

What we have here is the publication, in book form, of Zosa Szajkowski's research notes. These notes cover aspects of Jewish history between 1914 and 1945. Most of his topics (but not all of them) deal with American Jewish opinion; most of them (but not all) with that opinion as it touches American-Russian relations. He examines radical opinion most thoroughly, but is by no means confined to that alone. There are thirty-four chapters, ranging in length from over 30 pages to under 3 pages; and it is exceedingly difficult to discover any cogent relationship between such topics as "European Jewish Attitudes to the War" and "Jews and the New York City Mayoralty Election of 1917," between "Greenwich Village and the Lower East Side Radicals" and "Soviet Persecution of Zionism."

As with any collection of research notes, much depends on how widely the researcher has looked and how interesting and important were the things he chose to gather. In both of these matters Szajkowski performs admirably. He has examined prodigious quantities of primary sources-dozens of personal collections, the Yiddish press and sizable portions of non-Jewish newspapers and periodicals, government documents, and the papers of many Jewish and non-Jewish organizations. One cannot accuse him of any lack of diligence. Moreover, he has a wonderful eye for the dramatic, the poignant, the startling. The book is a mine of often quite remarkable material and readers will find hours of fascination in its pages.

Nevertheless, it has all the disadvantages of an undigested collection of notes. Almost half of the book (including the 150 pages of closely printed notes) consists of long indented quotations. There is little exercise of discretion and virtually no judgment about what is important and what is not: why the chapter on Judah Magnes's views deserves 30 pages while chapters on "The Versailles Treaty" and "World War II" are each dispatched in 6 pages is never explained; why the problem of Russian reemigration after 1917 deserves extensive treatment while there is no systematic consideration whatsoever of, say, the Balfour Declaration or the Russian Civil War is also unclear. One is left with the feeling that the author devoted a lot of space to things for which he had a lot of notes and a little space to things for which he had only a few. Thus, although the subtitle indicates that the study will cover three decades, over three-quarters of the text deals with the first five-year period.

Szajkowski's own thesis statement is unsatisfying. "My sole purpose," he writes, "is to prove that there was never a monolithic attitude of American Jews to radicalism" (pp. xx-xxi). While this formulation seems to legitimize the indiscriminate introduction of random contemporary opinions, Szajkowski is obviously doing battle against a proposition—that all Jews were radicals—which few scholars can seriously espouse. It is hard to imagine even a superficial student of the problem who could believe that Jacob Schiff and Emma Goldman, that Morris Hillquit and Bernard Baruch, that Sam Gompers and Lillian Wald ever had monolithic opinions about anything.

In short, while this book is the product of extensive research and is filled with deeply interesting material, it is seriously (if not fatally) marred by its lack of proportion, discretion, judgment or unity of purpose.

DAVID W. LEVY
University of Oklahoma

JOSEPH R. STAROBIN. American Communism in Crisis, 1943–1957. (Prepared under the auspices of the Research Institute on Communist Affairs, Columbia University.) Cambridge, Mass.: Harvard University Press. 1972. Pp. xvii, 331. \$12.95.

Although he participated in the events that he analyzes, Joseph Starobin's book is not an autobiography. Scholarly and well documented it rarely mentions his own role as foreign editor of the *Daily Worker* and one of the party's experts on youth. Yet it is full of the kind of insight that only direct involvement in the movement can provide.

In Marxist terms Starobin's analysis of the

party's last years is essentially revisionist. He is highly critical of William Z. Foster and the left-wing militant line adopted by the party after the overthrow of Earl Browder in 1945. ("Foster's doomsday drive," he calls it.) The party was on the verge of a breakthrough in 1945, says Starobin, with the potential of becoming an important force in American politics, but only if it followed Browder's path and turned itself into a form of "left-wing lobby." Browder, for all his mistakes, was trying to do what was necessary for communism in America: to reshape it to meet the American situation, to cut it adrift from its propensity to import its analyses, ideals, and programs from abroad.

Whereas others see the party as slavishly following the orders of Moscow and the CPSU, Starobin paints a less conspiratorial, though perhaps more depressing picture: the American Communists were "neither allies nor agents" of the Soviet Union. Indeed, for the most part, the Russians neither knew nor cared much about what they did. Direct contact with Moscow was minimal. But the party's leaders, the generation that rose in the 1920s, imprisoned it in a kind of "mental Comintern," following what they thought were the wishes of Moscow, unable to recognize that, in practice, polycentrism was emerging in the Communist world even while Stalin lived.

This was exemplified best, perhaps, with the controversial Duclos article. Here Starobin tends to reinforce his colleague at York University in Toronto, Gabriel Kolko, and the New Left interpretation of the cold war. Clearly, says Starobin, the article reflected the thinking of someone in power in Moscow, but it was based on a grave misreading of Browder's line. The article, in turn, was grossly misinterpreted by the CPUSA. Its leaders concluded, wrongly, that it called for a toughening of the line and the beginning of a cold war. The CPUSA, therefore, was virtually alone in shifting toward a more militant line and soon found itself out of step with the other Communist parties, including Duclos's.

Ultimately, says Starobin, Foster's militant line led to where it was doomed to lead: to a form of suicide, culminating in the self-destructive fiasco of the Wallace campaign of 1948.

Starobin is very persuasive, but there are aspects of his book that are open to dispute. For instance, his tendency to heap most of the blame on Foster & Co. leads him to underplay the very strong forces outside the CPUSA working toward the destruction of communism, socialism, Browderism, or any left-wing move-

ment at that time. But this is a measure of how provocative and full of ideas it is. It is also refreshing to come across a book on this subject that manages to overcome its sources, shunning what Starobin calls "the peculiar Esperanto of the Comintern," indeed translating it with grace, style, clarity, and a great deal of insight.

HARVEY LEVENSTEIN

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ERNEST R. MAY. "Lessons" of the Past: The Use and Misuse of History in American Foreign Policy. New York: Oxford University Press. 1973. Pp. xiv, 220. \$6.95.

THOMAS M. FRANCK and EDWARD WEISBAND. Word Politics: Verbal Strategy among the Superpowers. (New York University, Center for International Studies. Studies in Peaceful Change.) New York: Oxford University Press. 1971. Pp. xiii, 176. \$5.95.

Two important trends in recent analyses of postwar American foreign policy-a deep and even personal concern about the direction it is heading, and the use of insights derived from social psychology—inform both books reviewed here, albeit differently. Ernest R. May's "Lessons" of the Past focuses upon the role of history and historians in the formulation of this policy. Since policy makers inevitably use analogies from the past in formulating or justifying their own actions but often misperceive these past events or ignore those most relevant to their immediate needs, he argues, professional historians, by their training, knowledge, and insights, are uniquely situated to contribute substantially.

May shows how a preoccupation with events in the recent past, and beliefs about how apparently similar crises in this recent past should have been handled, shaped the policies made by American leaders: wartime planning for the aftermath of World War II, changing attitudes in the late 1940s toward the Soviet Union, the Korean intervention, and the Vietnamese War. He does not deny that other factors played a role, including traditionally anti-Soviet views of State Department experts, domestic political pressures, and even Soviet behavior. Nor does he argue that outcomes would have been substantially better or even different from those that did result had top-level officials sought more relevant analogies in history. The point is rather that selective perception of the past and its relationship to present policy needs prevented policy makers from making the best use of historical knowledge. May then shows how its better use might have improved the quality of decisions made about bombing North Vietnam

May is on less sure footing, however, in searching for concrete ways in which professional historians can contribute to more effective policy making. They can deliver better, more pointedly relevant information. Along with experts in other disciplines, historians, because their professional training requires them to search for critical variables accounting for change and stability in the past, can make forecasts about future developments. Meanwhile, the government can assist in making historians more helpful by providing funds to expand specialized training, arranging for tours of duty by historians in policy agencies, and, above all, adopting a more open information policy.

More problematic are psychological and structural barriers in the policy-making process. It is not clear how the availability of even the most reliable knowledge and competent advice could overcome a policy maker's tendency to rely upon what is in his personal experience; to perceive the past, the present, and policy alternatives in highly selective terms; to search out those historians as policy advisers who are most likely to produce results congenial with his own preconceptions; to create high-level policy conferences in which mutually shared predispositions are reinforced rather than questioned; to resist potential damage to personnel or policies that might result from historians rummaging prematurely among hitherto closed files. Such constraints sharply limit the contribution historians can make in planning policy.

An underlying thesis of World Politics, by Thomas M. Franck and Edward Weisband, is that, quoting President Nixon, "the rhetoric in international affairs does make a difference." Expanding upon a theme frequently heard after August 1968, they argue that the rhetoric and behavior of the United States vis-à-vis Guatemala in 1954, Cuba in 1962, and especially the Dominican Republic in 1965 provided a precedent that the Soviet Union could and did adapt to its own purposes to end the "Czechoslovak spring."

The volume, although marred by excessive righteous indignation, cute inaccuracies (the world as a "two-ghetto system"), and redundancy, underlines an important point: the Johnson and Brezhnev doctrines used to rationalize these interventions merely verbalized a reciprocity that had long existed. Except in the

Far East, both superpowers had delineated tacitly and openly the areas in which they would brook no interference from the other. Similarly, both refrained from direct interference in the other's delimited area.

This stabilizing reciprocity nonetheless posed dangers. For one thing, the incongruence between behavior and rhetoric-both sides acted to maintain their tacit "bargain," but each decried as immoral and contrary to international law the actions of the other-ran the risk of inciting to rash action some foreign statesmen who took both at face value, or leading American and Soviet policy makers to forget the tacit agreement underlying the incongruence. More seriously, the stability toward which the superpowers pushed was stultifying for their satellites. The authors thus see the Nixon doctrine and initiatives as a step in the right direction toward loosening existing bipolarity. An even broader strategy, akin to Charles E. Osgood's GRIT (Graduated Reciprocal Initiatives in Tension-reduction), can synchronize behavior and rhetoric to ease tension, to build a "normative, reciprocally applied system of superpower interaction and world order."

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HADLEY ARKES. Bureaucracy, the Marshall Plan, and the National Interest. Princeton: Princeton University Press. 1972. Pp. xiv, 395. \$12.50.

The author's stated aim in this volume is to use the Marshall Plan as a case study of the way in which the national interest comes to be defined in the design of a major foreign policy program. His approach is to focus on the way in which problems of administration were handled initially when the program was being designed in the executive branch and in Congress and finally by the way that the Economic Cooperation Administration handled them in administering the Marshall Plan. The principal thesis of roughly the first half of the book, which deals with the initiation and enactment of the program, is that the way in which substantive policy questions were encountered and resolved was in the debate of issues that first presented themselves as administrative in character. The elaboration of this thesis culminates in a list of policy "themes" and a collection of "operating rules" in the form of presumptions intended to guide the ECA in carrying out the plan, all of which are demonstrated to have been explicit or implicit in the legislation, or in other documentation, and to have been evolved, for the most part, in the consideration of administrative matters. The thesis is well sustained and its development provides, at least to me, original insights into the process of policy definition.

In the balance of the volume the author addresses himself to the question of whether the policies, the operating rules, and the priorities among them, the development of which he has traced, were actually applied in practice by the ECA. Broadly speaking (if I read him correctly) his answer comes out in the affirmative. Although there is little to question in the account of the ECA's operations this part of the book is less satisfactory, at least to one who was a participant in the program. Perhaps the difficulty is that because the discussion is organized around issues of administrative procedure and the analysis of the efforts made to achieve substantive economic objectives is fragmented, matters of secondary interest receive too much emphasis and the treatment of those that actually occupied the energies of the agency is inadequate.

For instance, the author is perceptive in explaining that the ECA was neither able nor eager to influence the behavior of European governments or private businesses at the level of individual projects or to control the commodity composition of their imports. That the recipient countries had sufficient foreign exchange resources of their own and sufficient domestic capital other than counterpart funds to preclude U.S. intervention at this level was, however, clearly understood as a fact of life from the beginning. It did not seem to be particularly significant as a deliberately adopted policy.

On the other hand, in the discussion of U.S. pressure for economic integration in Europe, for restraints on consumption required to permit a high rate of investment, and for increased productivity, the author touches on objectives that were of vital interest to the agency. Greatly underemphasized, however, was an objective related to, but in some ways more pressing than any of these; namely, the suppression of inflation. The achievement of these objectives was understood by all concerned to depend upon the adoption of appropriate monetary, fiscal, and trade policies. Appropriately, therefore, the concerns of both Americans and Europeans were with these broader areas of economic policy. The author's treatment may understate the degree to which U.S. influence was brought to bear in them.

An author is entitled to define his own subject matter. This author chooses in the second part of his volume to review the application of virtually all the policies and operating rules, the origin of which he discusses in the first half. As it seemed at the time, however, and still seems in retrospect, many of these had little relevance to the task at hand, except for their nuisance value, and the attention devoted to them in this book seems to be at the expense of a more coherent analysis of the way in which and degree to which the really important policies were implemented.

RICHARD M. BISSELL, JR. Farmington, Connecticut

DAVID EDWIN HARRELL JR. White Sects and Black Men in the Recent South. Foreword by EDWIN S. GAUSTAD. Nashville: Vanderbilt University Press. 1971. Pp. xix, 161. \$6.50.

This book is a study of the evolution since World War II of the racial views of those Protestant sects whose greatest appeal is to whites from the lower socioeconomic strata in the South. It is an insider's view in that the historian includes himself among educated young sectarian leaders who have "self-consciously searched their past to try to explain their church's record of racial prejudice and injustice and try to find precedent for liberal social action" (p. 118). Emphasizing sociology rather than theology as the basic determinant of the racial views of the sects, he explains the trend toward moderation in terms of the social ascent and striving toward respectability of significant numbers of members of the larger sects. At the other pole he explains the trend toward racial integration of the smallest, weakest sects in terms of lack of resources to maintain segregated facilities and institutions.

While interesting data is assembled to support these conclusions, the book suffers from overreliance upon publications of the various sects and public pronouncements by their leaders. More varied sources, particularly oral history interviews with both leaders and members, could reach a deeper level of truth. Moreover, the internal evolution within the sects is treated as if it were only slightly related to happenings outside: a defect that seriously weakens the analysis and undermines the long-range value of this work.

GWENDOLYN MIDLO HALL Livingston College, Rutgers University HERBERT S. PARMET. Eisenhower and the American Crusades. New York: Macmillan Company. 1972. Pp. xi, 660. \$12.95.

In the final sentences of this perceptive book the author writes the best short summary of his subject: "To label him a great or good or even a weak President misses the point. He was merely necessary" (p. 578). The preceding pages of the detailed narrative prove his point.

The opening of many personal and official papers of the Eisenhower presidency permits Professor Parmet to go well beyond earlier accounts based on contemporary newspapers and periodicals and the earlier memoirs by participants in the events of the period. The author has drawn heavily on his interviews conducted in the 1969-71 period with twenty-five associates of the general's and on larger collections of interviews at Columbia and Princeton relating to Eisenhower and to Secretary of State John Foster Dulles.

The legend that "Ike, the simple soldier," merely supplied a smiling façade for administrations dominated by leaders such as Sherman Adams and Secretary Dulles is effectively denied. Long before Dulles's illness made him ineffective in foreign affairs the president made his influence felt in the formation of American policy. A world leader who had talked with presidents, prime ministers, and kings and played a major role in international affairs at SHAEF and SHAPE did not have to defer in such matters to his secretary of state.

The general's political astuteness (obviously marred in Parmet's eyes by occasional slowness in taking a firm stand) is seen in his dealings with Senator Joseph McCarthy, a darling of the right wing of Eisenhower's party. Although at times, as in the case of General Marshall, the president saddened his friends by delaying his rebuke to the Wisconsin senator, he ended by rallying the forces that destroyed the power of that politician.

From the moment he committed himself to the preconvention fight in 1952 until the end of his presidency, Eisenhower faced opposition to part of his program by conservative leaders of his party. The general's sweeping personal victories were interpreted as public mandates for a conservative program and the failure of Republicans to control Congress after the 1956 landslide was blamed on Eisenhower's acceptance of some Democratic measures. In both terms he had to turn to Democratic leaders such as Lyndon Johnson and Sam Rayburn to

salvage some of his principal proposals from attacks by members of his own party.

With an eye on recent politics the author underlines the admixture of coolness and correctness that marked Eisenhower's enthusiasm for his vice president. But he gives Mr. Nixon high marks for his effective efforts in reducing some of the right-wing onslaughts on parts of the president's program.

The Republicans, Parmet suggests, had been too long out of power. The habit of opposing the White House threatened to be carried over into a Republican administration. A mixture of accommodation and firmness—administered by an astute leader—was called for. Franklin Roosevelt had won praise for his ability to push forward liberal measures without totally alienating some of the conservatives of his party. Parmet reminds us that Eisenhower in his own way could also combine the qualities of the Lion and the Fox.

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JONATHAN TRUMBULL HOWE. Multicrises: Sea Power and Global Politics in the Missile Age. Cambridge, Mass.: MIT Press. 1971. Pp. xii, 412. \$12.50.

In his study Multicrises, Commander Howe deals with the ability of the United States Navy during the nuclear age to provide meaningful deterrence in one area of the world while it is actively involved in another. His discussion focuses on the Middle East crisis of 1967, when the Sixth Fleet was positioned to deter Soviet intervention against Israel while the navy was also fighting in Vietnam, and on the Quemoy crisis of 1958, in which the Seventh Fleet demonstrated to discourage the Chinese Communists from occupying Quemoy shortly after the Americans had landed in Lebanon. Although American naval deterrence was by no means as overwhelming in 1967 as it had been in 1958, Howe concludes that it was effective in both instances and that in neither case were the navy's capabilities seriously compromised by its commitments elsewhere. In 1967, as in 1958, the British naval presence outside Europe was an important stabilizing factor, according to Howe. Soviet naval power markedly increased during the decade between the two crises without, however, challenging the ultimate capacity of the United States Navy to provide effective deterrence. In both crises the United States was associated with partners in the areas, Israel and Nationalist China, who demonstrated conspicuous ability to care for themselves and who made commitments that the United States was unable or unwilling to prevent.

Howe warns that the navy's deterrent capacity in the third world will be increasingly limited hereafter by expanding Soviet naval power, by the growing obsolescence of American warships, and by the British withdrawal to European waters. Nevertheless, he concludes that the United States must maintain "viable . . . strength throughout the world" to sustain "existing commitments" and to prevent "policy alternatives in future multicrisis situations" from being "significantly restricted" (p. 346).

Howe's study is really more a series of estimates and statements of belief than a book of history. For American naval thinking, the commander draws from revealing interviews with persons who were close to or actual participants in the events themselves as well as on his own perceptions as a naval officer. He is unable, however, to cite the basic records essential to a full and accurate understanding of the American naval position, and his estimates of the responses by the friends and foes of American deterrence are necessarily grounded on extremely flimsy evidence. Often tediously redundant his text is burdened with the language of the experts and endowed with numerous charts of capabilities, conveying an impression of scientific precision surely far beyond what Howe intends. None will deny, however, that Howe's book is a completely genuine expression of American naval opinion that may prove very helpful to historians once they are able really to study the role of the navy in the third world.

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HARRY EDWARD GRAHAM. The Paper Rebellion: Development and Upheaval in Pulp and Paper Unionism. Iowa City: University of Iowa Press. 1970. Pp. xv, 170. \$6.50.

The rebellion that is the focus of this book involved West Coast locals of the United Papermakers and Paperworkers and the Pulp, Sulphite and Paper Mill Workers. In all, just over 20,000 workers were involved in this 1964 separation from the established international unions in the paper industry. It does not make a very exciting tale, and the essentials could have easily been stated in an article. Only part of the book is devoted to a rather uninspired summary of the early history of the unions in

the industry. Graham's work is, however, not really a history but rather an analysis of what he perceives as a test of trade-union structure.

This kind of schism represents a rather unfamiliar form of dissent in American trade unions as compared to so-called rank and file movements or dual unionism. In this case the author convincingly demonstrates that schism was a result of a matured union bureaucracy that proved too inflexible to yield to demands for greater local autonomy and internal democratization. Furthermore, the central union leaders had enjoyed a cozy relationship with the West Coast paper companies since the middle of the 1930s and were not responsive to local demands in the 1960s for a more militant posture.

A kind of progressivism within the trade union is the solution Graham calls for, with local autonomy, recall elections, and even a form of the referendum. While such changes are undoubtedly a step forward the author does not question whether reforms that proved relatively ineffective in democratizing political institutions can be any more effective within the trade unions.

Research for this book was minimally sufficient, but certainly interviews might have added further dimensions to the study. One closes this book with the sound of scissors and the smell of paste most evident.

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### **CANADA**

IRVING M. ABELLA et al. The Influence of the United States on Canadian Development: Eleven Case Studies. Edited by RICHARD A. PRESTON. (The Committee on Commonwealth Studies of Duke University, publication number 40.) Durham, N.C.: Duke University Press for the Duke University Commonwealth-Studies Center. 1972. Pp. xii, 269. \$7.75.

This book is one of the first fruits of the Association for Canadian Studies in the United States, a collection of papers presented at the association's inaugural conference in 1971. The editor, Richard A. Preston, points out in his introduction that "development" is used in the title in its general and traditional sense to mean "the process of overall change that has produced the community known as Canada." The selection of papers demonstrates the breadth of this definition. The subjects range from Preston's and Carl Berger's stimulating essays on aspects of the intellectual history of

the Canadian-American relationship to studies of American influences on Canadian politics, the recent growth of sociology and political science in Canada, the Auto Pact, Maritime underdevelopment, the Canadian trade-union movement, and to such narrowly technical topics as American participation in early irrigation schemes in central British Columbia and the degeneration of the French language in Windsor, Ontario.

This diversity of theme and approach is the book's weakness, but it is a forgivable one. The object of the conference and the collection was to promote studies of Canada in the United States that move beyond Canadian-American relations into the more subtle realms of domestic Canadian life. The theme of American influences on Canadian development is a neglected and fruitful one for examination, and so pervasive that almost every corner of Canadian history invites study from that perspective. The temptation to display the widest range of examples was understandable, but it results. in a loss of force in the collection. Having made this demonstration the association might usefully organize its succeeding conferences around more tightly defined themes.

Despite its lack of focus the book does assist in opening up a number of paths of central inquiry. Richard Preston argues for intensive study of "the assimilating effect of American influence on Canadian life" as one important means of judging the prospects for an independent Canadian existence; Carl Berger notes the effects of cultural relativity on the Carnegie studies of the late 1930s and 1940s in Canadian-American relations (and by implication warns of similar pitfalls in scholarship undertaken in the very different atmosphere of the 1970s). What scholar—at least, what Canadian scholar -would be inspired today by the faith of J. T. Shotwell? "The role of history and the social sciences was to reveal those processes of civilization which made all peoples one, and to destroy the outdated conceptions of national sovereignty which were the causes of war. For Shotwell, the history of Canadian and American relations became a testament to the internationalist faith-that economic interdependence and the unrestricted flow of the forces of liberal capitalism, the interchange of populations, the embedding of parts of one nation within another, and the growth of a rational and nonideological approach to problems, were the sources of peace. This was the great theme which was revealed in the history of Canadian

and American relations and which conveyed a lesson to the whole world."

Robert Babcock's and Irving Abella's papers on the Canadian trade-union movement continue the valuable work of debunking certain Canadian assumptions about the sources of international unionism in Canada. If there is a common theme to be divined from the papers it is most explicit in these two: the suggestion that Canadian deference to American values, institutions, and leadership has its source as much in Canadian choice as in American assertiveness. That hypothesis requires critical study from both the Canadian and American perspectives. Its elaboration could be one of the most useful products of the dialectic introduced into Canadian studies by the current growth of the field in the United States.

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## LATIN AMERICA

BURR CARTWRIGHT BRUNDAGE. A Rain of Darts: The Mexica Aztecs. (The Texas Pan American Series.) Austin: University of Texas Press. 1972. Pp. xvii, 354. \$10.00.

The thorough but often insufficiently critical examination of sources—particularly sixteenth- and seventeenth-century Spanish chronicles—attributed to Burr Brundage by readers of his *Empire of the Inca* (1963) and *Lords of Cuzco* (1967) is evident in *A Rain of Darts*. Brundage describes his new work as a "political history" of the Mexica Aztecs, a story in which the principle themes are "the keen realization by the Mexica of the illegitimacy of their claim to the land" and "their expectation of the proprietorial return of the god." Dust-jacket commentary acclaims the work as serious, scholarly, and the first one-volume narrative history of the Mexica.

A Rain of Darts is a fascinating, amazingly synthetic chronicle of the legendary experiences, political and other, of the peoples of greater Anahuac. Had it been published in the days of Prescott or of Bancroft it might well have passed muster as political history; in the days of Torquemada and Mendieta it would have overshadowed all. It was written with serious and scholarly intent and is our first relatively complete, one-volume narrative treatment of the Mexica from the years of their association with the Toltecs of Tula to that humiliating summer day on which they submitted to conquest by Fernando Cortés and his fellows.

For Brundage the story of the Mexica, "whatever else it may be, is a tale of midnight murders, intrigues and wild revenges." It has "a lurid quality not often met in the chronicles of nations." As he renders it, one might well agree. His Mexica were a people "adrift in a great tempest of their own making," the "eminently quiet slaves" of a psychophilosophical orientation that was provided, at least in part, by economic and geopolitical circumstance, and the vassals of their teuctli, warrior lords, barons, knights, and sometimes priests whose "voracious hunger for battle, boastfulness, monstrous dignity and unending search for honors" formed the dynamic in their history.

For the general reader who is willing to absorb a welter of generally unfamiliar names, places, events, and concepts, A Rain of Darts will be an exciting and fulfilling tale. The specialist will admire the command of chronicle literature it evidences and may be stimulated by the vivid interpretive treatment provided individuals (such as the well-known Moctezuma I and Moctezuma II and the lesser-known Tlacaelel and Moquiuix), institutions, and concepts. The specialist will be distressed, however, as he attempts to grasp the intended definition of the nature and structure of the so-called Aztecan state. He will find that which is proposed rather simplistic and too often defined in terms applied to medieval European models, that is, the teuctli as a baron, and so forth. Finally, he will find that the sources cited often do not provide sufficiently for the conclusions reached; for example, Moctezuma II had an "abnormal fear of death," "knew his vassals hated him," and "believed the Macehualtin" were "vicious and lazy."

In sum, read A Rain of Darts and advise your students to do likewise, but read it as a fascinating tale, a stimulating adventure and departure point for further study and not a definitive history.

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SHIRLEY BRICE HEATH. Telling Tongues: Language Policy in Mexico, Colony to Nation. (Publications of the Center for Education in Latin America, Institute of International Studies, Teachers College, Columbia University.) New York: Teachers College Press, Teachers College, Columbia University. 1972. Pp. xx, 300. \$10.00.

This is a rather extensive case study of a dia-

chronic character that depicts in well-documented terms the "official" language policy of New Spain and the Mexico that succeeds it. The shift in this policy vis-à-vis the actual developments in language learning and perpetuation reveal the importance of the language factor in the complex sociopolitical structure of a nation.

In the course of her description of policy from the sixteenth-century Franciscan missionaries to the prevailing precepts of the Revolu-tion, Professor Heath "takes sides" in the sense of supporting in retrospect any indicated effort of the Indians to perpetuate their own language or to create of Nahuatl a lingua franca. She would seem to deprecate any move toward uniformity as a reactionary gesture of the elite. One may have the feeling that in supporting this position the author is at times somewhat subjective. The role of official bodies or academies is sometimes exaggerated, in my opinion, and Heath's analysis of the factors of acculturation or her acceptance of the statements of others regarding Hispanic acculturation seem to indicate at times a sympathy with normative precepts and prescriptive procedures that is not too common among anthropologists or linguists.

Thus it is that her well-researched document on attitudes toward language development and on bilingual situations throughout the history of Mexico is summarized in chapter 9, "Overview and Conclusions," with a treatise that in spots seems an advocacy of policy. As one who has spent much time in Oaxaca, I was struck by the quoted assertion that speakers of Oaxaca will use fifty Nahuatl words for every fifty Spanish, or that the Spanish of speakers in Mexico City exhibits very little influence from Nahuatl (p. 195). In the case of both cities the domestic lexicon has many Nahuatl terms, but they constitute a small fraction of the total vocabulary, and the unrelated Indian languages heard in the market at Oaxaca are Zapotec and Mixtec, neither of the Nahuatl family.

The author does not seem to separate in her consideration, as one might expect, matters of structure from lexicon or rules of spelling. Evidence now shows that the structure of American Spanish is part of a continuum of Andalusian Castilian that has changed very little in four hundred years, and the differences that are found now depend very little on substratum indigenous forces but rather on the factor of accessibility to Andalusian influences in the colonial period.

Heath's account of the important role of social scientists, many of them Americans, in

the shaping of language policy as a part of the sociopolitical outlook of the Revolution and of the reactions to this after Cárdenas is well written, but perhaps we shall find that structurally, at least, languages will continue to develop under the pressure of extralinguistic factors and that social coercion will always have a greater part to play than planning.

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CARMEN VENEGAS RAMÍREZ. Régimen hospitalario para indios en la Nueva España. México, D. F.: Instituto Nacional de Antropología e Historia, Departamento de Investigaciones Históricos. 1973. Pp. 223.

The purpose of this study is to explain the organization, development, and role of the various hospitals for Indians started by the Spaniards in New Spain. It is surprising that some 140 such institutions are listed in the appendix, and the author does not claim that the list is complete. Little is known today about the majority of these hospitals; it is likely that some of them were temporary institutions founded during an epidemic or famine, or that they served more as shelters for the aged or homeless than as treatment centers for the sick, Substantial information has been assembled, however, on several of the larger and more important Indian hospitals. These would include the famed Hospital Real de San José de los Naturales of Mexico City (founded about 1531), the several hospitals of Santa Fe started by Vasco de Quiroga in the sixteenth century, and the Indian hospitals of the province of Michoacán. Carmen Venegas Ramírez makes it clear that as a general rule the Indian hospitals served not only as charitable and medical centers, but as vital auxiliaries in the tasks of converting Indians to Christianity and of congregating them in urban settlements where they could better be put to work and taxed.

This interesting study is a useful addition to the literature on the medical, social, and religious history of colonial Mexico. The text is brief (134 pages), but the book is well organized, carefully documented, and based in part on original manuscripts. The appendixes are lengthy (80 pages) and valuable, and they include new information on payment of the medio real as well as the composite list of 140 Indian hospitals. Twenty-four black and white plates offer a useful visual dimension to the student. The most thorough work on the hospitals of

New Spain is still Josefina Muriel's two-volume work Hospitales de la Nueva España (1956, 1960), but persons interested primarily in Indians and their hospitals will generally be pleased with Régimen hospitalario para indios en la Nueva España. It falls a little short, however, of being a definitive study. One would expect that additional valuable information on the subject could have been gleaned from the Ramo de epidemias and Bienes nacionales of the Archivo General de la Nación, neither of which are cited. Nor does one find included in the bibliography any of the writings of José Joaquín Izquierdo, the dean of Mexican medical historians, or of such noted scholars as Woodrow Borah, Sherburne Cook, or Alfred Crosby, Jr. All major Latin American libraries will want to acquire this book, as will any person interested in Mexican medical history.

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HORST PIETSCHMANN. Die Einführung des Intendantensystems in Neu-Spanien im Rahmen der allgemeinen Verwaltungsreform der spanischen Monarchie im 18. Jahrhundert. (Lateinamerikanische Forschungen, number 5.) Cologne: Böhlau Verlag. 1972. Pp. vi, 328. DM 62.

One of the more important Bourbon reforms in Spanish America was the institution of intendancies, which both centralized many administrative functions and inserted a middle layer of supervision between district and central colonial authorities. Recent studies of the reform have concentrated upon individual colonies, the Río de la Plata and Peru. Now Horst Pietschmann adds a study of the new system in the viceroyalty of New Spain. He is interested in the antecedents of the reform, especially whether the idea should be ascribed to French influence or lay more deeply rooted in Spanish precedent and need. He is further interested in implementation and the change it brought. His original study, a doctoral thesis, was fuller than this book since a substantial section dealing with other changes in administration in Mexico resulting from the general inspection of José de Gálvez has been published separately in the Jahrbuch für Geschichte von Staat, Wirtschaft und Gesellschaft Lateinamerikas (8 [1971]: 126-220).

Pietschmann's study is careful and methodical, based upon detailed reading and extensive research. His conclusions may be summarized rather simply: intendants in Spain and Spanish America represented a native development rather than implantation of the French model; the attributes of intendants under the two crowns differed markedly. The intention of the ordinance of 1786 in Mexico was to relieve the viceroy of much administration by erecting a fiscal system only nominally dependent on him and by conceding to the intendants a large measure of autonomy. In practice the viceroys succeeded in absorbing the fiscal administration into their attributes and converted the intendants into completely dependent officials intermediate between the districts and Mexico City. Centralization and gathering of information for the government were substantially advanced, but the intention of tidying up territorial divisions did not work out completely since many fiscal and ecclesiastical jurisdictions still did not coincide with the boundaries of the intendancies. The intendants were most effective in their capitals and the adjacent districts where they exercised the functions of district administrator as well. Pietschmann judges that the reform and the officials it brought in had only a small role in the remarkable economic flowering of Mexico during the last years of the colonial period. Yet the evidence for judgment is still scant. The insertion of an intermediate level of administration was an important development, one that has been continued in the present federal system.

> WOODROW BORAH University of California, Berkeley

WINFIELD J. BURGGRAAFF. The Venezuelan Armed Forces in Politics, 1935–1959. Columbia: University of Missouri Press. 1972. Pp. 241. \$10.00.

This book is a study of the Venezuelan military's involvement in politics during the present century, focusing primarily on the period from the death of Juan Vicente Gómez in 1935 to the presidency of Romulo Betancourt in 1959. It is offered not only as a contribution to Venezuelan history, of value in itself, but also as "an additional case study for the comparative analysis of military politics in this hemisphere." It may therefore be viewed from divergent perspectives, with divergent results.

Judged as a work of straightforward historical narrative and analysis, it merits generally high marks and should please most readers. The period covered in detail was one of painful political transitions, coups, and crises in which the military was closely involved. The armed forces participated in coups in 1945 and 1948, nulli-

fied an election in 1952, imposed outright military rule during the ten-year period from 1948 to 1958, overthrew its own regime in the latter year, and finally acquiesced (by a narrow margin) in a return to civilian administration in 1959. Burggraaff's account of these developments and of the reasons for the military's involvement in them is generally well developed, carefully researched, and clearly and thoughtfully written. Though minimal attention is paid to the effects of economic and social developments on men and events, this shortcoming weighs less than the book's positive merits.

Judged on the other hand as a contribution to the comparative analysis of military politics the book is less satisfactory. The problem derives in part from its narrative focus, in part from methodological shortcomings, in part from Burggraaff's failure to utilize any overall theoretical or conceptual analysis through which his work can be tied to existing theory on the Latin American military or to the question of military involvement in politics generally. For example, though he is apparently familiar with the literature in the field, Burggraaff does not test any of the existing generalizations on his subject or develop any "testable" generalizations of his own. Nor does he use quantification

techniques or seek to study "the military's overall role in society, their social origins, their technical capacity, or their internal institutional squabbles over positions and promotions." Thus his book is of limited utility to scholars seeking to interpret the military's political actions in terms of socioeconomic influences or through the institutional make-up of the armed forces. Finally, I refer again to the fact that the author deals in only a limited way with economic and social developments, offering little data on the country's underlying socioeconomic structure.

It is obvious that no one book can do everything, and Burggraaff should not be criticized for not answering all questions or exploring all lines of inquiry. These comments are made simply because he stresses the value of his book for purposes of general analysis and also because it is desirable that the potential reader have a clear idea of what is dealt with in his book and what is not. In sum, the book is a worthwhile contribution to Venezuelan political history, but it is of only moderate value for the comparative study of military politics in Latin America as a whole.

GLENN E. MILLER
Franklin and Marshall College

# Communications

A communication will be considered only if it relates to an article or review published in this journal; publication of such a communication or of any part of it is solely at the editors' discretion. Limitations of space require that a communication, whether concerning articles, review articles, or reviews, be no longer than 300 words. The schedule of publication and the time needed to send a communication to the author of the article or review in question for such reply as he may care to make virtually preclude the possibility of publication in the issue following that in which the original article or review appeared. Unless, in the editors' judgment, some major scholarly purpose is served, rejoinders will not be published.

### TO THE EDITOR:

Arnold Thackray's article growing out of hiscontinuing study of the membership of the Manchester Literary and Philosophical Society (AHR, 79 [1974]: 672-709) provides a good example of overgeneralization based on the legitimate, but in some ways limited, technique of prosopography. Thackray develops a curious argument: because science was the most important field of inquiry of the Society, it was the most important field of inquiry to most of the members; and because some members of the Manchester elite belonged to the Society, "by the early nineteenth century science was established as the cultural mode of the Manchester elite" (p. 682) and therefore "science became the predominant mode of cultural expression in Manchester" (p. 681). Although the logical sequence of the argument, which I think I have fairly presented, deserves not a moment's notice, I would like to discuss the validity of some of the individual statements.

Thackray simply does not present a good case to prove that most Society members regarded science as their "cultural mode." On

the contrary, he admits that the early scientific thrust came from a few members of the medical professions and later from a handful of outstanding scientists of whom Dalton remains best known. The Society's merchants, manufacturers, and bankers seem rather conspicuous for their indifference to science, except on occasion as a "polite, indeed ornamental" (p. 705) preoccupation. It is not enough merely to note that a man joined the Society or remained a member over an extended period to prove that science was his cultural mode because other things might have motivated such behavior. An interest in literature certainly motivated some of the men under discussion. The Society initially grew out of informal weekly meetings devoted to the discussion of literary and philosophical topics, concerns indicated in the Society's title. When the Society introduced separate sections in 1836-37, one was specifically reserved for literature, the other two being for chemistry and natural history. Besides promoting learning-mainly scientific but also literary—the Society served another extremely important function, that of conferring social prestige. Thus it must be proven in each case that an individual truly joined and remained a member in order to adopt science as his cultural mode and not in order to pursue literary studies or simply to gain prestige. Thackray does not provide this proof and, given the paucity of evidence on nearly all members except the leviathans of wealth, probably cannot furnish such proof.

Nor does Thackray fare better when he begins talking about the Manchester elite. He really fails to define this elite and all but ignores the older part of it, the magistrates, manorial and parochial officers, and Anglican clergy who remained numerous, wealthy, and powerful, though not unchallenged, down to the 1830s. Even assuming that Thackray is confining himself to one segment of the urban

elite, he should have made it clear that merchants, manufacturers, bankers, and medical men in the Manchester district numbered at least in the hundreds, and more likely in the thousands, at any given time during the period from 1781 to 1852. If Thackray believes that all of the men in these categories were part of the "new elite" then surely his sample is inadequate since it appears from table 1 that men from these categories were joining the Society at a rate of only about four per year after 1781. If on the other hand Thackray believes that only the wealthiest and most prominent men from these four occupational categories comprised the new elite, then he must carry his discussion well beyond the Mc-Connels, Kennedys, and Heywoods. He must proceed to demonstrate that there was considerable overlap, if not congruence, between the highest echelon of the new elite and those joining the Society. This he does not do either. Such methodological lapses ultimately make his conclusions about the Society's membership of doubtful relevance to the Mancunian elite, no matter which of the above definitions is used.

Thackray stretches his argument in another direction as well. He claims that social isolation, political impotence, and tumultuous surroundings, among other things, resulted in the necessity of finding social legitimation within the local context and that these factors comprised basic preconditions for the establishment of the Society at Manchester. Yet these preconditions were hardly unique to the period of the early Industrial Revolution. How did social legitimation occur in Manchester before 1781? And did this one Society with its small, restricted membership really accommodate the need for legitimation in a sprawling industrial region?

Part of the answer lies in the fact that the Lit & Phil was merely one social and cultural institution among many. There was theater from the 1750s, concerts from 1744, a Gentlemen's Concert Society of amateur musicians from about 1770, a circulation library in 1756, and prestigious subscription libraries in 1770 and 1792. Social clubs also flourished. These included one in the early eighteenth century for leading manufacturers; the Assembly Rooms, opened in 1752 and rebuilt in 1792, which were notorious haunts for young men desirous of dancing their way into the hearts and fortunes of wealthy and eligible ladies; John Shaw's Club dating from the 1770s, which numbered among its early presidents James Bateman, the famous builder of steam engines, and James Massey, first president of the Infirmary and also first joint-president of the Lit & Phil; the exclusive Billiard Club, established in 1795, which included representatives from the Peels, Gregs, Philipses, and Heywoods; and the Scramble Club, founded around the turn of the century and made up almost entirely of merchants and manufacturers. The Lit & Phil was by no means first nor necessarily preeminent as the sociocultural locus for industrializing Manchester.

There were additional scores of philanthropic, educational, religious, and political committees, clubs, and societies before 1800 in which, through energetic participation, conspicuous largesse, and judicious acquaintanceship with acknowledged social leaders, prosperous merchants, manufacturers, bankers, and others could hope to achieve social legitimation. Dabbling in science and joining the Lit & Phil comprised only two activities among many that might have helped achieve such legitimation. But while both activities bear mentioning, they hardly warrant the burden of emphasis Thackray has placed upon them.

ROBERT GLEN
University of California,
Berkeley

## PROFESSOR THACKRAY REPLIES:

I am saddened to see how such strange meanings may be read into my article on "Natural Knowledge in Cultural Context." To cope with all Mr. Glen's arguments would take more space than the issues deserve. It therefore seems best simply to refer your readers back to my presentation (AHR, 79 [1974]: 672-709) and let them judge for themselves the saliency of his concerns. By way of example, I shall take up only one constellation of objections.

I wrote of my intention to use "the versatile if sometimes barbarous art of prosopography" (p. 678). Mr. Glen deprecatingly points to my employment of the "legitimate, but in some ways limited techniques of prosopography": scarcely a great advance. He then goes on to argue that "it must be proven in each case that an individual truly joined and remained a member [of the Manchester Lit & Phil] in order to adopt science as his cultural mode," and to indict me because I do "not provide this proof" and "probably cannot furnish such proof." Now in my naïveté I had assumed it would strain the patience of readers, to say nothing of the good will of the éditor or the resources of the AHA, to pre-empt some hundred of pages in printing the information I have collected on each of the 588 individuals who constituted the group under study. I had therefore hoped that the provision of systematic statistics, strategic examples and occasional wry anecdotes might give artistic verisimilitude to what, alas, apparently remained a bald and unconvincing narrative.

More seriously, the thrust of Mr. Glen's objections reveal his failure to comprehend the purposes of prosopography or the capacities of cultural history. Certainly, not all the members of the Lit & Phil, let alone all the Manchester bourgeoisie, turned to natural knowledge in this period. Certainly, of those who did, some few were vastly more energetic and influential than the rest. Certainly, other sorts of cultural activity flourished in the town. But to allow the particularistic cataloging of a hundred such matters to obscure the recognition of what was novel and distinctive about those forms of urban-industrial culture which found their exemplar in Manchester is, perhaps, to be a trifle myopic.

ARNOLD THACKRAY
University of Pennsylvania

#### TO THE EDITOR:

David E. Stannard ("Death and Dying in Puritan New England," AHR, 78 [1973]: 1305-30) concludes that the Puritans' manner of dying was at odds with their vision of death, that they were "gripped individually and collectively by an intense and unremitting fear of death, while simultaneously clinging to the traditional Christian rhetoric of viewing death as a release and relief for the earth-bound soul" (p. 1315). This thesis demonstrates a faulty understanding of both the traditional Christian view of death and the Puritan experience of dying.

The Christian view of death is not static. Stannard's "traditional Christian rhetoric" is late medieval, and his discussion ignores the tension in Christian thought explored by Milton Gatch, Jaroslav Pelikan, Oscar Cullmann, and others, between the Greek idea of the immortality of the soul and the Hebraic idea of the resurrection of the body. According to Stannard's "Christian tradition" facile and mechanistic concepts of salvation and assurance allowed optimistic confidence in the face of death. Developing that view he states that the dying man, assisted by family and friends, could "with a little work" and "by resolutely clinging to his optimistic belief in his own goodness and the justness of God" overcome temptation and attain salvation (pp. 1308, 1322).

If all else failed, there was always the chance for a "last minute sacramental reprieve" (p. 1326). First, even when facile and mechanical answers were attempted to the questions of death and salvation, they produced not "Christian optimism" but a loss of confidence in the religious establishment and were a primary cause for the Reformation and its rapid spread throughout Europe. Second, the Ars Moriendi actually contradicts rather than supports Stannard's view. Death was not made easy; it remained a great evil. Men in dying, the treatise warned, have the greatest temptations. To meet them required not "a little work" by decedent and friends but the supernatural intervention of a crucified Christ and the saints. The prayers and precepts prescribed for the dying man and his attending friends appeal again and again to God's mercy, never to His justice or to the goodness of the dying person. The "last minute sacramental reprieve," while possible, required repentance, contrition, and faith.

Stannard views death as either reward or punishment while his sources, medieval and Puritan, treat it as both reward and punishment. New England Puritans did indeed find in death "an out-let from sin and misery, and an in-let to Glory" (p. 1312). They held it "as fixed," following their mentor John Calvin, "that no man has made much progress in the school of Christ who does not look forward with joy to the day of death and final resurrection" (Institutes, III, 9, 5). But concurrent with this optimistic death-as-reward view they also viewed death as the greatest natural evil, the greatest of afflictions, the result of sin, and not originally a part of creation. For these reasons, the Puritans believed that death in itself should be feared. Leonard Hoar wrote that death was "unstung" for Christians, but while the sting of death "cannot poyson them" it could "pierce and pain them" (Sting of Death, p. 12).

To find Puritans fearing dying and death is, then, no surprise. Stannard is mistaken, however, in finding this fear to be "unremitting." Elisabeth Kübler-Ross's five stages of dying—denial, anger, bargaining, depression, and acceptance—might not be the best model for early New England, but the idea of stages is imperative. Anxiety followed by assurance was a pattern in Puritan spirituality and their experience of dying, unless cut short, was no exception. While young John Tappin died anxiously and painfully—he was, after all, "not without some hope" (p. 1316)—the equally young and spiritually precocious Nathaniel Mather, after a time of doubting, died peace-

fully (Magnalia, IV, 10). Leonard Hoar desired that he might "dye of a consumptive and lingering distemper," not out of hopes for a deathbed repentance, but that he would have time to be fully and consciously prepared for his "great change" (Sting of Death, [p. v]). John Eliot, whose dying words were "Welcome joy!" and "Pray, pray, pray!" is an example of a saint who completed the stages of dying (compare to "Communications," AHR, 79 [1974]: 917-18). This path to assurance, however, was often cut short by pain and delirium, accompanied by Satan's temptations. The dying fears of Increase Mather, Stannard's primary example, can probably be attributed to this cause since, according to Cotton Mather, "the Dark Vapours which assaulted and fettered his Intellectual Powers, broke in upon him" (p. 1315).

What about the peace the rhetoric promised? Whatever troubles saints suffered before their end, James Fitch wrote in Peace the End of the Perfect and Upright (1672), if marks of righteousness appeared in their life, then there was no reason to doubt that the Lord would make their end to be peace, if not before the soul left the body, then at least when the angels carried the soul to heaven. Ann Mason's death was Fitch's example of the peace God usually gave to saints. In health she was often "full of spiritual exercise and darksome objections," but in sickness "she had some taste of unspeakable peace and comfort, which in the midst of such pains so disturbing and confounding to the outward and inward senses, yet made her say, she should be at home in a little time, being ready to depart to her Fathers [sic] house, and she who had the spirit of adoption could not but under the pains of death, call Abba Father for help" (p. 12).

Stannard's use of sources is questionable in other areas. Many of his "consistant patterns" of godly dying come from Cotton Mather's The Thoughts of a Dying Man (1697), a tract expressly designed to awaken men to the fearful possibilities of death for those not fully prepared. It is not a compendium of saintly deaths. The "stout man," for example, whose heart was overwhelmed with dread of death and God's wrath (p. 1317), is identified elsewhere by Mather as a pirate on his way to execution! (Magnalia, VI, 5). The need for such a tract and for other contemporary sermons on the Last Judgment and hell consciously designed to arouse indicates that New Englanders, instead of being "gripped by an intense and unremitting fear of death," formed instead an increasingly worldly-minded, bourgeois audience. In

another instance, Stannard truncates a quotation from Cotton Mather (p. 1326) making it rule out the efficacy of death-bed repentance, whereas the full sentence finds such "Change of Mind" to be "usually no more than a conviction" (The Thoughts of a Dying Man, pp. 40-41).

The Puritans were, then, traditional in their view of death, a tradition that included a fear of dying itself and an optimistic rhetoric of its outcome for Christians. The fear, however, was not "intense and unremitting," nor was the rhetoric unmindful of assaults by the flesh, the world, and the devil. The tension between the two did not exceed that usually found existing between hope and experience.

GORDON E. GEDDES
University of California,
Riverside

#### TO THE EDITOR:

Perhaps David Donald only skimmed A History of Mississippi (AHR, 78 [1973]: 1523-25), and felt that a bland positive review would in some way further the cause of historical research within Mississippi. But the book is a—mitigated—disaster, and praising it can only disserve the profession.

From its inception, the enterprise was vitiated by traditional white racism. The state is 40 per cent black; within it live excellent black historians; qualified white historians teach at black institutions within Mississippi; fine black scholars from Mississippi now teach and write elsewhere. Thus it is outrageous that forty-four of forty-four contributors should be white, associated with white institutions.

Predictably, then, the book itself is racist. Bias is shown perhaps most blatantly by the editor himself, writing on higher education in this century. Dr. Richard McLemore ignores almost every major issue, particularly of the past twenty years, instead listing such "facts" as the names of the presidents of each white college. He makes no mention of Mississippi's "unwritten rule" against participation by white schools in events that might be integrated, nor of Mississippi State's violation of it in 1963. The clampdown on academic freedom at places like Millsaps and Mississippi State, 1954-64, goes unreported. His description of the formal desegregation of higher education, which he terms "integration," ignores the fact that desegregation has not really occurred and gives the reader no basis for understanding why new federal orders now face these institutions. McLemore even engages in the petty racism of listing the institutions unalphabetically so he can put the black schools last!

This theft of the past for public-relations purposes denies to all Mississippians a resource for understanding the present. And the book's errors are no accident. For example, when Dr. Bettersworth uses 1890s oral interviews to debunk the "free state of Jones" events, against better evidence to the contrary, he does so in order to expunge from state history the fact that there were at times whites who dissented from the racist policies of the state's white leadership. Such deletions destroy the heritage of those whites who would find dissenting heroes in their history with whom to identify.

Likewise, the lack of full biographical treatment of even one black Mississippian destroys the heritage of black Mississippians so far as this work is concerned.

Another kind of racism pervades the better chapters of the book. The chapter on civil rights is an example. Dr. McMillen has based his chapter on his fine book on the Citizens

Councils. It is not, therefore, a history of the black struggle for civil rights, but of the white reaction to that struggle. Even here, then, blacks do not act. The Reconstruction chapters similarly focus upon such matters as the bills passed by the legislature and do not get into the massive changes occurring in black institutions and social life.

There are two good chapters—by Moore and Bearss. Some other chapters have something to offer, but overall the work is very poor. Dr. Donald's review is simply far too glowing. His own acceptance of the idea that history should be racist is shown by the following sentences from his review: [these essays] "offer most readers all the information they ever need to know about Mississippi"; and "references to the black half of the state's population are, throughout, few." His review, along with the book itself, convince me that we simply cannot expect his torical scholarship about Mississippi from older white Mississippians.

JAMES W. LOEWEN Tougaloo College

# Recent Deaths

Jesse Dunsmore Clarkson, a native of Brooklyn, New York, died in Bay Shore, New York, on September 5, 1973, at the age of seventy-six. He spent his undergraduate years at Williams College (B.A. 1918) and, under the sponsorship of Carlton J. H. Hayes, received his doctorate at Columbia University in 1925. He began his teaching career at City College of New York. In 1930 he joined the faculty of newly established Brooklyn College where he remained until his retirement in 1967. In the 1950s and 1960s he repeatedly served as visiting professor of Russian history at Columbia University and in the same capacity at Berkeley in 1962.

Clarkson was one of the founders of Russian historical studies on this side of the Atlantic. This notable achievement was not prefigured by his erudite and imaginative doctoral dissertation published under the title Labor and Nationalism in Ireland (1925). This massive work gives a detailed, problem-oriented account of the history of the urban labor movement, both as a trade-union and a political movement, by penetratingly correlating the successive stages of labor's revolt against the established order in Ireland with the perennial struggle for national independence.

The mid-1920s marked a crucial turning point in Clarkson's scholarly activity. Uncommonly enough, in lonely self-reliance and in deviation from current academic practices and interests at a time when American research on Russian history was almost equal to zero, he boldly made a fresh start by concentrating his energies thenceforth on the methodical pursuit of the study of the Russian past. The first visible fruit of this historiographical reorientation was the translation and editing of a Marxist classic: M. N. Pokrovsky's History of Russia from the Earliest Times to the Rise of Commercial Capitalism (1931). Thirty years later Clarkson presented his own interpretive version of more

than a thousand years of Russian development in A History of Russia (1961).

A result of wide reading, persistent critical probing and sober reflection, this huge work, rich in colorful information, demonstrates Clarkson's analytical acumen, the clarity and precision of his thinking, the independence of his views, and the forcefulness of his incorruptible, sometimes caustic and provocative judgment. Though unevenly balanced in temporal and topical coverage and somewhat contrived in the portrayal of Russian culture, his volume is nevertheless held together by invigorating unity of thought cemented by a sharp grasp of the practical realities of historical life. Suspicious of personalistic and nonmaterialistic interpretations of historic continuity and change, Clarkson's central effort focuses on the process of the interplay of impersonal economic, social, and political forces and on the understanding of the web of structural relationships.

Clarkson had a highly intriguing, complex personality. This, blended with his learning, dedication, and unusual dialectical powers, accounts for his remarkable success, on various levels of discourse, as an inspiring teacher. As an administrator and academic community leader he was stubbornly committed to the safeguarding of exacting standards of performance. At Brooklyn College he exerted a decisive formative influence on the history department, most strikingly during the years 1937–50 when he served as its chairman.

Jesse D. Clarkson was a man of stature who will leave a mark in our profession.

HANS ROSENBERG University of California, Berheley

The sudden death of BEATRICE FRY HYSLOP in Rochester, New York, on July 23, 1973, deprived the American Historical Review of a distinguished contributor of articles and re-

views. For the last quarter of a century, moreover, she compiled its listing in French history for the Recently Published Articles section.

Born in the city of New York on April 10, 1899, the daughter of James Hervey and Mary Fry Hall Hyslop, she was educated in public schools and Barnard School for Girls, at Mt. Holyoke College (A.B. 1919), and at Columbia University (A.M. 1924 and Ph.D. 1934). Her father was a philosopher, psychologist, and leader in psychical research. Miss Hyslop taught briefly at Mt. Holyoke College from 1926 to 1928 and in 1936 was appointed instructor at Hunter College, where during the next thirtythree years she progressed steadily and retired with the rank of full professor. The opportunity to direct professionally oriented students did not come until late in her career, but she was a member of the graduate faculty of the City University of New York from its formation in 1961 until her retirement in 1969.

Her historical curiosity and her industry were prodigious. She gave of her knowledge and experience constantly and generously to students at all stages of their intellectual development; and throughout her life her articles, lectures, and reviews found place in major historical journals at home and abroad. Her interest in European history and the history of art had begun in college, and at Columbia she focused that interest on French history and began the researches in the French materials that were to establish her firmly as an authority on the cahiers. Even before she received the doctorate, the thoroughness of her method and her familiarity with French archives had been indicated by the publication of her Répertoire critique des cahiers de doléances pour les états-généraux de 1789 (Paris, 1933; Supplément, 1952). This and her doctoral thesis, French Nationalism in 1789 according to the General Cahiers (1934) were quickly followed by the Guide to the General Cahiers of 1789 (1936). Two of her major writings came later: L'Apanage de Philippe-Égalité, duc d'Orléans 1785-1791 (Paris, 1965) and a textbook, in collaboration with Jacques Godechot and the late David Dowd, The Napoleonic Era in Europe (1970). At the time of her death she had in preparation and under contract a new study of the cahiers.

As a woman of untiring friendliness and as

an active member of learned societies on both sides of the Atlantic, notably the American Historical Association, the Society for French Historical Studies, the Société des études robespierristes, and the Société d'histoire moderne, she contributed in an extraordinary degree to the promotion of Franco-American historical cooperation. Her position was recognized in various ways. In 1951-52 she was awarded a research fellowship from the Fulbright Foundation; and in 1955-56 on a Fulbright exchange professorship she lectured at the École des Hautes-Études of the Sorbonne and at the University of Toulouse. The government of France decorated her as Chevalier des Palmes académiques in 1931 and raised her to Officer in 1952, and in December 1961 invested her as Chevalier de la Légion d'Honneur. The review French Historical Studies paid her the unprecedented tribute of dedicating to her its Fall 1972 number; and the parent society in full meeting at Chapel Hill in March 1973 presented her with a specially bound copy of the issue. In her honor the Ash Avenue Unitarian Universalist Church in Jackson Heights held a memorial service on July 31, 1973.

GEORGIA ROBISON BEALE Orford, New Hampshire

Other members of the association who have died recently include: James E. Bland of Bowdoin College, Brunswick, Maine; Henry M. Dater of Washington, D.C.; Stanton Griffis of New York City; Clinton N. Howard of the University of California, Los Angeles; J. E. Jordan of California State College, Fullerton; Suzanne G. Korirsh of Redwood City, California; Jakob Aall Ottesen Larsen formerly of the University of Missouri at Columbia; Murray G. Lawson of Washington, D.C.; Georgiana Putnam McEntee, professor emeritus of history at Hunter College, New York City; Robert D. Meade of Randolph Macon Woman's College; Richard Lee Morton of Williamsburg, Virginia; Kenneth Munden of Arlington, Virginia; Bessie Louise Pierce, professor emeritus of history at the University of Chicago; Heston N. Potts of North Branch, New Jersey; E. C. Rozwenc of Amherst, Massachusetts; Albert K. Weinberg of Baltimore, Maryland; and Edward F. Wenz of Cincinnati, Ohio.

# Festschriften and Miscellanies

These volumes, recently received in the AHR office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other Festschriften and similar volumes that are amenable to reviewing will be found in the review section.

BOTZ, GERHARD; HAUTMANN, HANS; and KONRAD, HELMUT, editors. Geschichte und Gesellschaft: Festschrift für Karl R. Stadler. Vienna: Europaverlag. 1974. Pp. 583. DM 45.

Geschichte der Habsburger-Monarchie: ERICH ZÖLLNER, Aus Stammbüchern österreichischer Handwerker. Heinrich Lutz, Politik und militärische Planung in Österreich-Ungarn zu Beginn der Ära Andrássy. Protokoll der unter Allerhöchstem Vorsitze am 17. Februar 1872 abgehaltenen Konferenz. (16<sup>1</sup>) Protokoll der unter dem Allerhöchsten Vorsitze am 18. Februar 1872 abgehaltenen Konferenz. RICHARD GEORG PLASCHKA, Widerstand 1915 bis 1918 am Modell Pilsen. Ein Industriezentrum der Donaumonarchie im Spiegel der Berichte der Zivilbehörden.

Österreich 1918–1938: GUSTAV OTRUBA, "BAUET" und "Arbeiter" in der Ersten Republik. STEPHAN VEROSTA, Bemerkungen zum Brief Otto Bauers an Jean Longuet vom 9. Januar 1919. ERIKA WEINZIERL, Aus den Notizen von Richard Schmitz zur österreichischen Innenpolitik im Frühjahr 1933. LUDWIG JEDLICKA, Gauleiter Josef Leopold (1889–1941).

Zweite Republik: ADAM WANDRUSZKA, Österreich und Italien seit dem Zweiten Weltkrieg. RUDOLF STRASSER, Der Beitrag des Obersten Gerichtshofes zur Arbeitsrechtsentwicklung in der Zweiten Republik. FELIX KREISSLER, Die Entwicklung der SPÖ in ihren Programmen und in ihrer Politik: Vom Austromarxismus zum "Austrosozialismus" (1945–1973).

Geschichte der Arbeiterbewegung: ERNST WANGER-MANN, Die Auseinandersetzung über das Verhältnis von Reform zu Revolution in der deutschen und österreichischen Sozialdemokratie. FRANCIS L. CARS-TEN, Arthur Rosenberg als Politiker. ADOLF STURM-THAL, Werner Sombart und der amerikanische Sozialismus. YVON BOURDET, Georg Lukács im Wiener Exil (1919–1930). GEORGES HAUPT, Renaissance oder Stagnation des Marxismus heute?

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#### GERMANY, AUSTRIA, AND SWITZERLAND

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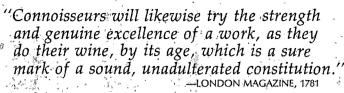
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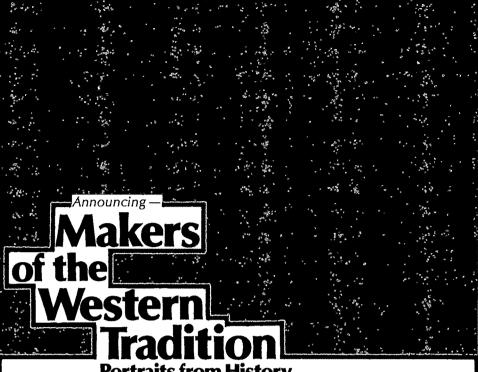
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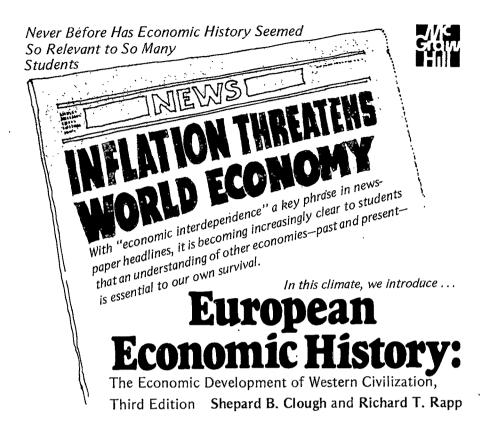
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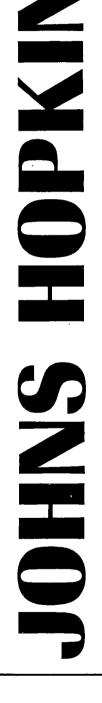
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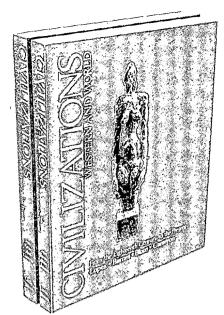
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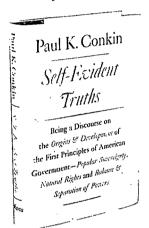
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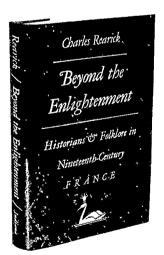
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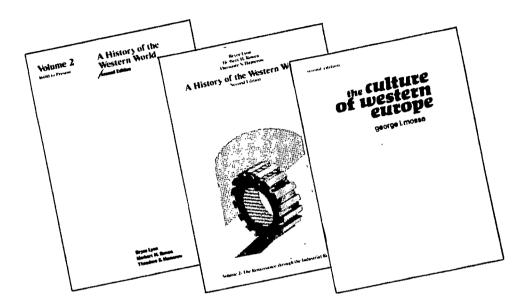
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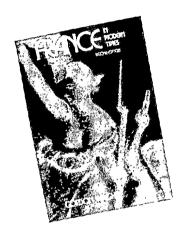
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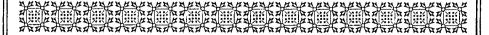
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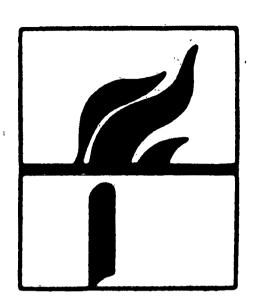
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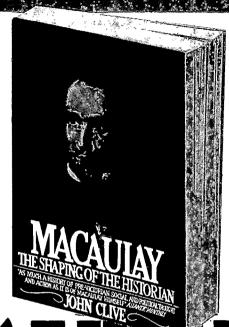
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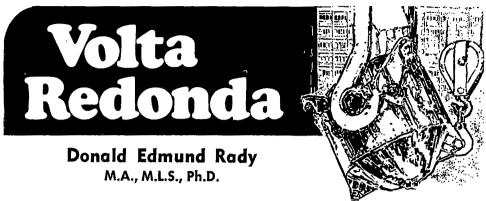
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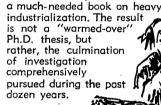


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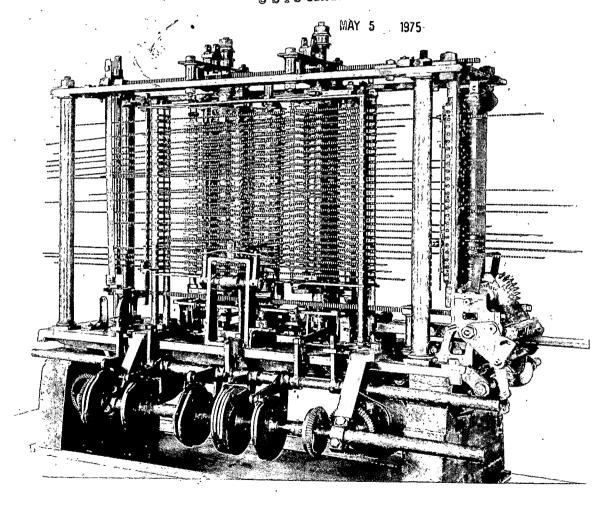
## Index of Advertisers

Academic Press	8	Louisiana State University Press 54
American Historical	2 4	McGraw-Hill Book Co. 21, 22, 23
Association 1, 2, 68	8, 69	Macmillan Publishing Co. 30, 31, 66
Baker & Taylor Co.	32	National Council for the
Barnes & Noble Books	47	Social Studies 64
Cambridge University Press	28	New American Library 40
Columbia University Press	19	Oxford University
Cornell University Press	6, 7	Press 2d Cover, 20, 48
Dodd, Mead & Co.	63	Prentice-Hall 16, 17
Dorsey Press	ŭ	Princeton University Press 4th Cover
•	33	Rand McNally & Co. 50, 51
Harcourt Brace Jovanovich 24, 25, 52		Random House 37, 38, 39
Harper & Row Publishers 11, 12, 13, 14	1. 42.	Rio Grande Publishing Co. 65
43, 5		St. Martin's Press 5, 9
Harvard University Press 3d C	lover	Scott, Foresman & Co. 41
D. C. Heath & Co.	29	Stanford University Press 4
Houghton Mifflin	18	University of California Press 10, 53
Indiana University Press	49	University of Chicago Press 27, 55
Inter American University Press	62	University of Minnesota Press 61
Johns Hopkins University Press	34	Vintage Books 59
Kraus-Thomson	60	Wesleyan University Press 58
Alfred A. Knopf	45	John Wiley & Sons 15, 26, 36, 46, 67
Little, Brown & Co.	5, 44	Xerox University Microfilms 3

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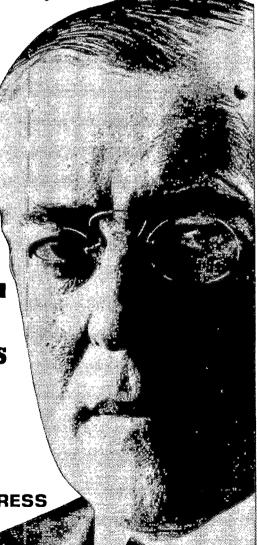
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#### The Limits of Quantitative Methods in History

#### ROBERT WILLIAM FOGEL

This article deals with seven questions: should quantitative methods be used in history? what is the status of the effort to apply quantitative methods to history? what kinds of quantitative methods should be used in history? will the use of quantitative methods make history scientific? to what kinds of issues should quantitative methods be applied? what role should quantitative methods play in the graduate training of history students? and what can be done to overcome the problem in communication created by the intrusion of explicit quantitative methods into historical literature?

FIRST, SHOULD QUANTITATIVE METHODS be used in history? In one respect I find it surprising that there is still an active debate on this question, for the implicit premise of the question is false. The question presumes that we have a choice as to whether or not we will use quantitative methods in history. I would argue that given the interests and objectives of historians such a choice does not really exist. It can be demonstrated that many of the most important questions which concern historians involve quantification in an essential way.

For example, historians are concerned not only with Andrew Jackson's attitude toward the Second Bank of the United States but with the effect of his attitudes and policies on the price level, on employment, and on

This paper was prepared for presentation at the Conference on Mathematics in the Social Sciences, convened at Southern Illinois University, Carbondale, April 21–23, 1974. Earlier versions were presented at the Conference on Quantitative Methods in History, sponsored by the Graduate Center of the City University of New York, on May 9, 1970; at the Institute on Humanistic Inquiry, held at the University of Chicago during January 19–23, 1972; and at the Historisk Institutt of the Universitet i Oslo on March 26, 1973. Parts of the answers to the first and third questions are derived from two of my previous essays: Railroads and American Economic Growth: Essays in Econometric History (Baltimore, 1964), 241–42; and "Historiography and Retrospective Econometrics," History and Theory, 9 (1970): 252–55. I have benefited from comments and criticisms by Robert McC. Adams, Thomas B. Alexander, Allan G. Bogue, Jerome M. Clubb, David Herbert Donald, G. R. Elton, Stanley L. Engerman, John Hope Franklin, Richard B. Freeman, François Furet, Eugene D. Genovese, Phillip M. Hauser, David Herlihy, C. P. Kindleberger, Wassily W. Leontief, William N. Parker, Willie Lee Rose, Richard N. Rosett, Robert P. Swierenga, Charles Tilly, John T. Wilson, and C. Vann Woodward.

other things that have been connected with the boom and bust of the 1830s. Consequently historians are not content merely to describe Jackson's policies; they also want to assess the consequences of these policies. But such an assessment depends on the dimensions of the economic effects that followed from the policies. Obviously if the policies had only trivial consequences, that is, if they had hardly any effect on the rise in prices or the subsequent fall in production and employment, then we would have one attitude toward what Jackson believed and did. And we would have another attitude if we thought the effects were large.<sup>1</sup>

Why then is there still a debate about whether or not quantitative methods ought to be used in history? Because the pervasiveness of quantitative considerations in historical study is not fully appreciated, and because most often when historians engage in quantitative analysis they do so implicitly rather than explicitly. Mathematics has long been an intrinsic feature of historical analysis, but its use has been covert and subliminal. This is because many issues that turn crucially on quantitative dimensions are disguised by words; they are not apparent because they are put forward in words instead of in numbers or equations. I sometimes illustrate this proposition by challenging my students to choose a page at random from one or another book of economic history that is in their possession and to determine whether or not quantification enters implicitly in the discussion on that page. It does so in a surprisingly high percentage of cases. The following quotation was chosen, not at random, but because it illustrates how ingrained and pervasive implicit quantification can be. The quotation is from George Soule's Economic Forces in American History.

Regional concentration of industries and specialized crops, though dependent on many factors, could not have developed so fully without railroad transportation. New England could find national markets for its textiles and shoes. Pennsylvania, with its coal and easy access to iron, could concentrate on basic iron and steel, shipping the products wherever they might be wanted. Iowa could, with its specially adapted soils and climate, become a corn and hog country.<sup>2</sup>

Now this apparently qualitative description is permeated with implicit measurement. The "regional concentration" referred to in the first sentence can only be defined in quantitative terms; the phrase implies the existence of a measure of the spatial distribution of productive activity. The sentence as a whole implies that the difference between the cost of transportation by railroad and the next most favorable medium was of such a magnitude

<sup>&</sup>lt;sup>1</sup> Recent research indicates that Jackson was more the victim of economic forces beyond his control than the prime agent in precipitating the boom and bust of the 1830s. See Peter Temin, The Jacksonian Economy (New York, 1969); and Hugh Rockoff, "Money, Prices, and Banks in the Jacksonian Era," in Robert W. Fogel and Stanley L. Engerman, eds., The Reinterpretation of American Economic History (New York, 1971), 448-58.

<sup>2</sup> George Soule, Economic Forces in American History (New York, 1952), 103.

that the absence of the railroad would have reduced regional concentration by a detectable, and therefore measurable, amount. Indeed the sentence implies that such a measurement has in fact been performed. The term "national market" in the second sentence, if it is to have anything but a trivial meaning, implies that the amount of shoes and cotton goods sold by various firms beyond some region specified as local was large relative to total output. The only significant economic interpretation of the phrase "easy access" (third sentence) is that the cost of obtaining iron in Pennsylvania was lower than in other designated areas. The statement that Pennsylvania concentrated on the production of basic iron and steel implies a system by which the amounts of the qualitatively different products of the state can be aggregated and against which the state's production of iron and steel products can be measured. Finally the statement that Iowa's soil and climate were "specially adapted" to corn production (fourth sentence) presumes the existence of a measurable relationship between corn yields on the one hand and rainfall, temperature, and various soil properties on the other.

My response to the first question, then, is that it is a waste of time to argue whether or not one ought to permit quantification in historical writing since it is not possible to exorcise this demon. The real question is how to use quantification to the best advantage.

When the question is posed in this manner, it becomes evident that rigid positions are untenable and must give way to flexible ones. It does not, for example, follow that because an argument presupposes the existence of equations the historian who employs it must necessarily impose the equations on his readers. Words and equations are alternate modes of expression, and which is preferable at a given point in a given essay is a problem of exposition involving esthetic as well as empirical and logical considerations. The best resolution of these often conflicting elements of historiographic literature will not only vary from essay to essay but even from point to point within the same essay.

NEXT, WHAT IS THE STATUS of the effort to apply quantitative methods to history? The movement for the systematic application of formal behavioral models and of the related mathematical and statistical methods to historical analysis is about two decades old. This type of work now has a firm foothold in the historical profession, both in the United States and abroad.

The mathematical approach has developed most rapidly within the field of economic history. The new economic history, also called cliometrics and econometric history, has become the predominant form of research in this field, at least in the United States. The majority of the articles published in the main economic history journals of the United States

are now quite mathematical, and cliometricians predominate in the leadership of the Economic History Association.<sup>3</sup>

The progress of the mathematical approach in the mainstream of history, while less rapid, has nevertheless been substantial.4 There are research groups engaged in the application of mathematical methods to history in at least a dozen American universities. Several national committees have come into being to encourage the application of mathematical methods to history. In 1965 the Mathematical Social Science Board (MSSB) established a History Advisory Committee through which it has organized conferences and advanced research institutes on various subjects that represent the frontier of research by quantitative historians. In conjunction with Princeton University Press, MSSB is sponsoring a ten-volume series entitled Studies in Quantitative History. The opening volume lays stress on the scope of quantitative methods that are today being applied to history and the variety of issues to which these methods are germane. Each of the other volumes deals with a historical problem that has been the focus of considerable research by quantitative historians, bringing together the best of this work. Among titles in the MSSB-Princeton series are Slavery and Race in the Western Hemisphere, The New Urban History, "The History of Parliamentary Behavior," and "American Electoral History."

The American Historical Association has an ad hoc Committee on Quantitative Data in History that is concerned with the collection of quantitative information and its transcription into machine-readable form. The Inter-University Consortium for Political Research (ICPR) has established a Historical Data Archives that is collecting and putting into machine-readable form large quantities of political, economic, and social information for both the United States and France. ICPR also runs an annual summer training institute on mathematical methods for historians. During the past year the Social Science History Association has been incorporated. With an organizing committee of seventy and four hundred pledges of support, the first meeting of SSHA is tentatively scheduled for late 1975.<sup>5</sup>

<sup>5</sup> The activities of MSSB are described in Charles Tilly, "A Report on the Work in Quantitative History by MSSB," mimeographed (Ann Arbor, 1974). The work of the ad hoc Committee

<sup>&</sup>lt;sup>3</sup> For assessments of the new economic history, see Thomas C. Cochran, "Economic History, Old and New," AHR, 74 (1968-69): 1561-72; Maurice Lévy-Leboyer, "La 'New Economic History," Annales: Économies, Sociétés, Civilisations, 24 (1969): 1035-69; John Habakkuk, "Economic History and Economic Theory," Daedahus, 100 (1971): 305-22; and Ralph W. Hidy, "The Road We Are Traveling," Journal of Economic History, 32 (1972): 3-14. Two collections of essays by new economic historians are Fogel and Engerman, Reinterpretation of American Economic History, and Peter Temin, ed., The New Economic History (Harmondsworth, 1973).

<sup>&</sup>lt;sup>4</sup> Assessments of quantitative work in the mainstream of history include William O. Aydelotte, "Quantification in History," AHR, 71 (1965-66): 803-25; David S. Landes and Charles Tilly, eds., History As Social Science (Englewood Cliffs, 1971); and Robert P. Swierenga, "Computers and American History: The Impact of the 'New' Generation," Journal of American History, 60 (1974): 1045-70. Some notion of the range of quantitative work in history can be obtained from Don Karl Rowney and James Q. Graham, Jr., eds., Quantitative History (Homewood, 1969); Robert P. Swierenga, ed., Quantification in American History (New York, 1970); and William O. Aydelotte, Allan G. Bogue, and Robert W. Fogel, eds., The Dimensions of Quantitative Research in History (Princeton, 1972).

Two journals have been established to promote quantitative history—the *Historical Methods Newsletter* and the *Journal of Interdisciplinary History*. A third journal, sponsored by SSHA, will probably begin publication in 1976.

The effort to apply mathematical methods to history has also made substantial progress in Europe. The leading European country in this respect is France, where the relative investment of intellectual resources in quantitative history may be greater than in the United States. The French center for this work is the Sixth Section of the Ecole Pratique des Hautes Etudes. There are several centers for the application of quantitative methods in England, the largest of which is located at Cambridge. Each of the Scandinavian countries has at least one historical institute that emphasizes quantitative methods. There are at least two institutes devoted to the application of mathematical methods to history in the Soviet Union.<sup>6</sup>

Despite various solid intellectual achievements, quantitative history is still confined to the edges of the historical discipline. This is not primarily because traditional historians are unwilling to recognize the accomplishments. While some traditionalists still sweep aside all quantitative work as nonsense, many acknowledge specific achievements. It is now widely agreed that one should, by all means, count where counting is possible and useful. There is, however, considerable skepticism about how much can be accomplished by formal mathematical techniques. The majority probably hold with Arthur Schlesinger, jr. that "almost all important questions are important precisely because they are *not* susceptible to quantitative answers."

As yet the published record of quantitative historians does not provide the basis for refuting Schlesinger's assertion. Only in economic history have quantitative methods overturned the central propositions of the literature

on Quantitative Data in History is described in the annual reports of the American Historical Association; see especially the annual report for 1971, pages 134-37. The activities of ICPR are described in Jerome M. Clubb, "The Inter-University Consortium for Political Research: Progress and Prospects," Historical Methods Newsletter, June 1969, pp. 1-5; Clubb, "Historical Politics: American Elections, 1824-1970," Items, 25 (1971): 46-50; and Inter-University Consortium for Political Research, A Guide to Resources and Services, 1973-1974 (Ann Arbor, 1974).

<sup>6</sup> Quantitative research in France is described in François Furet and Emmanuel Le Roy Ladurie, "L'historien et l'ordinateur: Compte-rendu provisoire d'enquête," Rapport collectif presenté par le Centre de Recherches Historiques de l'Ecole Pratique des Hautes Etudes (Moscow, 1970). The scope of quantitative work in Great Britain is suggested in E. A. Wrigley, ed., Nineteenth Century Society: Essays in the Use of Quantitative Methods for the Study of Social Data (Cambridge, 1972); Wrigley, ed., Identifying People in the Past (London, 1973); and Donald N. McCloskey, ed., Essays on a Mature Economy: Britain after 1840 (London, 1971). Some aspects of quantitative research in the USSR are considered in J. Kahk and I. D. Kovalchenko, "Methodological Problems of Mathematical Methods Application in Historical Research" (recently published in Historical Methods Newsletter, June 1974, pp. 217-24), and L. V. Milov and K. V. Khovostova, "Quantitative Methods Applied by Soviet Historians to Agrarian History," both of which were presented at the International Conference on History and the Computer held in Uppsala, Sweden, June 24-30, 1973.

<sup>&</sup>lt;sup>7</sup> Arthur Schlesinger, jr., "The Humanist Looks at Empirical Social Research," American Sociological Review, 27 (1962): 770, his italics.

and led on to far-reaching, new insights regarding the historical processes that were at work. But these achievements have had only limited significance to historians generally. Economic history is considered too special a field—narrow in the range of issues it embraces and highly focused on matters unusually well adapted to quantitative methods—for developments there to have great methodological relevance for the mainstream of history.

While the work of quantitative historians in political and social history has been closer to the central concerns of the discipline at large, the findings have seemed less compelling than in economic history. The new urban history, for example, has so far produced only a series of case studies. The analyses of social mobility in Newburyport, Philadelphia, and Boston are all quite interesting as local histories and in some respects highly suggestive. Still, the general significance of the findings remains to be established, although an important step in this direction was taken in the final chapter of Stephan Thernstrom's *The Other Bostonians*. Similarly, there have been some quite significant findings by the new political historians regarding the relative importance of economic class, religion, and ethnicity in popular voting behavior. But these, too, have tended to be focused on particular localities, and not enough localities have been studied as yet to produce warranted generalizations. 10

Although the accomplishments of quantitative historians have not been great enough to quash skepticism among senior historians, they have nevertheless been enough to fire the imagination of many graduate students. The new urban history, the new social history, the new demographic history, and the new political history are all attracting young disciples. This has led to a growing demand for training in behavioral models and statistical methods.

Very few departments of history have as yet adequately accommodated their curriculums to this growing market. As a consequence, students interested in mathematical methods are left largely to their own devices. Some attempt to remedy the deficiency by taking courses in social-science departments, but frequently they find that these courses demand prerequisites they lack. Even if they have the prerequisites, the courses are rarely geared

<sup>&</sup>lt;sup>8</sup> For assessments of the new urban history, see Stephan Thernstrom, "Reflections on the New Urban History," *Daedalus*, 100 (1971): 359-75; and Eric E. Lampard, "Two Cheers for Quantitative History: An Agnostic Foreword," in Leo F. Schnore, ed., *The New Urban History* (Princeton, 1975), 12-48; see also Samuel P. Hays, "A Systematic Social History," in George Athan Billias and Gerald N. Grob, eds., *American History: Retrospect and Prospect* (New York, 1971), 315-66.

<sup>&</sup>lt;sup>o</sup> Stephan Thernstrom, The Other Bostonians: Poverty and Progress in the American Metropolis, 1880-1970 (Cambridge, Mass., 1973).

<sup>&</sup>lt;sup>10</sup> Recent appraisals of the new political history include Swierenga, "Computers and American History," <sup>1052–55</sup>; Richard L. McCormick, "Ethno-Cultural Interpretations of Nineteenth-Century American Voting Behavior," *Political Science Quarterly*, 89 (1974): <sup>351–77</sup>; and the introduction to William O. Aydelotte, ed., "The History of Parliamentary Behavior," mimeographed (1974).

to the specific needs of historians. Nor do these courses usually provide instruction in the art of applying behavioral models to history.

There are, however, reasons to believe that situation will change dramatically during the next ten years. One indication of a new situation is the recent award of the Bancroft Prize to Stephan Thernstrom for The Other Bostonians. This is the first time that one of the leading prizes in history has been awarded for a study in which quantification has been so central. Rather than an isolated event, The Other Bostonians is, I believe, the vanguard of a series of books based on the systematic application of quantitative methods that will command the attention of the entire discipline. Other compelling quantitative histories are now in preparation or can be expected to be completed during the next decade. One of these is Charles Tilly's study of conflicts in Europe during the nineteenth century.11 It also is reasonable to expect that the score or more studies of social mobility in American cities during the nineteenth century that have been undertaken will soon provide the basis for an important synthesis. Other questions that have become the targets of concerted quantitative research and that should come to fruition within a decade or so include the analysis of the black experience during the half century between the Civil War and World War I, legislative history in Europe and the Americas, popular voting behavior during the past century, and social histories of sex, marriage, and the family, stretching back into medieval times for Great Britain and France and into the colonial period for the United States.<sup>12</sup>

11 This project, which exemplifies the kind of team research that has come into prominence with the large-scale collection and analysis of quantitative data, already embraces more than a score of separate studies. Aspects of the findings to date are reported in Edward Shorter and Charles Tilly, Strikes in France, 1830–1968 (Cambridge, 1974), and Charles Tilly, Louise Tilly, and Richard Tilly, The Rebellious Century (Cambridge, Mass., 1975). The capstone of the project, tentatively entitled "French Collective Action, 1700–1975," will attempt to survey and synthesize the whole range of evidence accumulated by Tilly's group, produce an empirically based theory of collective action, and provide a new paradigm for French history during the past three centuries. It is scheduled for completion in 1975.

12 Some impression of the scope of the research on social mobility can be gleaned from the papers in Stephan Thernstrom and Richard Sennett, eds., Nineteenth-Century Cities: Essays in the New Urban History (New Haven, 1969); Thernstrom, Other Bostonians, especially ch. 9; and Schnore, New Urban History; see also Oscar Handlin, Boston's Immigrants, 1790-1880 (Cambridge, Mass., 1941), and Handlin, *The Uprooted* (Boston, 1951). Recent quantitative work in legislative history is illustrated in Aydelotte, "History of Parliamentary Behavior"; see also Thomas B. Alexander and Richard E. Beringer, *The Anatomy of the Confederate Con*gress (Nashville, 1972). For examples of recent quantitative work in electoral history see Joel H. Silbey, Allan G. Bogue, and William Flanigan, eds., "American Electoral History: Quantitative Studies," mimeographed (1974); Richard Jensen, The Winning of the Midwest: Social and Political Conflict, 1888-1896 (Chicago, 1971); and Ronald P. Formisano, The Birth of Mass Political Parties: Michigan, 1827-1861 (Princeton, 1971); see also Lee Benson's innovative study, The Concept of Jacksonian Democracy: New York As a Test Case (Princeton, 1961). The scope of research into the history of the family is indicated in E. A. Wrigley, ed., An Introduction to English Historical Demography from the Sixteenth to the Nineteenth Century (London, 1966); Theodore K. Rabb and Robert I. Rotberg, eds., The Family in History: Interdisciplinary Perspectives (New York, 1973); and Michael Gordon, ed., The American Family in Social-Historical Perspective (New York, 1973). The new work on the post-Civil War experiences of blacks includes Richard B. Freeman, "The U.S. Discriminatory System: The Economics of Discrimination against Black Americans, 1870-1970," mimeographed; Richard Sutch and Roger

THIRD, WHAT KINDS OF QUANTITATIVE METHODS should be used in history? In considering this question it is useful to draw a distinction between informal or impressionistic methods of measurement and formal or rigorous methods of measurement. The most commonly applied methods of measurement in history are informal. Let me hasten to add that no pejorative connotation is to be attached to the term "informal." I am not in favor of overkill in measurement or anything else.

For many historical issues informal measurement is appropriate, and formal measures add nothing, or even detract from historical analysis. There is, for example, a passage in Arthur Schlesinger's *The Politics of Upheaval* describing a strategy conference at the White House after the Supreme Court struck down the NRA. At that meeting Homer Cummings, the attorney general, "striding the room in anger," urged Roosevelt to "get rid of the present membership of the Supreme Court." 13

Now I interpret the word "anger" as a measure of intensity of feeling. Schlesinger was not very precise. He did not report Cummings's pulse or respiration rates; no electrodes were attached to Cummings's brain so that one could determine his brain-wave pattern. With proper equipment one could have developed an elaborate set of measurements and used these to define precisely the term "anger" in this particular instance. But obviously such measurements, even if they could have been obtained, would be quite useless. They would add nothing to our understanding of the situation that Schlesinger sought to describe. All that was required in this case was a rough distinction between possible emotional states. "Anger" is a sufficient distinction; it is different enough from "joy" or "apathy" to have suited Schlesinger's purposes. While Schlesinger's measurement was not precise, it was appropriate to the issue with which he was concerned.

On the other hand, there are cases in which the reliance on informal measurement has badly misled scholars on vital issues. It was long believed, for example, on the basis of casual impressions, that the Jacksonian elections of 1828 and 1832 witnessed a new level of popular participation. When Richard P. McCormick assembled state-by-state counts of participation in these and earlier elections, it turned out that the impression was false. Electoral participation rates in various previous elections were approximately the

Ransom, "What Was Freedom Worth?" mimeographed; Joseph Reid, Jr., "Sharecropping and Agricultural Uncertainty," mimeographed (paper no. 257, Department of Economics, University of Pennsylvania, 1973); Stephen J. DeCanio, Agriculture in the Postbellum South: The Economics of Production and Supply (Cambridge, Mass., 1974); Robert Higgs, "Race, Tenure, and Resource Allocation in Southern Agriculture, 1910," Journal of Economic History, 33 (1973): 149-69; Gavin Wright, "Cotton Competition and the Post-Bellum Recovery of the American South," Journal of Economic History, 34 (1974): 610-35; Ralph Schlomowitz, "Institutional Arrangements in Southern Agriculture, 1865-1868," report no. 7475-12, paper presented to the Workshop in Economic History, University of Chicago, Jan. 17, 1975; and Stanley L. Engerman, "Changes in Black Fertility, 1880-1940," mimeographed, working paper prepared for the MSSB-Williamstown conference, Williams College, July 1974.

<sup>13</sup> Arthur Schlesinger, jr., The Age of Roosevelt, vol. 3: The Politics of Upheaval (Boston, 1960), 288-89.

same as those that prevailed during the Jacksonian era.<sup>14</sup> Similarly, in The Vendée Charles Tilly's tabulations of police rolls revealed that counterrevolutionaries in France during the 1790s were much more heterogeneous in their background than had been presumed and that artisans and other bourgeois elements had played a bigger role in the counterrevolutionary movement than had been realized.<sup>15</sup> Or consider the assertion, very widespread in the literature of the American West, that the reduction in transportation costs brought about by railroads was not only very large but a necessary condition for the settlement of the prairies. The casual impressions that gave rise to that assertion have also proved to be unwarranted. Analysis of data on transport costs and land values has revealed that ninety-five per cent of the prairie land in commercial cultivation in 1890 would have been cultivated even in the absence of railroads.<sup>16</sup>

One can divide the various methods of rigorous measurement currently employed in historical research into two categories—direct and indirect. The most common method of direct measurement in history is counting. My reference to counting as a rigorous method of measurement is not to be taken derisively. I use portentous language for what appears to be an elementary operation partly because I want to emphasize the dramatic change in interpretation that may result merely by moving from an impression to an actual count. I also wish to emphasize that counting is rarely an easy task in historical work. It was, for example, complex, costly, and time consuming to obtain from archival sources a valid distribution of the ages of slave mothers at the birth of their first surviving child. It required hundreds of man hours of research and thousands of dollars. Yet this "mere" act of counting yielded the discovery that half of all slave women were over twenty years of age at the birth of their first surviving child—a fact that threw into doubt the entire structure of traditional assumptions about the sexual behavior of slaves.<sup>17</sup>

Very often direct measurement is precluded. Then historians must make use of equations, for indirect measurement requires the existence of a

<sup>14</sup> Richard P. McCormick, "New Perspectives on Jacksonian Politics," AHR, 65 (1959-60): 288-901.

<sup>15</sup> Charles Tilly, The Vendée (Cambridge, Mass., 1964).

<sup>16</sup> Fogel, Railroads and American Economic Growth (Baltimore, 1964), 109-10.

<sup>17</sup> The distribution of ages of mothers at the birth of their first surviving child (reported in Robert W. Fogel and Stanley L. Engerman, *Time on the Cross* [Boston, 1974], 1: 137-38) was obtained from the probate records of approximately two hundred estates ranging in size from a few to over one hundred slaves. The sample has recently been expanded to 575 estates. These observations, which span the period from 1815 to 1860, are concentrated primarily in the states of Virginia, Georgia, and Louisiana. Other important sources of information on the demographic behavior of the slave population include the records of large plantations, the manuscript schedules of the U.S. census, and the pension files of Civil War veterans. These sources and some of the uses to which they can be put are discussed in Richard H. Steckel, "Slave Marriage, Fertility, and Society," report no. 7374-4, paper presented to the Workshop in Economic History, University of Chicago, Nov. 2, 1973. See also Steckel, "A List of 54 Plantations Containing Systematic Data on the Demographic Characteristics of the Slave Population," in Robert W. Fogel and Stanley L. Engerman, eds., "Evidence Relevant to the Post-Publication Debate on *Time on the Cross*," mimeographed (Rochester, N.Y., 1974).

functional relationship between what the investigator wants to measure and what he can measure. It is the intrusion of equations into a literary discipline that has aroused the most opposition to the employment of quantitative methods in history.

Despite appearances to the contrary, quantitative historians deserve neither credit nor blame for the introduction of equations into historical literature. Equations have always been a part of historical literature. Prior to the appearance of quantitative historians, however, these equations were implicit, covert, and subliminal. The contribution of historical quantifiers is that they have made implicit mathematics explicit. Moreover quantifiers are concerned with whether the implicit equations crucial to some arguments are the correct equations. Does the particular functional form implicitly designated by a historian conform to the reality that he wishes to describe?

Permit me to cite two examples that illustrate both the prevalence and treacherousness of covert mathematics. The first example is drawn from the literature on the early industrialization of the American economy. The iron and steel industry is one of the most frequently discussed manufacturing sectors. Textbook writers and others note that while the output of this industry grew at only a moderate rate before 1840, it expanded quite rapidly between 1840 and 1860. To support this statement it is common to cite the following figures on pig-iron production:

Year	Tons of Pig Iron
1840	287,000
1860	988,000

But pig iron is only one of the products of the iron industry, not its total output. As measured by value added, pig iron accounted for only about one-sixth of the total output of the industry in 1860. Consequently historians who use the rate of growth of pig-iron production to measure the rate of growth in total output are implicitly assuming the following functional relationship:

$$T = mP$$
 (1)
where
 $T = \text{total output}$ 
 $P = \text{pig-iron production}$ 
 $m = \text{a constant.}$ 

In other words, they are implicitly stating that total output of the industry is directly proportional to the output of pig iron. This is a linear equation with a zero intercept, the simplest of all equations.

Just because equation 1 is simple does not necessarily mean that it distorts reality. As a matter of fact, equation 1 is a good description of the relationship between pig-iron and total production from 1830 to 1860.

It is a poor description, however, of the relationship between these variables over the period from 1860 to 1900. Thus if one uses pig iron as a measure, one will conclude that the iron industry stagnated between 1860 and 1870, although an index of total output shows that the Civil War decade represented an era of unusually rapid expansion. Since the growth of the iron industry has been made an issue in the debate over the effect of the Civil War on Northern industrialization, the inapplicability of equation 1 for the period after 1860 is not an empty issue for historians.

The second example is drawn from the literature on American Negro slavery. In his famous essay, "The Economic Cost of Slaveholding in the Cotton Belt," published in 1905, U. B. Phillips argued that slaves were an unprofitable investment. To support his contention he assembled time series on the prices of slaves and raw cotton. These series showed that from 1815 on, slave prices rose more rapidly than cotton prices. According to Phillips that fact was sufficient to establish the proposition that the profitability of slavery must have declined over the period. Indeed, since the ratio of slave to cotton prices was much higher in 1860 than it had been in 1815, he drew the conclusion that by the eve of the Civil War slavery had become unprofitable. 19

Equation 2 is the algebraic representation of the Phillips argument:

$$i^* = k(P^*_c - P^*_s)$$
 where

i =the rate of return

k = a constant

 $P_c$  = the price of cotton

 $P_s$  = the price of slaves

\* = an asterisk superscript by a variable stands for the rate of change in that variable; thus  $i^*$  is the rate of change in i.

Equation 2 states that the rate of change in the rate of return is directly proportional to the difference between the rates of change in cotton and slave prices. When  $P^*_s$  is greater than  $P^*_c$ , not only will  $(P^*_c - P^*_s)$  be negative, but, if equation 2 is correct,  $i^*$  will also be negative. As in the previous example, the interpretation of one of the major issues of American history turns on the implicit assumption that certain variables are related to each other by a linear equation with a zero intercept.

I do not mean to give the impression that Phillips was naive. Quite the contrary, the issues in the economics of slavery that have occupied so much of the attention of econometric historians during the past two decades

<sup>18</sup> Robert W. Fogel, "The Specification Problem in Economic History," Journal of Economic History, 27 (1967): 293-94.

<sup>19</sup> U. B. Phillips, "The Economic Cost of Slaveholding in the Cotton Belt," *Political Science Quarterly*, 20 (1905): 257-75.

are the ones he defined. Not only is equation 2 related to the equation subsequently used by Alfred H. Conrad and John R. Meyer to estimate the rate of return on an investment in slaves,<sup>20</sup> but in one of the footnotes of *American Negro Slavery*, Phillips explicitly referred to the basic form of their equation,<sup>21</sup> namely:

$$P_{s} = \frac{H}{i} \left[ 1 - \frac{1}{(1+i)^{n}} \right] \tag{3}$$

where

 $P_s$  = the price of a slave

H = the expected average annual net income to be earned from the employment of the slave

n = the expected number of years between the purchase of the slave and his death

i = the rate of return on the purchase price of the slave.

Equation 3 states that the price an investor was willing to pay for a slave was equal to the discounted present-value of the average annual net income he expected to earn as a result of owning the slave. This is the standard formula for capitalizing an income stream. It is the equation used for determining the price of a long-term annuity or, with slight modification, the price of a long-term bond. Phillips was not only aware of the similarity between an investment in a slave and in a long-term security such as a bond, but he built much of his argument on that similarity.

It can be shown that the expression for the change in the rate of profit implied by equation 3 is not equation 2 but equation 4:

$$i^* = k[(\phi P_c^* - P_s^*) + \phi(Q^* - L^*) + (1 - \phi)M^*]. \tag{4}$$

All the symbols of equation 4 have been defined previously except

 $(Q^* - L^*)$  = the rate of change in the productivity of slaves—in total output (Q) divided by the number of slave workers (L)

 $M^*$  = the rate of change in the cost of slave maintenance

 $\phi$  = the ratio of the gross annual income  $(H_g)$  earned on a slave to the net annual income (H); i.e.,  $H = H_g - M$  and  $\phi = H_g/H$ 

k =a constant close to 1; its exact magnitude depends on the base-period values of i and n.

A comparison between equations 2 and 4 reveals that equation 2 is merely a special case of equation 4. Equation 4 will reduce to equation 2 when  $\phi = 1$  and  $Q^* - L^* = 0$ . Consequently the evaluation of the Phillips

<sup>&</sup>lt;sup>20</sup> Alfred H. Conrad and John R. Meyer, "The Economics of Slavery in the Ante-Bellum South," *Journal of Political Economy*, 66 (1958): 95–130.

<sup>&</sup>lt;sup>21</sup> The equation is presented in Arthur H. Gibson, *Human Economics* (London, 1909), bk. 2, chs. 6, 7. In *American Negro Slavery* (Baton Rouge, 1966), 359, Phillips cites Gibson and then states that his own discussion is "mostly in close accord with Gibson's analysis."

thesis comes down to the question of whether Phillips was justified in implicitly assuming that  $\phi=1$  and  $Q^*-L^*=0$ . For only then is information on the change in the ratio of cotton to slave prices alone sufficient to determine that profits were declining. Clearly the assumption that  $\phi=1$  is false. Since  $\phi=H_g/H$ , the condition for  $\phi$  to be equal to one is that expenditures on the maintenance of slaves (M) were zero—that slaves were being starved to death. All available evidence, including the evidence assembled by Phillips on the material conditions of slave life, refutes this assumption. Available evidence also contradicts the assumption that there was no increase in the productivity of slaves. A recent estimate based on a sample of data from the manuscript schedules of the census suggests that the rate of growth in slave productivity  $(Q^*-L^*)$  between 1850 and 1860 may have been as high as 2.9 per cent per annum.<sup>22</sup>

Thus the thesis that slavery was unprofitable, a proposition that dominated the historiography on the antebellum South for a half century, was based on a false equation. The relationship between the rate of change in profit and the rate of change in the ratio of slave to cotton prices is not well described by a linear equation with a zero intercept. Indeed, for many problems even equation 4 is inadequate. If, for example, one wanted to take account of the effects of differential advances in technology (or changes in the mix of slave-produced commodities, or shifts in the geographic locus of cotton production), a fairly elaborate model, involving several equations, would have to be employed.<sup>23</sup>

The lesson of these examples is simple. The prohibition of explicit equations will not eliminate mathematics from historiography. It will merely impede the effort to determine whether the implicit equations embedded in important arguments are true or false, whether these equations are adequate depictions of the reality with which historians are concerned.

Quantitative methods are like a plumber's tool kit. If you ask a plumber which wrench he should use on a particular pipe, he will reply, "The one that fits." For the historian the tool that fits is the one most appropriate to the historical reality that is being analyzed and that will yield the most information from the available data.

WILL THEN, THE USE of quantitative methods make history scientific? The answer to that question, in my opinion, is an unequivocal no.

Scholars who have interpreted my previous essays on methodology as

<sup>&</sup>lt;sup>22</sup> James D. Foust and Dale E. Swan, "Productivity and Profitability of Antebellum Slave Labor: A Micro-Approach," *Agricultural History*, 44 (1970): 45.

<sup>&</sup>lt;sup>23</sup> See, for example, the simultaneous-equation models employed by Claudia Dale Goldin to examine the effect of changes in the rate of change of the slave population on the distribution of slaves between rural and urban areas. "The Economics of Urban Slavery: The South, 1820 to 1860," mimeographed (Princeton, 1974), especially ch. 5.

attempts to make humanism subservient to social science in the writing of history may be perplexed by this response. It is true that I was willing to carry another banner before Stanley L. Engerman and I began to work on *Time on the Cross*. But the writing of this book has been a chastening experience for us. In the course of these labors, our views of the relationship between science and humanism in the writing of history have evolved significantly, as those who have read the successive drafts of the manuscript can testify. We have come to recognize that history is, and very likely will remain, primarily a humanistic discipline. We now believe that the issue raised by historical quantifiers is not whether history can be transformed into social science but the realm of usefulness of social-science methods in a humanistic discipline.

In both the prologue and appendix A of Time on the Cross Engerman and I emphasize that in attempting to weave the findings of the cliometricians into a wide-ranging reinterpretation of the slave economy we passed over from social science to traditional history. Appendix B is social science; volume 1 is not. Volume 1 is traditional history. It is distinguished from other traditional works on the history of slavery not because it is "more scientific" but because it draws more heavily on the findings of social science than has been true of most previous studies. The distinction between being scientific and making use of scientific findings may seem too slight to have much significance. But in my view the distinction is extremely important, and the failure to recognize that it is a crucial distinction has been at the root of much of the misunderstanding that has afflicted both sides of the debate over the role of scientific methods in history. It is this distinction between being scientific and making use of science that Engerman and I were trying to define when we said, "If we had confined our consideration of the economics of slavery purely to what can be achieved with the methods of the social sciences, this book would have been limited to appendix B, or to some more extended version of it."24

Moreover, we explicitly state that, useful as social science may be in providing certain of the elements that must be included in a historical synthesis, the synthesis itself lies beyond the range of social science.

The task which historians set for themselves cannot be achieved through social science alone. Because historians aspire to comprehend the totality of human behavior, their concerns transcend the subject matter of the social sciences and enter moral and aesthetic realms. Even with respect to those issues which fall within the scope of social science, historians frequently demand more than social science can deliver. This is certainly the case when historians attempt to combine all the elements of human behavior that concern social scientists—economic, social, political, psychological, and cultural—into a "seamless web."

Social science is incapable of producing such a seamless web. It produces, instead, particular bodies of knowledge. There is, for example, no theory which

<sup>24</sup> Fogel and Engerman, Time on the Cross, 2: 4.

encompasses all economic behavior, but only theories which deal with such particular aspects of economic behavior as income distribution, resource allocation, and economic growth. And the theories developed to analyze these problems are far from comprehensive. Economists can deal with income distribution, resource allocation, and economic growth only under certain quite specific sets of circumstances.<sup>25</sup>

Far from believing that there is an intrinsic antagonism between the humanities and social science, we hold that, when properly applied, social-science methods will enhance the scope of humanistic considerations in history. We believe, for example, that the use of social-science methods in *Time on the Cross* serves to emphasize and enlarge the moral aspects of the problem of slavery. The point is not that social-science methods can be used to resolve moral questions but that they can, in many instances, help to identify and clarify moral issues.

Moral debates are frequently based on assumptions regarding the state of economic behavior, of political behavior, and of social behavior. If, for example, it is argued that slavery was immoral because masters found it profitable to work slaves to death in seven years, then social science becomes relevant to that particular moral question. What social science has to offer is germane, not to the resolution of this moral issue, but to the economic premise that gave rise to the moral issue—the premise that slaveholders sought to, or were impelled to, maximize profits by working slaves to death in seven years. If the premise is shown to be false, the moral issue that it posed is not resolved; it simply disappears from the category of relevant moral issues pertaining to slavery.

By providing rigorous tests of assumptions made by other scholars regarding economic and social behavior under slavery, *Time on the Cross* has enhanced the relevance of some moral issues and diminished the relevance of others. In so doing, it contributes to a redefinition of the moral problem of slavery. Indeed it is our belief that by exposing various myths regarding economic and social behavior under slavery, *Time on the Cross* shows that 'the overriding questions generated by slavery were not economic and social but moral. That, by the way, is one of the considerations which led us to make *Time on the Cross* the title of our book.

I do not mean to suggest that Engerman and I limited our comments on moral questions merely to the examination of the behavioral assumptions which gave rise to these questions. In section 6.1 of appendix C we plunged directly into the debate regarding the substance of the moral problem posed by slavery. In criticizing the way in which Kenneth M. Stampp defined the moral problem, we projected our own view of the matter. But when we entered into this debate over values we did so as humanistic historians rather than as social scientists.

To what kinds of issues should quantitative methods be applied? Formal quantitative methods have their most obvious application in the analysis of the behavior of groups. Indeed, without the aid of formal statistical methods it is not possible to describe adequately the characteristics—let alone to explain their evolution over time—of such large groups as socioeconomic classes (workers, capitalists, peasants, slaves), populations of national or subnational political units (provinces, states, cities), political and social elites (legislators, revolutionary leaders, bishops, nobles), or other specific social, economic, or political categories (immigrants, members of religious orders, dissenters, taxpayers, voters).

The futility of attempts to resolve essentially statistical issues by nonstatistical procedures is well illustrated by the historiography of slavery. Neo-abolitionist and revisionist historians have been engaged in protracted debates on the material conditions of slave life. Among the points under dispute are the quality of the slave diet and the frequency of the breakup of slave marriages due to slave trading. With respect to the diet, neoabolitionists have charged that slaves were nutritionally starved, using as evidence certain statements by travelers to the South on the absence of variety in the slave diet or similar commentary found in narratives by ex-slaves.<sup>26</sup> But, of course, other travelers and other narratives indicate that the diet was quite good. These favorable instances have been cited by revisionist historians who maintain that slaves were well fed.27 It is not necessary to belabor the point that such isolated scraps of evidence, arbitrarily chosen from the mass of testimony thrown up by the controversy over slavery, cannot be used to resolve the debate over the nature of the slave diet.

The problem facing historians of the South, then, is how to characterize a distribution of diets that varied from very bad for some slaves to very good for others. This is obviously a statistical question; it is precisely the kind of question for which statistical methods were designed. Moreover the evidence needed to resolve the issue is in fact at hand. In Time on the Cross we showed how it was possible to apply the U.S. Department of Agriculture's "disappearance" method to census data in order to estimate the average consumption of ten principal foods by slaves living on large plantations (plantations of fifty or more slaves). This analysis led to the startling conclusion that on the average the nutritional content of the slave diet on such plantations exceeded minimum requirements in every one of the major nutrients.<sup>28</sup>

<sup>26</sup> See Kenneth M. Stampp, The Peculiar Institution (New York, 1956), 282-89.

<sup>&</sup>lt;sup>27</sup> See William Dosite Postell, The Health of Slaves on Southern Plantations (Baton Rouge, 1951). 31-38.

<sup>28</sup> Recent efforts to refute this finding have served to establish the robustness of the result—that is, it now appears unlikely that any plausible alternative application of the USDA procedure can reduce below minimum requirements the estimated average consumption, by slaves on large plantations, of any of the main nutrients. The original computation is described in Time on the Cross, 2: 90-99. A demonstration that the principal attack on this computation

The conclusion that the slave diet was generally quite varied—in a nutritional rather than in a gastronomic sense—also emerges from a statististical analysis of the commentaries of ex-slaves. Over a thousand of the narratives of ex-slaves collected by Federal Writers' Project of the Works Progress Administration contain characterizations of their diets under slavery. The overwhelming majority of those who made such comments indicated that their diets were good or very good. Interestingly enough, this result holds whether the person who interviewed the ex-slave was black or white. The last point is relevant since it has been argued that the WPA narratives are unreliable as a source of information because the majority of the interviewers were white.<sup>29</sup>

The foregoing indicates that quantitative methods are essential if historians are to succeed in shifting the attention of their discipline from a preoccupation with exceptional individuals to concentration on the life and times of common people. Indeed the application of quantitative methods in history has opened up the possibility that with respect to such issues as the evolution of the family, the determinants of occupational mobility, and the effect of religion on political and social behavior we may soon be able to say more about the experiences of ordinary people than of exceptional individuals. For one of the consequences of the introduction of quantitative methods has been the discovery that church records, probate records, tax rolls, and similar sources contain detailed information on a wide variety of human activities stretching far back into time.

While quantitative methods appear to be most useful as an aid in writing the history of groups, they also have bearing on the history of particular individuals. Statistical methods have, for example, been used to identify authorship—a matter that is frequently at issue in writing of the lives of prominent individuals. Quantitative methods can also be used in order to determine just how exceptional particular individuals were in aspects

leaves intact the finding that the average diet exceeded minimum requirements for all the main nutrients is contained in Robert W. Fogel and Stanley L. Engerman, "Further Evidence on the Nutritional Adequacy of the Slave Diet," paper presented to the MSSB-Rochester conference on *Time on the Cross*, University of Rochester, Oct. 23, 1974. In this paper evidence is also presented that the original computation substantially underestimated the degree of variety in the slave diet. Nevertheless it should be stressed that adequacy "on the average" does not preclude the possibility of seasonal inadequacies in one or more nutrients even for slaves who were normally well fed. Moreover investigation into the extent and determinants of variation in the diet from plantation to plantation is still at a preliminary stage. Stephen C. Crawford has found that while small slaveholdings showed more variation in the quality of the diet than large ones, the share of ex-slaves who considered their diet adequate or better was about the same on small as on large plantations. "A Note on the Relationship between Plantation Size and Diet Adequacy," in Fogel and Engerman, "Evidence Relevant to the Post-Publication Debate."

<sup>&</sup>lt;sup>25</sup> For a description of the problems involved in quantifying the data in the ex-slave narratives, as well as a report on preliminary findings, see Stephen C. Crawford, "Toward a Quantitative Analysis of the Data Contained in the W.P.A. and Fisk University Narratives of Ex-Slaves: Some Preliminary Findings," report no. 7475-1, paper presented to the Workshop in Economic History, University of Chicago, Oct. 4, 1974. See also C. Vann Woodward, "History from Slave Sources," AHR, 79 (1974): 470-81.

of their behavior or beliefs and in order to evaluate the efficacy of particular policies pursued by prominent figures. Did the fiscal and monetary policies of Franklin D. Roosevelt ameliorate or exacerbate the Depression? Did Charles Sumner exercise exceptional power in the Thirty-seventh Senate? These are questions that are amenable to quantitative analysis.<sup>30</sup>

WHAT ROLE SHOULD QUANTITATIVE METHODS occupy in the graduate training of history students? Instead of attempting to provide a general answer to this question I would like to describe the response that is under consideration at the University of Chicago.

The department of history at Chicago recently decided to initiate a systematic training program in mathematical methods for Ph.D. candidates. To facilitate the development of such a program it endorsed the establishment of a Committee on Quantitative Methods in History (CQMH). This committee, which will have degree-granting powers, will be quasi-independent of the history department but will work in close conjunction with it. The duration of CQMH has been set at ten years, with the aim of fully incorporating its activities within the history department by the termination date.

I used the term "quasi-independent" to describe the administrative relationship of CQMH to the history department. Some administrative independence seemed warranted in order to facilitate recruitment of new faculty and to provide the measure of administrative flexibility needed to expedite experimentation in new courses and in the development of training programs that may require a substantial amount of cooperation with various departments of social science and with the mathematics and statistics departments.

On the other hand, the objective of the Chicago program is not the establishment of a new discipline but the enrichment of an existing one. It is believed that the quantitative transformation of history which is possible during the next decade will, if achieved, not bring about an elimination of traditional methods. Rather it will permit quantitative methods to become a fully legitimate, an important, and a widely practiced instrument of historical research. The task at hand is the development of a program that makes education in quantitative methods *one* of the regular features of the training of Ph.D. candidates in history. Consequently it is agreed that all degrees given by CQMH during its life will be granted jointly with the history department. It is also agreed that all new faculty

<sup>30</sup> The impact of the fiscal policies of Roosevelt are evaluated in E. Cary Brown, "Fiscal Policy in the 'Thirties: A Reappraisal," American Economic Review, 46 (1956): 857-79. The monetary policies of the Roosevelt era are considered in Milton Friedman and Anna Jacobson Schwartz, A Monetary History of the United States, 1867-1960 (Princeton, 1963). Various measures of power exercised by senators in the Thirty-seventh Senate are set forth in Allan G. Bogue, "Some Dimensions of Power in the Thirty-Seventh Senate," in Aydelotte, Bogue, and Fogel, Dimensions of Quantitative Research in History, 285-318.

appointments to CQMH should require approval by the history department and be joint appointments.

The principal tasks facing the organizers of CQMH are the integration of activities that already exist at Chicago into the new program, the expansion of the faculty capable of applying mathematical methods to history, the development of a curriculum, the development of a publications program, and the determination of appropriate ways of contributing to national and international efforts to advance the application of mathematical methods to history.

The recruitment of a faculty is the most critical problem to be solved if the new program is to be a success. In this connection it is planned that new faculty positions will be built around the core of scholars already at Chicago. The temporary organizing committee includes both present members of the history department who apply quantitative methods in their research as well as social scientists in anthropology, economics, geography, political science, and sociology who have strong historical interests. But even at a major university such as Chicago, the existing faculty will have to be augmented if the objectives of the program are to be achieved. It is believed that seven new appointments will be necessary, five at the senior level and two at the junior level. One of the appointments would be used for senior visiting scholars, especially from other countries. The breakdown of five and two is not rigid. In a field in which the technology is changing rapidly, some of the most innovative scholarship will emanate from the younger men. The exact breakdown between junior, senior, and intermediate faculty will, to some extent, depend on Chicago's power to attract those they want.

There are several aspects to the problem of curriculum. One is the development of a year-long course aimed at introducing history students to the broad array of mathematical methods now being applied in historical analysis. Among the topics covered in such a course would be elements of mathematics, including set theory, probability theory, calculus, and linear algebra; an introduction to statistical methods, including statistical inference, regression analysis, and design of samples; methods of quantifying qualitative data, including content analysis; and behavioral models in economics, political science, and sociology.

Such a survey course would fulfill two needs. First, it would provide the typical history student who does not plan to specialize in quantitative history with the background required to understand and appraise historical works that employ mathematical methods. Second, it would provide the history student who wants to master one or more of the quantitative techniques with the elementary foundation needed to take the more advanced courses offered by CQMH or in one of the departments of the social sciences.

In addition to the introductory course three intermediate "tool" courses

are planned. These will be directed at students who desire not merely literacy in quantitative history but a sufficient command of behavioral models and quantitative techniques to be able to apply them in their own research. The intermediate courses will be designed to provide students with a working command of the economic, social, and political models most relevant to current historical research, without requiring them to obtain the equivalent of a Ph.D. in one or the other of the social sciences. Of course some students will go beyond this intermediate level. Advanced courses may, therefore, be introduced directly within the CQMH program. Alternatively it may be more appropriate to establish special sections of existing courses within other departments.

An additional ten to fifteen courses would be introduced that aim to educate students in the *art* of applying mathematical methods in history. Students enrolled in such courses would normally be required to have previous training in the basic techniques, at least at the intermediate level. These courses would focus on the substance of historical problems, emphasizing the ways in which behavioral models and quantitative techniques can be employed to obtain evidence and advance analysis of issues that would otherwise be intractable or at least more difficult to resolve.

In one respect such courses would appear to be similar to traditional history courses, for they would be organized largely by time and geographic region. They would differ, however, by focusing on those substantive subjects—including the social history of the family, black history, urban history, collective biography, parliamentary history, and the history of popular voting behavior—where quantitative methods have been, or give promise of being, most productive.

Instructors of these courses will be scholars whose distinction rests not so much in their command of technical matters per se as in their ability to use these techniques to illuminate history. Several scholars at Chicago have already achieved distinction for their ingenuity in applying quantitative methods to a wide array of historical issues. Adding to this core of instructors will be the central objective in recruiting new faculty.

Still another area of concern of CQMH is the promotion of interuniversity and international programs to service the needs of the new approach. An important way that Chicago might contribute to interuniversity needs is through the establishment of a permanent summer training institute on mathematical methods in history. Such a program would be integrated into Chicago's regular summer session but directed primarily toward Ph.D. candidates at other institutions, to new Ph.D.'s who want training in this area, and to older scholars who want to be brought up to date on new developments.<sup>31</sup>

<sup>31</sup> Chicago is, of course, not the only university considering the institution of a major new program for quantitative methods in history. Last April, Dean Henry Rosovsky initiated discussions on the advisability of establishing a standing committee on quantitative methods in history in the Faculty of Arts and Sciences at Harvard University. The history department at

What, finally, can be done to overcome the problem in communication created by the intrusion of explicit quantitative methods into historical literature? The problem of communication among professional historians is more tractable than the problem of communication between historians and the reading public. As I have tried to indicate in the answer to the previous question, I believe it is possible to design a course that will provide nonquantitative historians with a sufficient command of the mathematical methods now employed in history to permit them to understand and assess quantitative studies. If the work of quantitative historians becomes sufficiently compelling, such a course will no doubt become a standard part of the training for the Ph.D. in history.

But such a relatively easy solution will not bridge the gap between quantitative historians and the very large public that reads history. Even if we assume that future generations of high-school graduates will have better training in mathematics than current generations, it will be a long time before typical college freshmen enrolled in the American history survey course can cope with the mathematical and statistical models that are now commonly employed in economic history and increasingly employed in social and political history. The typical parents of these freshmen, moreover, will never have an adequate mathematical background.

Consequently unless the problem in communication is addressed vigorously, we face the danger that history will be transformed into an esoteric subject that is directly accessible only to rigorously trained professionals. That, of course, is one possible route of development, which, if realized, would require a corps of intermediaries or popularizers who could explain to the lay public the findings of professional historians. This is the line of development that has taken place in the physical sciences.

A better solution, in my opinion, is for quantifiers themselves to assume the burden of translating quantitative history into ordinary language. As I have argued, quantification will not transform history into a science but merely expand the store of scientifically validated knowledge on which historians can draw. For the foreseeable future history will be compounded out of a mixture of systematic evidence subject to rigorous statistical tests and informal or fragmentary evidence. The exclusion of one or the other type of evidence will impoverish rather than enrich history. Specific human examples, fragmentary as they may be, are often needed to interpret and give meaning to tables and diagrams that summarize systematic evidence. And statistical methods are needed to tell us which human examples are typical and which are atypical.

The desideratum is a generation of scholars trained in the ability to make use of all of the categories of evidence on which historians must

the University of Rochester is planning to hold a series of summer institutes on quantitative and econometric methods as a prelude to making training in these methods an integral part of the preparation for the Ph.D. degree.

## Quantitative History

## A Review Article by CHARLOTTE ERICKSON

WILLIAM O. AYDELOTTE. Quantification in History. (Addison-Wesley Series in History.) Reading, Mass.: Addison-Wesley Publishing Company. 1971. Pp. x, 179. \$2.50.

LEE BENSON. Toward the Scientific Study of History: Selected Essays. Philadelphia: J. B. Lippincott Company. 1972. Pp. xi, 352. Cloth \$8.75, paper \$3.95.

WILLIAM O. AYDELOTTE et al. The Dimensions of Quantitative Research in History. Edited by WILLIAM O. AYDELOTTE, ALLAN G. BOGUE, and ROBERT W. FOGEL. (Quantitative Studies in History.) [Princeton:] Princeton University Press. 1972. Pp. ix, 435. \$12.50.

VAL R. LORWIN and JACOB M. PRICE, editors. The Dimensions of the Past: Materials, Problems, and Opportunities for Quantitative Work in History. (Essays presented to the American Historical Association's Committee on Quantitative Data.) New Haven: Yale University Press. 1972. Pp. vi, 568. \$17.50.

E. A. WRIGLEY, editor. Nineteenth-Century Society: Essays in the Use of Quantitative Methods for the Study of Social Data. (Publication of the Cambridge Group for the History of Population and Social Structure.) New York: Cambridge University Press. 1972. Pp. vii, 448. \$27.50.

None of these collections of articles and essays is a guide to the mathematics of quantitative history. Not more than three of the articles are written in equations. Non-numerate members of the profession will not often find themselves at a loss to understand what is being said. This is just as well, because most of the authors exhibit a missionary aim, namely, to persuade historians, other than those in economic or demographic history, to employ quantitative methods. The missionary appeal is also directed toward possible donors of research grants, since many of these projects required more resources for research assistants and computer time

<sup>&</sup>lt;sup>1</sup> For an excellent survey of recent quantitative work, which includes judgments on guides to how to do it, see Robert P. Swierenga, "Computers and American History: The Impact of the 'New' Generation," *Journal of American History*, 60 (1974): 1045-70.

than historians have been wont to use. In addition, in differing degrees, these volumes survey and criticize sources for numbers from the past, delineate the kinds of problems susceptible to measurement and statistical analysis, point out some of the difficulties that are encountered in classifying and manipulating quantitative data, as well as present some examples of varieties of quantitative history.

Professor Aydelotte has been for two decades an advocate and practitioner of quantitative methods. Quantification in History gathers together four articles written since 1954, prefaced by a new introduction and concluded by the publication of six letters exchanged with Professor J. H. Hexter between 1967 and 1970. The most substantial of these articles is "The Problem of Historical Generalization," reprinted from Generalization in the Writing of History: A Report of the Committee on Historical Analysis of the Social Science Research Council (1963), edited by Louis Gottschalk. Most of Aydelotte's pleas for more quantification and the formulation of testable hypotheses in historical writing are sweetly reasonable and moderate, and his claims as to their application modest.

Lee Benson is neither so vague nor so moderate. This collection of his essays includes his important and stimulating "Research Problems in American Political Historiography" (1957)2 in which he demonstrated, with a few simple figures about voting patterns, that some specific generalizations which had graced substantial historical works about the elections of 1824, 1860, 1884, and 1896 could be overturned by stating clearly the implied hypotheses and testing them by analyzing geographical distributions of votes, voting patterns over time, and rates of shifts in voter behavior. Benson also enjoined historians to examine interest groups other than economic ones in the study of group political behavior and indicated a method of measuring voting from this point of view. It is not, perhaps, too much to say that Benson lifted the historical study of interest groups and voting behavior from the ditch in which Beard and his critics had left it. The impetus led Benson himself to complete his brilliant Concept of Jacksonian Democracy in 1961. There is no need to particularize to make the point that the historical study of voter behavior is today a flourishing field of scholarship.

The other essays in the volume indicate a shift in Benson's view of the aims of history. In the 1950s he laid emphasis upon the historical context, on Marc Bloch's assertion that "a historical phenomenon can never be understood apart from its moment in time." Benson then defined the historian's goal as "to uncover and illuminate the motives of human beings acting in particular situations, and, thus, help men to understand themselves." His attitude toward methods of investigation was eclectic and

<sup>&</sup>lt;sup>2</sup> Published originally in Mirra Komarovsky, ed., Common Frontiers of the Social Sciences (Glencoe, 1057), 118-89.

<sup>(</sup>Glencoe, 1957), 113-83.

3 Marc Bloch, The Historian's Craft, tr. Peter Putnam (New York, 1953), 35, cited in Benson, Toward the Scientific Study of History, 50.

pragmatic. "That is, if historians explore the possibilities of simultaneously employing traditional impressionistic methodology and systematic quantitative techniques in attacking the complex problems involved in understanding man's past, they are likely to come closer to their goals and improve both types of methodology to boot." Historians are even justified in proceeding without theory.

From 1961 onward Benson appears to have become increasingly committed to a view of history as a social science and of scientific history as the foundation of all other social sciences. The potential of modern computer technology and cooperative scholarship seems finally to open the door to the positivist goals, exemplified by H. T. Buckle, of discovering universal laws of human behavior verified by empirical data. In "Quantification, Scientific History, and Scholarly Innovation" Benson hailed as ushering in the new science the inauguration in 1963 by the American Historical Association of an ad hoc committee, of which he was chairman, to collect the basic quantitative data of American political history, together with the great decentralized "working confederation of political researchers" in the Inter-University Consortium for Political Research established at Ann Arbor in 1962. Nevertheless as late as "Middle Period Historiography," published in 1971, he was still conceding that there were different kinds of historical writing which might continue to coexist.<sup>5</sup>

In "Explanations of American Civil War Causation: A Critical Assessment and a Modest Proposal to Reorient and Reorganize the Social Sciences," published for the first time in this volume, Professor Benson seems to have lost his moorings. Again he takes on four historians—G. Barrington Moore, Eugene Genovese, David Donald, and Eric Foner. His brilliant and learned dissection of their interpretations is more polemical than his work of seventeen years ago. He dismisses them as "irredeemably erroneous" for faulty classification of phenomena, not for mistaken implicit quantification. He has little to offer instead except another classification. Writing of a serious "methodological gap" in the study of mass behavior, he advocates large sustained efforts to find new methods. His typology on the causes of the Civil War is not very encouraging as to the direction this search is taking.6

But no matter. It is the system that is at fault, not the researchers.

Such research, I trust, some day will be undertaken when adequate resources exist to develop credible explanations of Civil War causation. But for that happier state of affairs to come about the existing historiographic system must be overthrown and replaced. . . . The job of credibly explaining Civil War

<sup>4</sup> Benson, Toward the Scientific Study of History, 75; see also 80, 82, 90.

<sup>&</sup>lt;sup>5</sup> Ibid., 103, 192, 195. Benson's "Quantification, Scientific History, and Scholarly Innovation" was originally published in AHA Newsletter, June 1966, pp. 11-16; "Middle Period Historiography: What Is To Be Done?" was originally published in George Athan Billias and Gerald N. Grob, eds., American History: Retrospect and Prospect (New York, 1971), 154-90.

<sup>6</sup> Benson, Toward the Scientific Study of History, 334-40.

causation is not beyond the powers of man. But I think it is beyond the powers of men burdened by the fetters of the present historiographic and social science systems.<sup>7</sup>

The man who so perceptively criticizes Moore and Foner for reification objects to the system that "permits" Moore's explanation to be taken seriously. He who objected to the ahistorical explanations of Frederick Jackson Turner and Achille Loria now condemns the Founding Fathers for having created a "defective" constitution that led to a powerful presidency and an irresponsible party system.8 How thoroughly he could demolish others who indulged in such ahistorical hindsighting! His "modest proposal" for understanding the Civil War consists not of testable hypotheses and usable methods but in the setting up of a billion-dollar institute, funded for twenty years and completely independent of existing academic institutions. By leaving "its directors . . . free to decide who they wish to have participate in its work" it would avoid the atomistic liberalism of the present establishment. This institute should study intrasocietal violence on the assumption that "no such thing exists as an innate human instinct for aggression, hostility or violence."9 If Benson does not submit a convincing case for a new scientific history, it should be noted that none of this has anything to do with quantitative methods in history.

AYDELOTTE AND BENSON were both associated with the production of two of the other volumes that bear such similar titles. Aydelotte was one of the editors of *The Dimensions of Quantitative Research in History*, and he contributed one of the papers. Benson presented a paper at the meetings in Harvard in 1966, when early drafts of these papers were delivered and criticized. That meeting, a subsequent one in 1969, and publication of the volume were sponsored by the Committee of the Mathematical Social Science Board, established in 1964 under the aegis of the Center for Advanced Study in the Behavioral Sciences. This volume emphasizes what has been and can be done with quantitative methods. The editors hope "to clear the air and to indicate, more precisely than would be feasible by other means, both the possibilities and the limitations of quantitative methods." <sup>10</sup>

The essays are concerned with French, British, and American history and range in date from the sixteenth to the twentieth centuries. They indicate a variety of historical problems that might be approached in part

<sup>7</sup> Ibid., 271, 316n.

<sup>8 &</sup>quot;Given the nature of American society in the late eighteenth century and its reasonably predictable evolution in the nineteenth, the governmental system created by the new constitution was almost certain to, and did actually, produce" those features Benson faults. *Ibid.*, 318-19. 9 *Ibid.*, 327, italics in original.

<sup>10</sup> Aydelotte, Bogue, and Fogel, introd., Dimensions of Quantitative Research in History, 3.

by quantitative methods and of statistical tools available for those prepared to use them in a discriminating and appropriate manner. The questions tackled are by no means trivial (as is sometimes suggested) as a quick tally will show. The first three papers deal with aspects of social mobility. Lawrence Stone and Jeanne Stone report some preliminary findings in a larger study of the entry of new men into the top landed class in Britain from the early sixteenth century to 1879. Gilbert Shapiro and Philip Dawson examine whether access to opportunity, as measured by the numbers of ennobling offices in a particular place, or frustration from an inability to secure elevation produced the more radical complaints in the cahiers de doleance in 1789. The chapter by Stephan Thernstrom, drawn from his study of social mobility in Boston since 1880, 11 is concerned with the relationship between religious belief and mobility. Charles Tilly's contribution continues an already well-developed study of crowds in France. The contributions on political behavior also fall within a well-developed field. Professor Aydelotte again uses scale analysis to test the issues, other than the Corn Laws, that may have divided British Conservatives in the 1840s. An analysis of a few indicators of voter behavior in twentiethcentury congressional elections is afforded by Gerald Kramer and Susan Lepper. Allan Bogue's experiments with a number of different indexes to locate the men who wielded power in the Thirty-seventh Senate (1860) contain the most original applications of statistical methods. The much neglected subject of urban expenditure in small towns (population 10,000-25,000) is analyzed for the year 1903 by J. Rogers Hollingsworth and Ellen Jane Hollingsworth. The final essay, by Robert Fogel and Jack Rutner, addressed to the question of whether federal land policy in the late nineteenth century directed too much capital and labor into agriculture or inhibited development by encouraging speculators to withhold land from use, is the only one in economic history. Demographic studies are not represented.

A considerable range of statistical techniques and models has been employed by the contributors. At least two of the papers are of special interest for the source materials used. The Stones are investigating country houses above a defined size to locate the top landed class; indeed their paper is about the construction and remodeling of those houses as a first stage in the analysis of their owners. Newspapers have yielded the basic incidents on which Charles Tilly has constructed his index of collective violence in France, though material about the composition of the participants on both sides, their aims, and the background to the larger incidents he quantifies comes from other sources as well. The introduction to the volume places each essay in its historiographic context with a few firm and masterly strokes.

<sup>11</sup> Stephan Thernstrom, The Other Bostonians: Poverty and Progress in the American Metropolis, 1880-1970 (Cambridge, Mass., 1973).

THE VOLUME EDITED by Val Lorwin and Jacob Price was commissioned by the American Historical Association under the guidance of the Committee on Quantitative Data in History, which was reorganized and expanded in 1967 to include scholars outside the field of United States history. The committee faced an information gap when it began to consider the prospects for quantitative work by American historians whose main scholarly interest lay outside the United States. Two conferences were held during the winter of 1967 to hear reports about the present state of quantitative history and the sources for it in many parts of the world. Not all the surveys have been included in this published volume, but some attempt was made to get contributors to write to a common brief, including a survey of sources and the provision of preliminary bibliography, a review of quantitative work already produced on the country or group of countries covered, and proposals for priorities should resources become available to assist the extension of quantitative history in that field. The following areas appear in this volume: medieval Europe; Britain from 1650 to 1830; France since 1789; Germany in the nineteenth and twentieth centuries; the Nordic countries since the late seventeenth century; five centuries of Spanish history; Russia from the sixteenth to nineteenth centuries and in the Soviet period; Latin America in colonial times and in the nineteenth and twentieth centuries; Japan since 1600; and India since 1500.

As might be expected with such a vast initial undertaking, the various writers have adhered unevenly to the brief. The most useful bibliographies of both authorities and sources have been provided by David Herlihy on the Middle Ages, Charles Tilly and Louise A. Tilly on France, Arcadius Kahan for Russia, and John J. Te Paske for colonial Latin America. A few writers have buried bibliographical references in notes at the end of their respective chapters, where they serve as neither accessible notes nor bibliography. No bibliographies have been attempted for India and Japan, but the article by Kozo Yamamura and Susan B. Hanley on Japan strikes one as a very good introduction, from firsthand knowledge, to a variety of source materials, together with hints from experience on how to approach the vast bodies of data that apparently survive from the Tokugawa period (1600–1867). Morris David Morris's account of Indian materials is largely secondhand. Kahan mentions Soviet source materials, such as family budgets, that not even Soviet academicians have been able to examine.

The surveys of historical literature cover monographs in the native languages as well as English, except in the case of the article on India. The essays on France, Spain, and colonial Latin America are quite outstanding, both for their clear presentation of the views and methods of succeeding generations of scholars and for their introduction to the institutions generating statistical information. Birgitta Oden's contribution on Scandinavia is critical of the sources but not of authorities. No essay on authori-

ties is provided for Russian history. Where, as in France and Germany, quantitative work is by no means new, Charles Tilly and James Sheehan deplore the flatness of statistical methods of past and living scholars. In spite of the wealth of source material, Japanese scholars are reported to have hesitated to conduct quantitative research. All the essays discuss demographic sources. Those on Japan, Latin America, India, and Russia are limited to materials for economic history. The article on Germany stresses material for studies of political behavior and elites. It is the only essay that mentions quantitative studies of businessmen.

The writers on Japan, India, Spanish America, and the Middle Ages are sanguine about the usability of vast troves of material not as yet subjected to quantitative analysis, or indeed used hitherto by historians at all. In contrast, David Landes, Louise Tilly, and Birgitta Oden are somewhat doubtful that much more can be squeezed out of the sources themselves for the early modern "proto-statistical" period in France, Britain, and Scandinavia. Were the sixteenth and seventeenth centuries less statistical dark ages in Asia than in Western Europe? It remains to be seen. Almost every writer emphasizes the importance of regional materials, including those who feel that dead ends have been reached as to aggregates for some eras.

Recommendations differ as well, partly depending on the state of knowledge of the sources in the area covered. Most contributors want surveys and bibliographies of the sources of the statistical raw materials. The feasibility of data banks like that at Michigan is in everyone's mind. Aydelotte's contribution consists of a suggestion for a British data bank for political behavioral studies. Quite a few of the essayists are keen on more conferences. It is difficult to imagine that many countries in the world will fund, on the lavish basis required, their historians and social scientists interested in historical studies. Yet the AHA and its committee must be congratulated for their flexible initiative in starting this kind of inventory. One is inclined to compare this outward-looking scholarly thrust with that of the AHA in its second and third decades, when European archives were combed for materials relating to American history. Then, too, historians were somewhat defensive in the face of self-confident social scientists. J. Franklin Jameson wondered what subsequent generations of historians would make of the mountains of source material uncovered in his day. When Turner suggested that Jameson turn to the writing of economic history, the latter demurred with considerable insight into the nature of unexploited source materials for social and economic history.

I have always thought it much more difficult to document, with any sense of security, the social and economic history of the U.S. than the political or constitutional. You do not have definitely limited bodies of materials, handed down

<sup>12</sup> See F. J. Fisher, "Sixteenth and Seventeenth Centuries: The Dark Ages in English Economic History?" Economica, n.s. 24 (1957): 2-18.

by authority, like statutes or other manageable series, but a vast lot of miscellaneous material from which the historian picks out what he wants, and so the effort to document must often be a process of selection, always open to the suspicion of being a biased selection, or one made to sustain a set of views.

Elsewhere he referred to the documentary materials for social and economic history as "voluminous masses of low-grade ore, from which to get a little gold." The first editor of the AHR saw the problem with which recent proliferation of finer statistical tools and the computer are beginning to offer some assistance.

The volume edited by E. A. Wrigley of the Cambridge Group for the History of Population and Social Structure in England consists of detailed, technical essays designed to assist the researcher tackling the British decennial census volumes or the census manuscripts for 1841, 1851, and 1861. The 1871 manuscripts have been opened since the book was prepared. The only general methodological paper, by Roger Schofield, is an introduction to "Sampling in Historical Research." Articles by Michael Drake, Michael Anderson, Peter Tillot, and Alan Armstrong deal with the scope and inaccuracies of the census and recommend procedures for using the enumerators' books. Papers about methods for using census material in studies of the family by Michael Anderson, of net overseas emigration from Britain by Dudley Baines, and of education by B. I. Coleman, as well as a survey of the statistics of crime by V. A. C. Gatrell and T. B. Hadden, attest to the quantitative work in social history currently being undertaken in Britain where sophisticated statistical procedures are more in evidence than they are among British economic historians, many of whom feel a discreet hesitation to place a weight of sophisticated models on raw material probably inferior to that available for the American economy in the nineteenth century. In social history the quantitative study of demographic and social structure is attracting resources, but the study of social mobility in the nineteenth century has barely begun, if indeed the study of the anonymous can ever proceed very far in the absence of fuller local directories.

TAKEN TOGETHER, the volumes under review, which can now rightly be described as only the tip of the iceberg, in Britain and the United States at any rate, provide evidence not only of considerable achievement but also of a number of areas of research in which historians are deeply engaged, wrestling with problems of the trustworthiness of the data, the best means of handling it to test their ideas, and the ambiguities of the tests applied.

<sup>13</sup> J. Franklin Jameson, The American Historian's Raw Materials (Ann Arbor, 1923), 45; Jameson to Frederick Jackson Turner, Nov. 25, 1927, and Jameson to M. W. Jernegan, Mar. 13, 1931, both in Jameson, An Historian's World: Selections from the Correspondence of John Franklin Jameson, ed. Elizabeth Donnan and Leo F. Stock (Philadelphia, 1956), 327, 340.

As Charles Tilly writes, he would like to "consider us all converts. There the work begins." 14

Unless things have changed very much since these books were published, the quantifiers appear to be still on the defensive, vis-à-vis both other historians and the social scientists. Aydelotte refers to discussion that "has occasionally been acrimonious" and to the "current offensive against quantification." The editors of *The Dimensions of Quantitative History* show a wistful candor about the difficulties of communication at the conferences in which traditional historians and statisticians were invited to discuss the essays in quantitative history. "A historian who tries to bridge this gap is sometimes left dangling in between and has trouble in making effective contact with specialists in either direction, with exactly the two professional groups who should, properly, be able to help him most." <sup>16</sup>

The problems of communication, if not the acrimony, are likely to persist until university graduates are as well versed in mathematics as nineteenth-century students were in Latin. The strident tone of some of the early missionaries of this generation of quantifiers put people's backs up as much as did Charles Beard in his day, though now clearly with less disastrous results for the type of history being advocated. Lee Benson's book gives plenty of instances of somewhat gratuitous insults to the whole historical establishment. But even the more diplomatic Professor Aydelotte aims his shafts rather indiscriminately at historians-in-general.<sup>17</sup>

Linked with the early tone, which seemed to discredit antecedents rather than acknowledge debts to them, were the claims of "newness"—in both methods and results. Take but two examples from the field of economic history. British economic historians at first thought that the brash young Americans were trying to teach their grandmothers to suck eggs. Sir John Clapham had long since demonstrated that counting where possible could clear up some rather important matters. So also, when Alfred H. Conrad and John R. Meyer first wrote about the profitability of slavery, they did not emphasize the fact that some historians had already questioned U. B. Phillips's view that it survived in spite of its unprofitability. An emphasis on the novelty of current scholarship can make teaching more interesting for the teacher as well as stimulating to the student. I well remember the intellectual excitement in the classroom when the new Keynsian economics was taught at Cornell when I was a graduate student. But I suggest that it is neither scholarly nor truly scientific in spirit to play this game in academic literature, and it may indeed rob students of

<sup>&</sup>lt;sup>14</sup> Charles Tilly, "Quantification in History, As Seen from France," in Lorwin and Price, Dimensions of the Past, 95.

<sup>15</sup> Aydelotte, Quantification in History, 35, 60.

<sup>&</sup>lt;sup>16</sup> Aydelotte, Bogue, and Fogel, introd., Dimensions of Quantitative Research in History, 7-8. See also Aydelotte, Quantification in History, 169-70.

<sup>17</sup> Benson, Toward the Scientific Study of History, 240, 271, 279, 287, 305; Aydelotte, Quantification in History, 15-17.

insight into the way in which scholars build upon each other's work in extending knowledge even with the most exciting new techniques. The volumes on "dimensions" indicate that in this respect the new economic and quantitative history have come of age. The editors note that James Malin used the census-enumerator books in the early thirties for mobility studies, that Orin Grant Libby recognized in 1896 the "possible value of an intensive study of roll calls," that Crane Brinton and Donald Greer were quantifying groups in French revolutionary studies in the thirties, and so forth.

Some of the resistance has no doubt arisen because historians were unequipped to undertake the new sorts of quantitative studies themselves or even to understand any longer, say, the pages of the Journal of Economic History. Professor Hexter seems to have taken the view in his interchanges with the "deaf" quantifiers that it was impossible to write both mathematics and English, that quantification would destroy history as literature. One does not have to agree that historians have a monopoly of good style to regret that Charles Tilly did not translate his large computer categories, such as "Not Acting Collectively before Disturbance," or "Collective Action Not Clearly Connected with Disturbance," into the more specific instances that were coded only to be absorbed in the larger categories. And Allan Bogue, whose work I admire enormously, perpetrates the following sentences:

A centroid factor analysis shows that two factors account for most of the variance in the table of correlation. The committee power index loaded more heavily than any other variable on the second of those extracted. Clockwise orthogonal rotation to maximize this loading produced the loadings shown in Table 2. The impression of clustering derived from the correlations is reinforced.<sup>18</sup>

It is illuminating to notice that the surveys of quantification in Germany, France, Scandinavia, and Japan revealed no such resistance to quantification as had been stirred up in America. The writers of these surveys also assume that even in America the battle has long since been won in economic and demographic history. The basis on which quantification has been absorbed in some fields and some countries can be distinguished. One-dimensional descriptive statistics are not new. Two-dimensional figures drawn from the sources, such as time series, raise no objections. Even when one moves on to index numbers, correlations, and regression analysis, so long as they are based upon numbers drawn from historical sources or samples drawn from them, and even when surrogates such as wholesale prices are used for retail prices in a cost-of-living index, one can expect little resistance from people working on problems for which such tech-

<sup>18</sup> Charles Tilly, "How Protest Modernized in France, 1845–1855," in Aydelotte, Bogue, and Fogel, Dimensions of Quantitative Research in History, 222, 224; Bogue, "Some Dimensions of Power in the Thirty-Seventh Senate," in ibid., 307.

niques are helpful. William McGreevey refers to the "European style" of quantification, which "builds the corpus of available data, but neither the utility nor the direction in which such data are augmented is determinate, since there are few hypotheses to direct the work." Actually Aydelotte argues for no more than counting or measuring when possible, a position that should require no defense.

Resistance to testing formal models has come not only from ignorance but also from suspicion of the synthetic figures extrapolated, guessed, and sometimes borrowed from another period of time or another place to fulfill the requirements of the model.<sup>20</sup> On similar grounds counterfactual hypothesizing has been somewhat less than welcomed, particularly as some of the early uses made of such models projected the alternative worlds for a half century or more.21 As David Landes writes, the political arithmeticians of seventeenth-century Britain "had recourse from the start to estimated means, population estimates and multipliers in order to arrive at aggregate figures. . . . The trouble with Petty's work was that he wanted to make a point—a characteristic affliction of all social scientists—and that his method gave him too much free rein."22 In his article on Spanish sources Juan J. Linz points to some contemporary estimates, by just such political arithmeticians that influenced contemporaries and that historians use at their risk.28 Thus synthetic figures are not new, though as they are refined with more attention to the historical context and available raw data, as well as to the requirements of models, it is hoped that historians will show less resistance to this kind of quantification.

The writers in these volumes are willing to concede most of the points that have been raised by the critics of quantification "American style." Some practitioners have used jargon or been otherwise unintelligible or clumsy in their writing. It is agreed that the traditional historical method of source criticism should be applied to all data used in quantitative studies. The problems that can be tackled quantitatively are limited in scope, and some exercises have indeed been trivial. Apart from Lee Benson, who

<sup>10</sup> William McGreevey, "Quantitative Research in Latin American History of the Nineteenth and Twentieth Centuries," in Lorwin and Price, Dimensions of the Past, 485.

<sup>20</sup> Ibid.; Charles Tilly, "Quantification in History, As Seen from France," in ibid., 110–11, 125; Louise A. Tilly, "Materials of the Quantitative History of France since 1789," in ibid., 138–39; Benson, Toward the Scientific Study of History, 116–17. See also E. H. Hunt, "The New Economic History: Professor Fogel's Study of American Railroads," History, 53 (1968): 3–18.

<sup>21</sup> Robert Fogel, Railroads and American Economic Growth: Essays in Econometric History (Baltimore, 1964); John R. Meyer, "An Input-Output Approach to Evaluating British Industrial Production in the Late Nineteenth Century," in Alfred H. Conrad and John R. Meyer, eds., Studies in Econometric History (London, 1965), 183-213.

<sup>&</sup>lt;sup>22</sup> David Landes, "Statistics As a Source for the History of Economic Development in Western Europe: The Protostatistical Era," in Lorwin and Price, Dimensions of the Past, 55.

<sup>&</sup>lt;sup>23</sup> Juan J. Linz, "Five Centuries of Spanish History: Quantification and Comparison," in ibid., 233.

seems to be chasing a nineteenth-century will-o'-the-wisp, they make no claims to finality or certainty. They warn against spurious precision.<sup>24</sup>

Above all, the weight of opinion in these volumes favors mixed methods of historical research, not an excessive reliance on quantification. "It is absurd, however, to contend that there is any exclusive road to knowledge, that intellectual rigor can be achieved only by the use of figures, or that the value of research depends on the kinds of techniques used rather than on the intelligence with which they are applied."25 In The Dimensions of the Past are cited a number of instances of quantitative estimates, based upon historical sources, that have not gained credence because they are at odds with "qualitative" knowledge.26 Michael Anderson emphasizes that quantitative work on the British census is of no value without an extensive use of contemporary descriptive sources that suggest questions to be asked of the sample data. The strength of Allan Bogue's essay on power in the Thirty-seventh Senate lies precisely in the diversity of means by which he has approached the problem. Where the personal papers, memoirs, and memorial addresses in Congress do not support unambiguously the quantitative identification of Senators Doolittle and Clark as powerful figures, he concedes that they remain somewhat enigmatic.27

Not all the writers in The Dimensions of Quantitative History have so successfully practiced what they preached. At the risk of appearing to niggle in the face of arduous labors that have added to our knowledge, I would suggest that Charles Tilly, Aydelotte, and Thernstrom have not succeeded in mixing methods but present mainly an account of the gathering and manipulation of their quantitative data.28 It is a bit disturbing to have Professor Aydelotte tell us that one must follow a policy of "rigid exclusion" of all facts, however interesting, that cannot be tabulated or on which insufficient information is available, though he does save the day by conceding that such facts might be referred to in the text.29 Perhaps the feature that most worries the lone historian about large quantitative

<sup>&</sup>lt;sup>24</sup> Aydelotte, Quantification in History, 3, 27, 29, 30, 34–35, 47–48, 55, 59, 80, 83, 94, 95, 151, 175; Aydelotte, Bogue, and Fogel, introd., Dimensions of Quantitative Research in History, 9, 10; Michael Anderson, "The Study of Family Structure," in Wrigley, Nineteenth-Gentury Society, 52. <sup>25</sup> Aydelotte, Quantification in History, 14, see also pp. 36-37, 56, 175; and Landes, "Statistics As a Source," in Lorwin and Price, Dimensions of the Past, 65.

<sup>26</sup> See Phyllis Deane's first estimates of British national income in the eighteenth century and the controversy over J. C. Toutain's estimates for French agriculture in the eighteenth century, discussed by Landes, "Statistics As a Source," in Lorwin and Price, Dimensions of the Past, 58-59, 73-74; see also Landes's criticism of François Crouzet's use of French trade statistics for the eighteenth century in ibid., 64. Also see the "rumbles in the rice fields" controversy about Japanese agricultural output, in Kozo Yamamura and Susan B. Hanley, "Quantitative Data for Japanese Economic History," in *ibid.*, 511-13.

27 Bogue, "Some Dimensions of Power in the Thirty-Seventh Senate," in Aydelotte, Bogue,

and Fogel, Dimensions of Quantitative Research in History, 310-11.

<sup>28</sup> My criticism of Gerald Kramer's and Susan Lepper's study of congressional voting would be more severe, since they have removed the time dimension from their regressions. "Congressional Elections," in ibid., 256-84.

<sup>29</sup> Aydelotte, Quantification in History, 146.

studies that have attracted generous funds is the difficulty any other historian has in checking the reliability of the data, especially when the counts have been generated for the first time, either totals or samples, in the course of the research. It is not an answer to say that other samples from other times and places will confirm or reject the findings. That quantifiers are themselves conscious of the problem is evident in their references to the "trained readers" they employ and their careful instructions to assistants, in their acceptance of a fair amount of error, and in the caution shown about what should be fed into data banks. If "the rawer the better" is a good motto for choice of statistics, how does one tag the data to include the bias and inappropriateness of sources and the policies and aims of institutions and persons generating the raw data? These problems arise as soon as one moves out of the firm ground of votes in elections and legislative assemblies.<sup>30</sup>

Such doubts by no means inhibit one from accepting the modest and reasoned claims of the quantifiers considered here as to the value of such work for historians. In certain fields such as economic and demographic history and in the study of large social groups in the past the effort to quantify is essential. Statistical methods make it possible to handle large masses of data otherwise closed to the historian and to test explanations within the limits of probability. Even the most skeptical of the "old" economic historians will now recognize the spin-off in clearer analysis and greater rigor of research, though these ends were not innovations of the new wave of quantifiers. Some of the most valuable work has been destructive—for example, Thernstrom's finding that residential segregation does not explain differential rates of social mobility in Boston, or Shapiro's and Dawson's retreat to a near rejection of Tocqueville's hypothesis that opportunities for ennoblement provided an adequate explanation for attitudes of the French bourgeoisie, or the Hollingsworths' demonstration that explanations of levels of urban expenditure on an aggregate basis look very different when examined region by region. In the end the worth of scholarship depends on the integrity and insight and knowledge of the scholar. Controversy, even personal rivalries, have not proved obstacles to the extension of knowledge.

Insofar as these controversies are about substantive issues, the center of the storm is not so much whether or not to quantify but about the nature of historical knowledge and inquiry. The rift still concerns that relationship between history and the social sciences that began with their births as separate disciplines.<sup>31</sup> Quantification has gotten mixed up with the underlying differences in outlook because the new methods came in

<sup>&</sup>lt;sup>30</sup> On means of surmounting this problem in the future, see Swierenga, "Computers and American History," 1066.

<sup>&</sup>lt;sup>31</sup> For a view quite different from Lee Benson's present position, and one that I find more congenial, see H. Stuart Hughes, "The Historian and the Social Scientist," AHR, 66 (1960–61): 20–46.

by way of the social sciences. Lee Benson's typology is helpful here. He lists four sorts of history: history as literature or entertainment; history as "identity," the study of the past to answer individual or group needs; history as philosophy, "to help liberate men from parochial outlooks and give them the widest possible range of choice of 'values to live by'"; and history as social science, "to contribute to the overall scientific study of human behaviour"—not simply to advise decision makers, but to give human beings knowledge to make rational choices.<sup>32</sup>

While there may be pragmatic reasons for identifying history as a social science and the will to do so may reflect another period of loss of confidence among historians, there is still a difference between history and the social sciences in the nature of the inquiry, a difference that explains the controversy and the difficulties of communication. The social scientist is searching with an instrumental purpose for general, overarching laws about mankind. He is looking at the past for regularities and similarities and is willing to raid it for materials to test theoretical models drawn in the present. In contrast, the aim of many historians is to understand the past in its own terms as well as ours. If there is an instrumental purpose, it is more indirect, as in Benson's definition of the philosophical historian. As has so often been said, the historian focuses on the uniqueness of past time and place and human behavior, the historical context. He seeks to understand what differentiates us and our problems from those of our predecessors.

Louise A. Tilly pinpoints the difference in view when she discusses the "grand debate" in French economic history—"a division between those who are mainly interested in borrowing procedures from the social sciences in order to understand French history better and those who are more concerned to test general sociological hypotheses by the case of France."<sup>33</sup> Professor Aydelotte reveals his own social-science orientation when he confesses, "The issues of British politics of a century and a quarter ago are now dead, and I can muster little enthusiasm for them. On the other hand, the votes and debates on these questions can be used to shed light on certain theoretical problems, the nature of political attitudes and why men hold them."<sup>34</sup>

This is not to say that individuals may not enter upon historical inquiries by way of the social sciences or that historians may not become social scientists. The two sorts of inquiry cannot really be elided, and it would be

<sup>32</sup> Benson, Toward the Scientific Study of History, 199.

<sup>33</sup> Louise A. Tilly, "Materials of the Quantitative History of France since 1789," in Lorwin and Price, Dimensions of the Past, 139.

<sup>&</sup>lt;sup>34</sup> Aydelotte, Quantification in History, 160. See also Aydelotte, Bogue, and Fogel, introd., Dimensions of Quantitative Research in History, 45, and the statement by Kramer and Lepper that they study elections in the past "since they may help to explain the political dominance of particular groups in society, the electoral benefits to an incumbent of pursuing a particular policy, or the political consequences of proposed or possible changes in political structures and procedures." "Congressional Elections," in ibid., 256.

impoverishing if one or the other were to disappear. Historians will want to extend themselves and their knowledge of the present by reading social-science theory, but they are likely to remain selective and eclectic in dealing with men and motives in other times and circumstances. In turn, the social scientist will use historical works as well as historical sources. For good social science, one needs historical scholars who do try to understand the historical context, who have been trained in the methods of critical use of sources, even if they are no more than common sense, to check the inferences from the data and sometimes even to criticize the guesses of an impatient social scientist who has not been seduced by the allurements of Clio.

## Reviews of Books

## **GENERAL**

GÉZA ALFÖLDY et al., editors. Probleme der Geschichtswissenschaft. (Geschichte und Gesellschaft: Bochumer historische Studien.) Düsseldorf: Pädagogischer Verlag Swann. 1973. Pp. 176.

Generous editorship and a vague title made it possible to reunite in this collection nine papers by Bochum historians, greatly differing in content and quality. Seemingly, the editors aimed at the existential problems of history to be discussed by representatives from different fields and more than half the papers fall into this category. Thus, Rudolf Vierhaus provides a characterization of history in general, emphasizing "structures" as the main objects of elucidation and the main factors of continuity, linking the past with the present and revealing the latter's historicity. Géza Alföldy justifies ancient history in three detailed statements: (1) its structures can indeed be perceived, despite the often fragmentary character of its sources; (2) it is an integral and necessary part of general history, both by reason of the structures it elucidates in which Western civilization originated and by the outstanding methodological experiences and contributions it offers; (3) it is useful and relevant to modern society in that it raises consciousness and critical perception by providing depth perspective and "alternative models" for comparison. Ferdinand Seibt adduces similar terms to demonstrate the relevance of medieval history, pointing out in addition how a widespread, if often unconscious, popular interest in history contradicts the "crisis of history" in schools and universities. Hans Mommsen's Betrachtungen zur Entwicklung der neuzeitlichen Historiographie in der Bundesrepublik describes the motives, failures, and achievements of German historians from 1945 to 1973 as well as their clashes about the character and use of historical studies. His fascinating account of how a country, steeped in history and philosophy and open to all modern intellectual and methodological currents, tries to come to grips with one of the greatest historical shocks experienced by any nation is certainly the most valuable essay of this collection and of particular interest to American historians for comparative purposes.

Other contributions include an introductory lecture in prehistory, an outline of research done on early medieval aristocracy, a study on the relation between autocracy and anarchy, remarks on the history of science and economy in the eighteenth century, and a summary of trends in twentieth-century German economic history.

Most of the papers have in common a healthy emphasis on Strukturen, occasionally expanded to more dubious Gesetzmässigkeiten reminiscent of Marxism. The leftist student rebellion that swept through the history departments in Germany has left its marks: the call for increased emphasis on socioeconomic factors is ubiquitous. The fact that it is not monotonous leaves the field open to a lively, ongoing discussion.

MICHAEL P. SPEIDEL University of Hawaii

EMMANUEL LE ROY LADURIE. Le territoire de l'historien. (Bibliothèque des histoires.) [Paris:] Gallimard. 1973. Pp. 542.

This collection of twenty-nine pieces by the author of Les paysans de Languedoc (1966), among other works, includes book reviews for newspapers such as Le Monde, chapters from collaborative volumes, an encyclopedia entry, reports read at professional meetings, and articles from various periodicals—the Annales, of which he is a director, naturally being prominent among them. We are thus presented with little

General 367

that is not already in print (the conference papers being the exception), but we are given the chance conveniently to survey both the range and the several principal themes of Professor Le Roy Ladurie's work over roughly the past decade.

In his foreword he stakes out these themes or, as he calls them, "provinces" of his historical territory: first, the "material, sociological, cultural history of rural civilizations" in modern times; second, "serial and quantitative history," statistically based and applied, for example, to demography; third, "interdisciplinary zones," where history can draw upon the methods and materials of fields as diverse as ethnography, economics, physiology, meteorology, even glaciology. Throughout he stresses his debt to the computer, though he sensibly observes that "in history, as elsewhere, what counts is not the machine, but the problem" (p. 11).

It is impossible in a short review to do justice to the breadth of historical interest revealed by the present volume. Having identified his organizing themes, the author sets off energetically to pursue each through a variety of case studies. The longest entry is a fifty-page analysis of official statistics for French army recruits (1819-26), an elaborate recording of physical, occupational, and other data, with relatively little interpretation attached. Among the shortest, at almost the opposite end of the quantitative scale, are three reviews of books on sorcery and witchcraft by a Frenchman, an American, and a Spanish Basque, respectively. From the population of early modern Paris to the search, by radioactivity tests, for silver from Potosí in French and Spanish royal coinage, from agricultural productivity to climatology, our lively guide takes us backward and forward, in time, space, and choice of subject matter.

Of all the spokesmen for the Sixth Section of the École Pratique des Hautes Études, the Parisian fortress and fire base for quantitative historians, Professor Le Roy Ladurie has long struck me as one of the most effective. In no sense a bigot, for all his zeal, he remains immensely good humored, supple, and fresh in his intellectual approach. He generally tries to get, and give, as much as he can from his data, in terms of conclusions at once significant and reasonable. And he does not just write about how his methodology might be used—he uses it. Not everyone will rely as heavily on those methods, but no one can any longer safely ignore them.

FRANKLIN L. FORD Harvard University

ARTHUR RAISTRICK. Industrial Archaeology: An Historical Survey. London: Eyre Methuen; distrib. by Barnes and Noble, New York. 1972. Pp. xiii, 314. \$18.75.

With the publication of this volume industrial archeology joins oral history, psychohistory, and quantitative methods as another research tool available to the historian. The author believes industrial archeology has emerged, over the last two decades, as a discipline primarily because of the work done by Michael Rix and Kenneth Hudson. Now Raistrick has given us a comprehensive survey of our past and present understanding of this subject, shown how it differs from social and economic history and the history of technology, and laid out in masterful detail the kinds of raw materials used in the processes and structures of past industry, as well as the varieties of evidence that remain for study.

The author has three biases that he does not hesitate to identify. First, he holds that earlier studies of industrial archeology, frequently to the point of excluding consideration of other eras and topics, emphasized the time period from the beginnings of the Industrial Revolution to the present and stressed machinery and mechanical power. Raistrick convincingly argues that there is a compelling need to record and preserve "the monuments" of industry from prehistoric and Roman times to the present. Second, because he lives and works in Yorkshire, many of his examples and illustrations, accumulated during a half-century as an engineer and university teacher in geology, are intentionally drawn from the North of England. He believes that ample evidences of coal and lead mining, textiles, transportation systems, iron and steel, and chemicals are found there. Third, Raistrick wonders whether the study of the machines, processes, and structures of nineteenth- and twentieth-century industry properly falls within the range of the industrial archeologist. Perhaps the extent of responsibility in these instances is to record their existence and over-all appearance. In the final analysis, however, the author perceives the role of industrial archeology as not merely to record remains "but to place industry in its proper environment and perspective as a continuing and developing theme in human endeavour."

The findings of the industrial archeologist are part of a people's national possession, cultural background, and evidence of their industrial progress. Hence, means must be available to display the results in regional, rural, company,

and industrial museums where the best techniques of preservation, restoration, and recording can be employed.

Illustrations, drawings, footnotes, and a bibliography adequately complement the readable text.

ROBERT E. CARLSON
West Chester State College

MARCELLE KOOY, editor. Studies in Economics and Economic History: Essays in Honour of Professor H. M. Robertson. Durham, N.C.: Duke University Press. 1972. Pp. xi, 313. \$13.75.

The Festschrift has become an institution that requires examination. It is not good enough to put together a set of papers linked by nothing more than the fact that all the contributors have a desire to contribute to a particular celebration. A little over a third of this book deals with South Africa's history and its future: "Aspects of Economic Development in South Africa" by C. W. G. Schumann; "A Revisit with the Cape's Hottentot Ordinance of 1828" by Leslie Clement Duly; "The Evolution of Monetary Policy in South Africa" by Brian Kantor; and "South Africa's Salvation in Classic Liberalism" by W. H. Hutt. The rest of the book is concerned with a wide range of economic and historical problems that I could deal with only on the level of clarity and plausibility: "Population and Potential Power" by J. J. Spengler; "Considerations of some Aspects of the Rise of Capitalist Enterprise" by Frederigo Melis; "Considerations on the Industrial Revolution" by Amintore Fanfani (the latter two are both marred by allusiveness and by prose that is not easy to understand—the editor would well have exercised more control here); "Retardative Factors in French Economic Growth at the end of the Ancien Régime and during the French Revolutionary and Napoleonic Periods" by Shepard B. Clough; "Fluctuations and Growth in the 19th Century" by H. J. Habakkuk; and finally "Note on Secret Price Cutting in Oligopoly" by B. S. Yamey.

To return to the essays on South African history: Schumann's essay is difficult to assess because there is no clear statement of purpose. The article is divided into periods—"Pre Union," 1910–45, 1946–69—and ends with a discussion of "The Future." The latter section seems to be written on limited assumptions, particularly that of sustaining an immigration rate of 30,000 a year and that, given noninterference from outside, "the present quite remarkable state of comparative social and political

stability . . . can be maintained" (p. 22). Duly's essay-economics or economic history?-is a valuable corrective of the view that an ordinance passed is an ordinance applied. He writes: "For the Khoikhoi, the weakness in the colony's governmental system, the indifference . . . of officials, and the absence of an active role by London . . . merely emphasised the tremendous importance of the informal processes that were influencing Khoikhoi-European relations" (p. 46). Mr. Kantor's article on monetary policy soon becomes unintelligible because he assumes a knowledge of monetary theory on the part of the reader and includes long extracts from official reports that are not properly edited of explained. Finally, W. H. Hutt's article is neither economics nor economic history but a political sermon. Mr. Hutt has a complex scheme for the solution of South Africa's ills, based to some extent on a highly idiosyncratic reading of the "lessons of the history" of the United States in the past ten years or so. Starting from the unpopular but legitimate proposition that "the fears of the presently dominant Whites . . . are reasonable, realistic and genuine" (p. 111) he rapidly moves into an area of abstract blueprinting, including qualified franchise and the election of the president from and by the "Judiciary" (p. 113). Mr. Hutt has good points to make on the probabilities of increased tension once concessions begin to be made to those who have been discriminated against, but most of his prescriptions are remote from present or probable political contexts.

It is a pity that so distinguished a teacher and scholar as Robertson was not honored by a set of essays that either expanded on his own work or were at a level of generality to catch the interest of scholars in the social sciences. Included in the volume is a list of Robertson's works listed by year, most of them articles written since he came to South Africa in 1930. His historical work included a frequently cited pair of articles," 150 Years of Economic Contact between White and Black"; others were on land tenure at the Cape, systematic colonization, the historical evolution of South African wage levels, and settlers in Natal. In European history his works include Aspects of the Rise of Economic Individualism (1933), "The Adam Smith Tradition" (his inaugural lecture, 1950), "Marx, Menger, Mercantilism and Max Weber," and "Researches in Italian Economic History of the Period of the Risorgimento." On South African problems he published a stimulating short book, South Africa, Economic and Political Aspects (1957), as well as many articles on contemporary problems including those of the economics and history of the Second World War. One of the functions of a Festschrift ought to be to inform readers of the total corpus of an author's work, especially articles undeservedly forgotten. In this respect this works succeeds admirably, reminding us of the wide range of interest and steady contribution of a noted South African academic.

J. E. BUTLER
Wesleyan University

EDUARD VAN DEN BRINK. Rooms of hatholieh: De opvattingen van Christopher Dawson over kultuur en religie. (Academisch proefschrift, Vrije Universiteit te Amsterdam.) Groningen: V. R. B. Offsetdrukkerij. 1970. Pp. 275.

Dr. van den Brink claims that Dawson was a great historian and compares him to Arnold Toynbee. But Pieter Geyl was not the only one to think that Toynbee's broad world views were dwaasheid, folly. Dawson was a prolific writer and a very good one; and this happens to be the third dissertation about him. Does this make him a great historian? Dawson took positions rather than doing primary research. And his positions-down with Byzantium, caesaropapism, Luther, communism, and the city; up with St. Thomas, papal monarchy, the Christian Renaissance, Mussolini (says van den Brink), and Western Civilization courses in the freshman year at American universities—are those of forty years ago. His work has aged badly in comparison with that of Gilson or Bloch, for example. Many of the themes happened to be wrong, and writing to any system does not seem to work any longer. Perhaps it has not worked since Anselm's day.

Dr. van den Brink, in this worthy and painstaking study, asks whether Dawson was a Roman or a Catholic, that is, whether he was narrowly sectarian or more broadly Christian in outlook. He concludes that the man was both. Perhaps it would have been useful to point out that the Church is now doing all it can to escape from the prison of Europe in general, and that of the thirteenth century in particular. It would also seem likely that if the present attempts to build a European community are to be successful, the economic and scientific themes that Dawson neglected will be at least as important as his cultural ones.

STEPHEN B. BAXTER
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Chapel Hill

C. J. CUMING and DEREK BAKER, editors. Popular Belief and Practice: Papers Read at the Ninth Summer Meeting and the Tenth Winter Meeting of the Ecclesiastical History Society. (Studies in Church History, 8.) New York: Cambridge University Press. 1972. Pp. xii, 330. \$19.50.

This is a collection of twenty-six different essays by members of the British Ecclesiastical History Society. All have some relationship to what is described as popular religion within the Christian context. The essays are arranged chronologically with the first ones dealing with late Roman and early medieval topics, subsequent ones dealing with late medieval and Reformation topics, and the remaining half of the book dealing with early modern and modern topics. All of the authors wrote quite independently and for the most part they speak as specialists in some particular era of history. They have chosen topics in their field that pertain to popular religion. Some of the topics are extremely quaint. It is the rapid sequence of brief but specialized discussions that makes this a charming and fascinating book.

One must be somewhat arbitrary in citing particular items. For the medieval period there is a notable presentation by Marjorie E. Reeves of pictorial prophecies about the medieval popes (plates reproduce examples that are somewhat reminiscent of tarot cards). In the middle portion of the book the essay on Protestant spirituality by Gordon Rupp is one of the most substantial. Dr. Rupp writes with the depth and clarity of one who has devoted decades of research and reflection to his topic. One need notshare his admiration of the reformers in order to be impressed and edified by what he has to say. For the modern period, the longest essay is W. R. Ward's presidential address d∈aling with developments in popular Methodism in the early nineteenth century. This essay does presuppose a rather technical knowledge of Methodism. The information communicated by this study is enhanced, however, by the fact that the essays immediately preceding and following touch on somewhat related matters. All of the essays are in English except one by A. Latreille, who provides an interesting discussion in French on the historiographical problems of evaluating popular religiosity in nineteenth-century France. The volume concludes in a most striking manner with a discussion by Stuart Mews of ecclesiastical attitudes toward black athletes in England and America during the twentieth century, especially as these related to Jack Johnson, the first black world-champion boxer. This exceptional essay provides a notable example of how

the disciplines of ecclesiastical history can be fruitfully applied to significant contemporary questions.

Having considered what this book is, something should also be said about what it is not. First of all it is not in any sense a systematic history of popular movements or popular developments within Christianity. Some of the essays only deal with popular religion in a most extended sense of the phrase. None of the essays deal with the kind of cults discussed, for instance, in V. Lanternari's The Religions of the Oppressed (1963). Nor do the authors allude to the study of Christian "people movements" as carried out by D. A. McGavran and his students. Considering that the greatest number of essays deal with England it is surprising that there is no discussion of the feasts and fasts, the prayers and carols, or the beliefs and superstitions of that majority of poor and humbler people nominally belonging to the Church of England. It remains, however, a very interesting volume.

H. BOONE PORTER, JR.
Roanridge
Kansas City, Missouri

ROGER BASTIDE. African Civilisations in the New World. Translated from the French by PETER GREEN. With a foreword by GEOFFREY PARRINDER. (Torchbook Library Edition.) New York: Harper and Row. 1971. Pp. vi, 232. \$12.50.

This short but stimulating study concerns African cultural survivals among blacks in the Americas. Roger Bastide, a professor of ethnology at the Sorbonne, employs both comparative and historical methods to present a wealth of factual material and analyses concerning the enduring aspects of African civilizations in the New World. His work is firmly based on French, English, Spanish, Portuguese, and other language sources, and it is obvious that he has great knowledge and understanding of the cultures of blacks in the Americas, especially of their religions, and also of the African background. It is indeed surprising that Bastide's books were not translated into English earlier since he is one of the leading cultural anthropologists concerned with African civilizations in the Americas.

Bastide concludes that religion is the most spectacular African survival among blacks in the Americas although folklore is the most prevalent. On the other hand, the economic infrastructure is the weakest survival because it is the

most vulnerable to environmental factors. His discussion ranges from a consideration of isolated, self-sufficient communities where African civilization still survives almost intact to the syncretism and amalgamation of a variety of cultural traditions by blacks in the New World. African religious survivals provide material for the major discussion in the book and Bastide's clear and objective chapter on voodoo, which divests it of the clichés common in most writings on the subject, is a major contribution of his work. As Bastide points out, religious survivals are largely due to the ethnic associations that were permitted blacks even under slavery and that still exist today in large cities of Latin America. Because the religions of Africans from the Sudan and Guinea coast area were generally more systematized and complex, they endured better than those of the Bantu people taken as slaves from the Congo and Angola regions. Paradoxically, most African folklore survivals are of Bantu origin.

Bastide's careful research and rigorous analysis are unfortunately marred somewhat by occasional lapses of scholarship. An example is his sweeping generalization that "Africans have a passion for titles" (p. 29), which recalls the hackneyed and unscientific statements about German proclivity for militarism, Chinese inscrutability, etc. Bastide also does himself and his discipline a disservice when he gives support to the myth of the black man's sexual superiority by repeating the saying that Indian women in the New World gave themselves to Indian men out of duty, to Europeans for money, but to Negroes for pleasure (p. 74).

The merits of this study, however, far outweigh these shortcomings, and its contribution will be greatly appreciated by Africanists, Afro-Americanists, and all those concerned about the cause of racial understanding.

WILLIAM L. BOWERS Bradley University

M. R. D. FOOT. War and Society: Historical Essays in Honour and Memory of J. R. Western, 1928–1971. New York: Barnes and Noble. 1973. Pp. xiv, 349. \$19.50.

This Festschrift is dedicated to a brilliant young English historian whose main interest was the study of war and its impact on society; hence the title of this volume.

In a touching tribute the editor describes the personality and accomplishments of John Randle Western, who studied at Oxford and Edinburgh and taught at Manchester. The essays, General 371

contributed by his friends and colleagues, are arranged in chronological order and their topics cover a period of almost two thousand years. Three papers deal with the European, Asian, and domestic aspects of World War II and five treat World War I and its aftermath—the Irish Republican Army, the Liquor Control Board, Britain's relationship with India, South Africa in the war, and the impact of the war on Britain's political system. Three essays deal with nineteenth-century problems: conscription in Europe, the mid-Victorian army, and the naval militia. Other studies in the volume discuss military problems during the French Revolution, and the problem of revolution and revolt in early modern England, ancient Greece, and ancient Rome.

A volume containing studies as diverse as these is difficult to evaluate. Both subject matter and approach vary a great deal, which makes for fascinating reading but blurs the focus—a common weakness of Festschriften.

The essays of most interest to me were R. A. C. Parker's "The British Government and the Outbreak of the War with Germany 1939," and V. G. Kiernan, "Conscription and Society in Europe before the Great War." The former focuses on the often repeated charge that the two-day delay of the British declaration of war, following Germany's invasion of Poland, was but another indication of Chamberlain's appeasement policy. The author, relying heavily on hitherto unpublished sources, shows that it was French reluctance to fight Hitler, especially Bonnet's, coupled with the British cabinet's desire for solidarity with France, that produced the delay.

Mr. Kiernan's study deals with European conscription and its impact on society, from Spain to Russia. He sees a link between discipline in the armies and discipline in the factories and believes that once the bourgeoisie had grasped this connection, their fears of arming and training the masses were allayed.

Altogether this is a worthwhile collection and a fitting tribute to a dedicated teacher of history.

GEORGE O. KENT University of Maryland, College Park

RAM LAKHAN SHUKLA. Britain, India and the Turkish Empire, 1853–1882. New Delhi: People's Publishing House. 1973. Pp. xi, 262. Rs. 30.

As a detailed examination of Britain's policies toward the Turkish Empire in the mid-nineteenth century, Professor Shukla's work proposes to elucidate the motivation behind British policy. It also seeks to examine the origins of pan-Islamic sentiments among Indian Muslims, specifically, their sympathy for the Ottoman sultan-caliph, which flowered into an anti-British political movement in the early 1920s. Unfortunately the work does not succeed in either aim.

One must commend Professor Shukla for his careful gleaning of the British records of the period, Foreign Office correspondence, private papers, and records of the Foreign Department of the government of India. His detailed narrative, however, does not make up for the lack of analysis of the material. While he carefully runs through the differing opinions of various principal officials, Salisbury, Lytton, Ripon, et al., there is very little attempt to analyze how these differences were resolved, in terms of what priorities. Was British policy toward Turkey dictated more by considerations of the European balance of power or by Indian considerations? The latter seems clearly indicated, yet Shukla, beyond recording differences between the government of India and the home government, does not go beneath the surface or clarify differing motivations.

The most interesting chapter of the book (ch. 6) is the one concerning British use of the caliphate. In it Shukla rather skillfully shows how the British played upon Indian Muslim sympathies for the caliph of Islam to further their own purposes. Unfortunately there are inconsistencies here. He implies that the pan-Islamic movement was all the Britishers' doing, but he has shown in a previous chapter that pro-Turkish sympathies already existed among Indian Muslims. There is much to be said for the argument that the British promoted pan-Islam, which later turned against them. But if it is a point worth making, it is worth making well. The difficulty is in treating Indian Muslim opinion as monolithic. The British records make that mistake, but Shukla, as an Indian, should not. In the mid-nineteenth century there were Indian Muslims who were pro-Turkish, others who were not. Further, those who looked with sympathy toward the caliph did so for a variety

Shukla has failed because he relies too heavily on British records. The only place where Indian opinion is referred to, other than through British eyes, is in the appendix, where he summarizes the native newspaper reports (British abstracts of the vernacular press). He

should have included this source within the body of his study.

GAIL MINAULT
University of Texas,
Austin

R. V. SAMPSON. The Discovery of Peace. New York: Pantheon Books. 1973. Pp. xxiv, 205. \$6.95.

It is tempting to dismiss Sampson's polemic in the service of peace—a peculiar combination of scholarship and sermon—as a dated, simplistic, moralizing tract. Using an interesting (though ultimately unsuccessful) mixture of intellectual history, social psychology, and political theory, Sampson wants his audience to share his inner conversion that the only true path to peace, a path illuminated by Tolstoy and a few others, is the refusal of peoples to do evil or be violent. The achievement of peace depends solely on a general application of the gospel truth, "resist not evil, return good for evil" (p. xix). Liberal internationalists who worked for peace through law or organization were deluded; they ignored the common-sense truth that peace is a condition achieved by being peaceful. It was Tolstoy, for Sampson, whose vision penetrated the errors erected into orthodoxy and the truths consigned to indifference by those in power.

To achieve the self-control of the nonviolent personality, Sampson urges us to consider the nature of power, a subject he explored in his earlier book, Psychology of Power (1966). Defined as the urge of some men to control others, power varies little over time and clime in its dependency on coercion and violence; thus, it is morally illegitimate. In the final analysis, however, power is only effective because of popular acquiescence. Tolstoy understood that not even the authority and charisma of a Napoleon would have moved the wholesale slaughters introducing modern history without the willingness of followers. Fighting stopped when people stopped following or became exhausted. Tolstoy's War and Peace, characterized as "the death knell not just of kings but of power itself . . . a profoundly subversive book . . . much and deeply feared" (p. 125), is offered as the first great statement of modern consciousness on human liberty and survival.

The middle bulk of Sampson's book purports to explore the sources of Tolstoy's intellectual inspiration. De Maistre, Stendhal, Herzen, and Proudhon each receive a chapter summarizing their main ideas, but the total effect is awkward and superficial. Instead, the well-known

shortcomings of intellectual history in illustrating "influence" are glaringly revealed.

Most astonishing from a twentieth-century author is a discussion of power that totally ignores the nexus between economics and politics. The Discovery of Peace dramatizes the weaknesses of the psychological obsession overtaking serious social analysis. A methodological elitism and idealism emerge to obscure the author's courageous foray into the most serious issue facing humanity.

As a study of Tolstoy's intellectual background, this work jells badly. As a guide to correct political and moral habits, it will only convince those already in church.

SANDI E. COOPER
Richmond College,
City University of New York

CHARLES P. KINDLEBERGER. The World in Depression, 1929–1939. (History of the World Economy in the Twentieth Century, volume 4.) Berkeley and Los Angeles: University of California Press. 1973. Pp. 336. Cloth \$10.00, paper \$3.45.

Having slain a host of unicausal dragons in his Economic Growth in France and Britain, 1851-1950 (1964), Charles Kindleberger is on a quest to destroy another. This time it is the turn of Milton Friedman and his famous view of the world depression as being born and bred in the United States alone and specifically in our failure to maintain an optimum supply of money. A better explanation, says Kindleberger, is that the international economy, severely troubled by the overhang of war debts and reparations and by worsening dissymmetries in trade, was deprived of the leadership it needed to cope with a crisis (the repercussions of the New York crash). He defines "leadership" as an ability "to set standards of conduct for other countries; and to seek to get others to follow them, to take on an undue share of the burdens of the system, and in particular to take on its support in adversity by accepting its redundant commodities, maintaining a flow of investment capital and discounting its paper." Only Britain and the United States could perform such functions. But after 1929 Britain could not; and the United States would not, because of the mishmash of counsel offered the president and the stultifying role of the "congressional rabble."

The central interest of this book, however, is not its main thesis or the attacks on Friedman, Samuelson, Svennilson, Arthur Lewis, and others; it is rather the highly useful discussion General 373

of the main financial and monetary events. After a running start in the 1920s, Kindleberger takes us through the depression decade year by year, dealing with investment, trade, money and security markets, and gold movements in the United States and Western Europe. The book is crammed with judicious insights concerning the motivations of the principal actors, insights based on memoirs as well as the works of economic historians. This is a work by an economist for historians, and historians will benefit greatly. There is not a single equation in the book, and the charts and tables are accessible enough for the most nonquantitative among us.

Social aspects of the depression have no place in this work; Kindleberger notes the omission, with regrets, in the preface. He pays little attention to the era's increasing concern for social welfare. Makers of national policy, however, were forced increasingly to turn their attention to domestic ("forgotten man") problems, and their commitment to international financial "leadership" was bound to weaken. Kindleberger's avoidance of social history may explain his painfully inadequate treatment of the Popular Front governments in France. Perhaps these topics will be treated in later volumes of the series, History of the World Economy in the Twentieth Century; Kindleberger's book, labeled volume 4, is the first to appear.

MARTIN WOLFE
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DAVID W. WAINHOUSE, with the assistance of FREDERICK P. BOHANNON et al. International Peaceheeping at the Crossroads: National Support—Experience and Prospects. Baltimore: Johns Hopkins University Press, in cooperation with the Washington Center of Foreign Policy Studies, Johns Hopkins University. 1973. Pp. xi, 634. \$22.50.

The charter of the United Nations conferred on the Security Council the primary responsibility for the maintenance of international peace and security and provided that an international armed force for its use would be created later under separate agreements. These agreements were never effected and when the first occasion arose when international peace was presumably endangered, the United Nations began a series of ad hoc arrangements that were referred to as peace observation, peace supervision, or peace-keeping forces. None of these forces was armed, save for self-defense, and none engaged in enforcement measures.

Mr. Wainhouse has prepared eight case studies of these activities under the United Nations, two case studies under the authority of the Organization of American States, and one under the Arab League. In each instance the case study provides a brief historical background; an explanation of the organization and management of the peacekeeping force; the contributions of the various national states in personnel, logistics, and finance; a few relevant key documents; and a conclusion. Special attention is given to the contributions of the United States, and four chapters at the end of the study consider general problems of peacekeeping as they were manifest in the several case histories.

It is quite clear that each of the peacekeeping efforts was unique in the circumstances of its initiation, the factors of consent of the parties involved in the conflict, and the considerations that must enter into value judgments concerning success or failure. In the major conflicts in the Middle East, the United Nations was content to secure an end to hostilities but unwilling to provide the means to enforce its resolutions for a permanent settlement. And in West New Guinea, where for a time United Nations forces were in control of the disputed territory, a "peaceful solution" of the conflict between Indonesia and Holland was considered more important than the principles of justice and democracy for the people of West Irian. In all the instances of peacekeeping under the United Nations there was some constitutional conflict of authority between the Security Council, the General Assembly, and the office of the secretary general. Very often the mandate given to the secretary general was ambiguous because no clear directive could be agreed upon and he was obliged to improvise as best he could.

No brief review could do justice to the value of this study for it is a mine of information concerning peacekeeping experiences in the cases examined. It provides extensive statistics, maps and charts, observations on the attitudes and policies concerning peacekeeping of the various national states, references to documents and other sources of information, and constructive suggestions for future peacekeeping efforts.

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ANDREW W. CORDIER and WILDER FOOTE, editors. Public Papers of the Secretaries-General of the United Nations. Volume 3, Dag Hammarshjöld, 1956–1957. New York: Columbia University Press. 1973. Pp. xv, 729. \$22.50.

The third volume of the Public Papers covers the period of the Suez and Hungarian crises of 1956—the period of the United Nations' greatest activity. The successful handling of the Suez crisis demonstrated how the UN could best be used. After the British and French vetoed action by the Security Council, the problem was transferred to a Special Session of the General Assembly under the "Uniting for Peace" resolution, which had been adopted in 1950 to circumvent the Soviet veto as in the Korean case. The General Assembly gave Dag Hammarskjold almost blanket authority to establish a cease fire, create UNEF, arrange for the withdrawal of British, French, and Israeli troops, and undertake the task of reopening the Suez Canal. Recognizing and seizing the opportunities provided by the basically similar American and Soviet stands on the main issues, Hammarskjold became the behind-the-scenes leader and manager of the UN's efforts.

Unlike the Korean War of 1950–53, where the so-called UN force was in reality an allied force under U.S. command, UNEF was the first truly international force with the secretary general exercising actual command under the rather general and imprecise mandate of the General Assembly. The Congo crisis of 1960-62 also led to the establishment of a UN force under the effective command of the secretary general, but it differed fundamentally from the Suez crisis in that the Soviet Union and the United States soon disagreed in their views and policies about the UN and of the secretary general's role and operations. In the Suez crisis, however, the USSR supported or went along with the UN's objectives and the secretary general's activities as long as these were consistent with Soviet policies in the area. In the Suez crisis the prestige and authority of the UN reached its apogee. As the editors of the book point out, the two months of November and December 1956 were the most innovative and fruitful in the world organization's history.

Although there are only hints of it in the volume, this period was also a watershed for Great Britain and France. The complete failure of their policies and of their efforts to take control of the canal by force marked the beginning of their decline in influence in the postwar world. The withdrawal of their armed forces was cloaked and facilitated by their being able to present it as a compliance with the will of the world community rather than as an ignominious defeat.

The papers on the creation and operation of the first UNEF in 1956 may be usefully compared with those of the second UNEF in 1973. Although Lester Pearson, the Canadian secre-

tary of state for foreign affairs was the one who first conceived the idea of a UN emergency force made up of small and middle powers, Dag Hammarskjold was its chief architect, builder, and manager, and he operated with a minimum of guidance from either the General Assembly or the Security Council, The documents clarify that the questions of the duration of UNEF and of its withdrawal were never settled but that Hammarskjold recognized that it could continue to operate in Egypt only with the consent of the host country. In case that consent was withdrawn, he would refer the matter to the advisory committee he had created with the General Assembly's assent. Since 1973, however, the Security Council, under the insistence of the USSR, has maintained tight control over the entire operation, including the continued operation of UNEF, which has been given a sixmonth renewable mandate. In addition, the secretary general has undertaken to obtain advance clearance from the Security Council for every policy decision. The United States and USSR are working much more closely in the Middle East crisis now than they did in 1956-57; they have learned and are avoiding the bitter lessons of the Congo crisis, where Soviet opposition put the whole future of UN peacekeeping and its financing in jeopardy and which resulted in the tragic death of Dag Hammarskjold.

But while the UN was establishing a remarkable record of achievement in Suez, it was at the same time creating a sorry spectacle of failure in Hungary. It is true that it was the powerful and committed Soviet Union, rather than the distracted and wavering Britain and France, that sent massive armed forces into Hungary and was the defendant in the dock; but the failure of both the United States and the secretary general as well as of the membership as a whole to take any effective action, or even to make any really serious attempt to do so, raises many questions to which the present volume provides no answers. Historians can argue for years about the reasons for the total ineffectiveness of the UN in Hungary, which contrasted so vividly with its "finest hour" in Suez. As Hammarskjold remarked at a later time, none of the member states, and in particular the great powers, was willing to take any stronger action in the Hungarian case. It is noteworthy that only some fifty pages of the volume are devoted to Hungary as compared with some three hundred pages to Suez.

The volume also contains the texts of statements and reports by the secretary general on

Ancient

the manifold activities of the organization, such as economic and social problems, atomic energy, and human rights. One of its most valuable features is its documentation of the exposition and evolution of Dag Hammarskjold's views of the charter and of the roles of the organization and of the secretary general in support of the charter provisions.

Unquestionably, however, the most interesting part of the book, at least to this reviewer, who was a minor participant in those exciting days, is the lucid and penetrating description by the editors of the factual historical developments that are illuminated by the secretary general's papers. Their commentary on and analysis of these developments are indispensable to an understanding of the complex activities and work of the United Nations.

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## ANCIENT

v. M. MASSON and v. I. SARIANIDI. Central Asia: Turkmenia before the Achaemenids. Translated and edited with a preface by RUTH TRINGHAM. (Ancient Peoples and Places, volume 79.) New York: Praeger Publishers. 1972. Pp. 219. \$12.50.

There are few countries that take their archeology more seriously than the Soviet Union. Many sites have been excavated and much information recovered in recent years, but Soviet archeology has remained a closed book to Western prehistorians, both because so few of them read Russian and because many of the reports are published in severely limited editions that do not reach the West. Thus, a book such as the one under review is a welcome addition to the literature, regardless of its merits. That this happens to be a good book makes it even more welcome.

Starting with a brief introduction Masson and Sarianidi recapitulate the evidence for the various periods of Central Asian prehistory. There are chapters on the Paleolithic, Mesolithic, and Neolithic (Djeitun) periods, two chapters on the Chalcolithic, three on the Bronze Age, and one on the Early Iron Age. The material within the chapters is usually arranged in a paradigmatic manner. First the sites excavated and the resultant finds are presented. It is only then that the authors proceed to interpretive statements, generally without obvious overlays of ideological content. They are strongest when they deal with internal Central Asian matters. When they

search for parallels in the literature and when they refer to Western scholarship in the Near East much of their information is dated, and they seem unfamiliar with the work done in recent years in Near Eastern prehistory. Large distances are bridged by tenuous parallels, and modern anthropological theory seems to be largely ignored.

375

There are a number of other shortcomings in the book. The maps are inadequate, and there are not enough of them. The plates are clear, but they lack a scale, so that their usefulness is impaired. Finally, a chart or set of charts setting out the various periods and sites would have served as a useful guide to the reader unfamiliar with this area.

The translation by Tringham is readable and usually clear. If one can offer criticism here, it is that non-Russian names were not checked against the standard usage in the West, and would be somewhat confusing for the uninitiated. Thus Chesmi Ali is rendered as Chasma Ali, Susa as Suza, etc. Finally, D. E. McCown comes out as J. McCowan. But these are minor points in what is a very useful book. One hopes it is the first in a long series of works by Soviet scholars that will be available to scholars and students working in the West.

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RALPH S. SOLECKI. Shanidar: The First Flower People. New York: Alfred A. Knopf. 1971. Pp. xv, 290, x. \$8.95.

Nine Neanderthal skeletons were found during the excavations of Shanidar Cave in northeastern Iraq. These discoveries alone suffice to make this one of the more important Paleolithic excavations of modern times.

Though Shanidar is important for other reasons as well, Ralph Solecki, the director of the excavations, concentrates in this book on the Middle Paleolithic period and on the discovery of these human fossils. The story is told in a popular style, with diversions to describe local geography, the wild and woolly Kurds, and life in and around the archeologists' camp. It is a personal narrative. For the archeologist it yields useful insights into the thought process of a famous colleague and contains information about Shanidar not readily available elsewhere. I am not sure just what the book provides the layman.

The narrative is uneven. At times the story bounces along with vigor and our interest is captured and held; at other times the author

loses us in repetitious descriptions of routine excavation. There is much romance in archeology. Yet it is difficult to maintain an absorbing narrative based solely on the discovery of things in the ground. As with all historical research, there are actually three exciting stages in the progress of archeological discovery: the uncovering of the objects themselves, the revelations that come with a detailed analysis of the things excavated, and, finally, the integration of the new data with the old to alter our overall understanding of a period in the past. The author deals with the second and third phases in the Shanidar story only briefly in his introductory chapter and in the last twenty-five pages of the book. When pollen analyses were done on the soil that had surrounded one of the Neanderthal skeletons, it was discovered that the body had been originally buried with flowers. This remarkable find forces us to rethink the whole question of the nature of man 60,000 years ago, and the book closes with a too brief chapter entitled, "Toward accepting Neanderthal man as our ancestor."

This is exciting stuff; a unique find, a clever bit of analysis, and a new perspective on the Middle Paleolithic. More of this and less brushing of bone in the ground would have made the book more stimulating and useful for the informed, nonprofessional reader.

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ANGEL CABO and MARCELO VIGIL. Condicionamientos geográficos; Edad antigua. (Historia de España Alfaguara, 1.) Madrid: Alianza Editorial Alfaguara. 1973. Pp. vii, 450.

This opening subdivision of a seven-volume survey of Spanish history plainly addresses itself not to the specialist but to a university and general lay audience. It fully lives up to expectations aroused by a series that includes contributions by A. Domínguez Ortiz and Miguel Artola, the general editor. In the third of the book allotted to the long-term geographical factors conditioning Spanish life through the ages, Angel Cabo examines with both insight and adequate statistics the peninsula's landforms, climates, and vegetation cover; the coastal, tableland, and sierra zones; the river systems; and ancient routeways. He skillfully relates all these factors to the regional diversities of Iberian agricultural, pastoral, mining, and industrial economies in prehistoric and historic times. More attention might have been paid to soils, grasslands, brushland (matorral, as important as forest in this immemorially pastoral land), rural settlement forms, and field systems, although urban centers are well covered; but this lack detracts little from an expert, informative account that takes due note of the constant interaction between landscape and society and is enriched by comparisons with other European countries—a planned feature of this series—on land use, sizes, and types of landholdings, crop yields, stockraising, and similar topics.

In his two-thirds of the volume, Marcelo Vigil with equal success views the human profiles of ancient Iberia. Three of eight chapters outline the prehistoric cultures, the impact of Phoenician, Carthaginian, and Greek penetration (with good comments on the native kingdom of Tartessos), and regional ethnography just before the Roman occupation. On this last subject Vigil is especially effective, replacing the usual time-worn generalities about the Iberian peoples as a whole with a succinct regional survey embracing the manifold differences in tribal life and cultural level, although he does surprisingly little with the Basques, reputedly still very much with us. Succeeding chapters take the story through the six centuries under Rome, with major reference to problems of Romanization, population changes, the henceforth basic road network, and economic, social, and political structures. Despite limitations of space, Roman Hispania is properly presented within the larger framework of Roman imperial civilization and in terms of recent scholarship in the field. Vigil ably illumines the distinctly partial success of the Romanizing process outside the cities, above all in the northern countrysides that were to constitute the bases of medieval Spain. Religious life, specifically as this relates to indigenous cults, the mystery religions, Sephardic Jewish origins, and the earlier evolution of the heresy-torn, rigoristic Hispanic Church, is less satisfactorily handled.

Those who, like Américo Castro, prefer to start Spanish history after 711 may wonder why this Alfaguara series, weighted as it is in favor of the modern period—three volumes to 1700, four to 1931—includes such a volume as this. But less committed minds will find this an excellent summary record, with selective bibliographies, of those fundamental telluric, ethnic, socioeconomic, and cultural forces without due acknowledgment of which the long history of Spain remains incomprehensible.

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Ancient 377

CLAUDE MOSSÉ. Athens in Decline, 505-86 B.C. Translated from the French by JEAN STEWART. Boston: Routledge and Kegan Paul. 1973. Pp. 181. \$11:75.

This new book by the distinguished author of La Fin de la Démocratie Athénienne (1962) appears in an English translation, and apparently in translation only. The present subject is much broader in scope, dealing with political history as well as with social and economic developments, while instead of ending with the triumph of Philip it continues for another quarter of a millennium to the conquest by Sulla. Her views on the finality of Philip's victory are not modified, as the rubric "Chaironea and the end of Greek liberty" (p. 68) indicates. Why then does the author decide to recite the melancholy chronicle of frustration? At the very end we are given a reason: "All things considered, it is perhaps this intuitive awareness of the real problems and the impossibility of solving them which, across the gulf of time, gives such rare value to the history of the decline of Athens." In 1911 W. S. Ferguson described this same period in a monumental work, Hellenistic Athens. The reason he gives for ending with Sulla is worth remembering: "Athens had no future except as the seat of a great university, and this was but a modest one. High culture is a delicate plant. It thrives only in the keen air of a free country. In a hot-house it makes but a sickly growth" (p. 458). In part the difference in point of view reflects the contrast between the period before Sarajevo and our own. For Ferguson, the history of Athens after Alexander represents a successful holding action, the maintenance of a way of life; for Mossé, on the other hand, it represents a series of futile attempts to deal with an intolerable social situation. The sufferings of the underprivileged outweigh the cultural achievements of the famous

Any short account of a long period tends to be interpretive rather than factual, but the present work is usually well balanced. Occasionally this is not so, as when we read that Philip was "assassinated on the orders of Olympias, the wife he had repudiated" (p. 79). And this is an unfounded assumption. More misleading is the author's account of Demosthenes: "We must not attempt, as some have done, to make excuses for Demosthenes" (p. 89). She is speaking here of the Harpalus affair, yet her overall view of the great orator is one-sided. She does not take into account the new evidence, in the form of

a missing letter of Demosthenes convincingly reconstructed by J. A. Goldstein in *Letters of Demosthenes* (1968), which puts his conduct in a very different light.

Now and then one suspects an error has occurred in translating from the French. We are told of merchants who borrowed money in Athens and "shipped a load of wheat to Egypt" (p. 92). In point of fact they bought in Egypt, then sold the grain in Rhodes.

(In general this is a readable and much needed book on a period too often neglected. The way in which inscriptions, passages from the orators, and essays (by Xenophon, Heraclides Criticus, and others) are woven into the text

Criticus, and others) are woven into the text will send the reader to the sources, where he can make up his own mind about the author's conclusions.

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PHILIP A. STADTER, editor. The Speeches in Thucydides: A Collection of Original Studies with a Bibliography. Chapel Hill: University of North Carolina Press. 1973. Pp. xii, 172. \$10.95.

In Greek literature the first author to compose speeches for his characters in what might be termed historical narrative was Homer, but in no historian proper are such speeches more important than in Thucydides. In early 1972 the University of North Carolina sponsored a colloquium on this topic, the papers of which are presented here.

To itemize, Immerwahr considers the concept of power or dynamis as a basic theme in Thucydides' work, especially in the speeches. Raubitschek analyzes the puzzling first speech of the Athenian ambassadors at Sparta, and Hammond takes up especially the speeches of Hermocrates to illustrate the interweaving of specific and general statements. Stahl treats the course of events and speeches in Books Six and Seven; McCoy discusses Book Eight in an essay that differs from the others in focusing mainly on the historical development there described. In a very thoughtful analysis Westlake relates the speeches to the preambles (or introductions) and postscripts with which Thucydides surrounds them; Stadter surveys Plutarch's use of Thucydidean speeches. West provides an introductory list of the speeches, totaling 141 if one includes the most indirect of discourse, and also a concluding bibliography. The latter is very full but typographically difficult to use with ease. No one directly treats the most famous Thucydidean speech, the Funeral Oration of Pericles, but in a most unexpected way the reader will find himself surprised and given food for thought. In Stadter's essay, that is, one perambulates gently over the apparently open and pleasant greensward of Plutarch's lives, only suddenly to find a land mine going off in the observation that Plutarch apparently did not think the oration genuinely Periclean. From direct citations in the Moralia Plutarch certainly knew this speech well; but as Stadter emphasizes, the biographer of Pericles bluntly asserted that "he has left nothing written except his decrees," does not cite the oration among the fragments then known of Pericles's speeches, and uses a decree to illustrate Pericles's spirit. Plutarch was, after all, more careful in evaluating his sources than appears on the surface; anyone who wishes to take the oration as truly Periclean must consider Plutarch's caution.

The quality of the contributors to this volume is high. Sometimes they throw light on various aspects of Thucydides's work; more often perhaps they illuminate the baffling majesty of its narrative and speeches. Westlake deserves the last word in his wise observation, "I often find myself tempted to try to penetrate the mind of Thucydides, but it is a most hazardous undertaking."

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PIERRE BRIANT. Antigone le Borgne: Les débuts de sa carrière et les problèmes de l'assemblée macédonienne. (Annales Littéraires de l'Université de Besançon, volume 152. Centre de Recherches d'Histoire Ancienne, volume 10.) Paris: Les Belles Lettres. 1973. Pp. 397.

Among the officers of Alexander the Great, Antigonus the One-eyed (ca. 383-301 B.C.) was one of the most prominent in the struggles among Alexander's successors as they fought to gain control of the conquered territories. Yet in one respect Antigonus has remained something of a mystery for lack of readily accessible evidence for an important period of his career, between the years 334 and 321 B.C. During these years, 333-323 B.C., he served as satrap of Greater Phrygia, a post in which he had the responsibility of keeping Alexander's communications open. Likewise he is mentioned for three victories over Persian forces after the battle of Issus. But there is a conspicuous lack of infor-

mation about Antigonus at this period—a silence that, it has been suggested, may be traced to the circumstance that Ptolemy, who wrote an influential history of Alexander's campaigns, was a bitter enemy of Antigonus in the wars among the successors.

The present study, which is devoted to the history of the years 334-321 B.C., brings a distinguished contribution. The author, who is a member of the faculty of ancient history at the University of Tours, provides a painstaking examination of the evidence, literary, epigraphical, and papyrological, which, in addition to clarifying the events of the period, brings new light to the sources on which our knowledge depends.

An important part of the study is devoted to a detailed investigation of the political and juridical role of the "assembly of the army," that is, the Macedonian troops, which were in fact the only stable political element that was capable of intervening in the power vacuum created by Alexander's death. This inquiry includes a renewed consideration of the debated problem of the existence, in the distinctive Macedonian political system, of an "assembly of the people" alongside the "assembly of the army."

The critical account of the sources and the valuable bibliography enhance the value of the book. The work is a thorough and judicious treatment of a difficult passage in history that will be indispensable to scholars and will not easily be superseded.

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ALAN CAMERON. Porphyrius the Charioteer. New York: Oxford University Press. 1973. Pp. x, 286, 31 plates. \$19.25.

The values of any society are evident in its choice of heroes. As Norman Baynes noted, the heroes of the Byzantine world were the ascetic saint and the victorious charioteer. Both were victors in a strenuous struggle, the last exemplars of the Hellenic contest spirit. Much has been written about the athletes of God, less on the heroes of the Hippodrome who were idolized by fanatic fans, the famed Greens and Blues of Constantinople. Alan Cameron has remedied this lack with a scholarly study of the most renowned charioteer, Porphyrius (also known as Calliopas), who delighted the racing crowds for several decades in the late fifth and early sixth centuries. Thirty-two epigrams in the Greek Anthology celebrate the glory of the first driver

to have his statues erected in the Hippodrome, next to those of emperors, while still competing in the races. The bulk of this book is a critical analysis of the epigrams and a careful study of the inscriptions on the bases of the monuments. As such, the work is detailed and scholarly, a credit to British classicism. Cameron also cites the Guinness Book of Records on cricketers and jockeys, and he appreciates the merits of Robert Graves's novel Count Belisarius. The great general, it will be remembered, married the daughter of a charioteer, and the most famous woman of the Circus demimonde, Theodora, "hooked a future emperor."

An extraordinary driver who still raced at the age of sixty, Porphyrius was a favorite of both Blues and Greens. As champion of the Greens, he led his fans in a pogrom against the Jews of Antioch in 507, and at Constantinople he helped rally support for the Emperor Anastasius during the revolt of Vitalian in 515. The bond of the crowd-pleasing Porphyrius with the unpopular Anastasius adds weight to Cameron's thesis that the monarch began the practice of allowing statues of champion drivers to divert the energies of the rival factions from rioting to fund raising. The cunning Anastasius (who incidentally was a Red) prompted Porphyrius to change his colors frequently in order that both Greens and Blues could glory in his victories. In 498 beast games were banned, and in 507 pantomimes were forbidden. Hence, the races were the focus of popular interest, but the feverish atmosphere of the Circus was explosive and the intensive rivalry of the factions often erupted in serious riots. After overreacting to earlier disorders, the emperor seems to have solved the problem with the Porphyrius cult. What emerges from Cameron's detailed arguments is an important re-evaluation of the role of the Circus factions, which previous writers have assumed were politicized groups representing social, economic, and religious interests. "There is not a scrap of evidence for such hypotheses—and much against," he asserts in a valuable final chapter.

In a forthcoming book, Circus Factions, Cameron will explore this significant topic from the Augustan era to the tenth century. While the Porphyrius book may be formidable to nonspecialists, Circus Factions should be of great interest to social historians and students of popular violence. This reviewer is looking forward to it.

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## MEDIEVAL

ROBERT DELORT. Life in the Middle Ages. Translated by ROBERT ALLEN. Lausanne: Edita Lausanne; distrib. by Universe Books, New York. 1973. Pp. 345. \$35.00.

The author intends this volume "for all those who, though not historians, are interested in medieval society . . . [also] for those who want to discover the traces of the Middle Ages that still remain in the technology, religion and mentality that make up our daily life at the present time. . . ." (foreword). That he will succeed in arousing the interest of the general reader, even that of the scholar, is reasonably certain, given the abundance of excellent illustrative materials that graces the volume. These illustrations afford the reader a sharper impression of what life was like in the M:ddle Ages than most texts could, including the book under review. In the case of this volume that is not wholly regrettable since the author is overly ambitious in the number of topics he considers and also too anxious to make his statements as distinctive as his illustrations. His observation that "the population [of the Middle Ages] received fewer high-energy particles than we do today and that the sun is no longer quite the sun they knew" (p. 18) gives some notion of the broad sweep of his brush. There is, indeed, no facet of medieval life that passes him by-physical geography, agriculture, technology, travel, peasantry, townspeople, aristocracy, clergy, Jews, Crusades, standard of living, amusements, and what have you—although he finds little time for literature, thought, education, and popular piety. Among the more arresting of his observations is his statement that "a man was considered old at 35" (p. 59). This is nonsense. He bases this figure on the presumed life expectancy of the period, which was actually almost as high then as ca. 1800 when, according to Edward Gibbon (Autobiography), most children were still predeceasing their parents-and who would have called George Washington an old man at thirty-five! Except that the reader should beware of gulping down similar affirmations that the author throws out on such a wide variety of subjects, it would be unjust to cite his statement about being old at thirty-five since the substance of what he offers accords with the views of many scholars.

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JOHN T. MCNEILL. The Celtic Churches: A History, A.D. 200 to 1200. Chicago: University of Chicago Press. 1974. Pp. xiii, 289. \$10.00.

It is a pleasure to have another book from the pen of John T. McNeill, long known as a historian of the church. The present work draws upon a lifetime of experience and at the same time shows familiarity with the current literature. The author writes clearly and agreeably, producing a very readable narrative history that will entertain and will also serve as a textbook and as a reference book. It replaces Gougaud as the standard reference work for those interested in the history of Celtic Christianity.

McNeill begins by a summary of the early history of the Celts and of British Christianity, goes on to study the Christianization of Ireland and Irish monasticism, and then discusses the contributions of Irish Christianity in missionary work, ecclesiastical institutions, art, and letters. The last chapters deal with the extensive Irish influence on the Continent. Throughout, the greatest attention is paid to the lives of individual saints, which, the author asserts, form the most important part of the history of Celtic Christianity.

This emphasis means that other ways of treating the question do not receive adequate attention. One looks in vain, for example, for sociological or political analysis, and occasionally explanations lead nowhere: the Celtic church had no well-articulated institutions, we are told, because the Celts had no genius for organization. The book also lacks dimension in the history of ideas: there are several discussions of peregrinatio without reference to Gerhart Ladner's brilliant article "Homo Viator." Occasionally there is a strange mixture of critical analysis and uncritical narrative. The section on St. Patrick, for example, begins with a clear discussion of the current state of research but then concludes with an uncritical, semihagiographical narrative of the saint's life.

With only a few lapses, however, the documentation, notes, and bibliography are up to date. In the future, more analytical historians of the Celtic church will need to use this book and will value it.

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GEOFFREY ASHE. Camelot and the Vision of Albion. New York: St. Martin's Press. 1971. Pp. 233. \$5.95.

RICHARD BARBER. The Figure of Arthur. Totowa, N.J.: Rowman and Littlefield. 1973. Pp. 160. \$7.50.

I am at a loss to explain the recent interest in Arthur and in certain aspects of what the late Sir Thomas Kendrick described as the "British Antiquity" (that is, the Druids). Of course there have always been enthusiasts for this period, of whom I am one, but now, judging by the number of books that have appeared on the subject in recent years, there is a wide interest in early British history. Geoffrey Ashe is not puzzled, however. As secretary of the Cadbury-Camelot Excavation Committee he was able to observe at first hand the enthusiasm for that project in the form of money, inquiries, and visits to the site. His explanation is that we are dealing with a new form of patriotism, one to replace the outmoded jingoism of empire. The new patriotism looks to the "Matter of Britain" as its idealized past while at the same time it looks to the future. Ashe wrote in an earlier work (The Quest for Arthur's Britain [1968]): "From this a new and acceptable patriotism, a new sense of national vocation may surely come."

Unless one reads the prologue of Camelot and the Vision of Albion and realizes that it is Ashe's "own small Golden Bough," the rest of his book will come as a surprise since it deals more with Titans, William Blake, Zionism, and Gandhi than it does with Arthur. It is also a bit of a good thing, for when Ashe deals with these things and his own intellectual pedigree he is fascinating. The book is well worth reading for the latter alone. When he deals with Arthur, he is perverse when he is not merely wrong.

Considerations of space allow me to mention only two Arthurian matters, and those briefly. Anyone who has read the published excavation reports of South Cadbury and the separate opinions of Leslie Alcock, who headed the excavations, knows that South Cadbury was not Camelot. Anyone who has read much further knows that Camelot never existed except in the fertile brain of Chrétien de Troyes or some slightly earlier source and in the imagination of later antiquaries. Ashe also knows this and says so (p. 7). He goes on to say, however, that "Cadbury Castle could have been the original Camelot in another sense, as the real Arthur's headquarters." And again, "This city of the imagination is a gorgeous image projected by the tradition of a real Arthur with a real headquarters" (p. 71). In other words South Cadbury is Camelot after all, a prejudice that was imbedded in the title of the Cadbury-Camelot Excavation Committee.

A closely related objection is to Ashe's conception of the "Arthurian fact." According to

the argument there was a British war leader at the end of the sixth century who fought against the invading Saxons and who defeated them at the Battle of Badon. For such a figure Cadbury Castle, in terms of its size and location, would have been an appropriate base. Traditionally that leader was Arthur. Unfortunately for Ashe the evidence reads somewhat differently. There was a British or at least sub-Roman war leader named Ambrosious Aurelianus. The British did defeat the Saxons at the Battle of Badon. There are nine known fortified sites of roughly this same period and area of which Cadbury is by far the largest. There is no provable connection between the traditional Arthur and any of these facts or for that matter is there any necessary connection among them.

To turn from Camelot and the Vision of Albion to Richard Barber's The Figure of Arthur is to enter a different intellectual world. Like Ashe, Barber is not a professional scholar in this field (even though he has an earlier book on Arthur). That fact, frankly, is often painfully obvious whenever he departs from the Arthurian material per se. But Barber has a fine critical mind, which, when applied to the Arthurian sources, comes as a welcome relief from the emotional partisanship that usually surrounds the figure of Arthur. I have not yet read a better introduction to the subject. On the other hand I am dubious concerning the main positive argument of his book. In brief Barber holds that the original Arthur was a Scottish prince who died in battle against the heathen barbarians in the north of England. This Arthur of Dalraida acquired some local bardic fame. Later, through a similarity of name and a foreshortening of folk memory he was adopted by the Welsh, associated with the events surrounding Badon, and transformed into the Arthur of pseudohistory and legend. It would take a linguistic scholar to judge adequately the quality of Barber's arguments, particularly in respect to how the northern Arthur was transplanted into southeast Britain.

DONALD A. WHITE

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A. CAMPBELL, editor. Charters of Rochester. (Anglo-Saxon Charters, 1.) New York: Oxford University Press, for the British Academy. 1973. Pp. xxxv, 69. \$9.95.

Thirty years ago, Sir Frank Stenton noted in his Anglo-Saxon England that a new edition of Anglo-Saxon charters was "much to be desired." Despite his plea, scholars until now have had

to rely upon Walter D. Birch's Cartularium Saxonicum: A Collection of Charters Relating to Anglo-Saxon History (3 vols., London, 1885-93) and John Kemble's Codex Diplomaticum Aevi Saxonici (6 vols., London, 1839-48). These editions are inadequate because neither editor made much effort to distinguish between authentic charters and forgeries; moreover, additional documents have come to light since their publication, and recent scholarship provides much better tools for dating and authenticating Anglo-Saxon materials. In 1966 the British Academy and the Royal Historical Society jointly began preparation of a new edition of Anglo-Saxon charters that will include all pre-Conquest title deeds that have survived, whether formal charters, wills, writs, leases, or other documents. The first volume in this project, Charters of Rochester, has now appeared, edited by Alistair Campbell.

Campbell's collection contains thirty-seven charters, four in Old English, the rest in Latin, although eleven Latin charters have descriptions of boundaries in Old English. Most of the documents are royal grants of lands to the cathedral church of Rochester, ranging from a spurious charter of Ethelbert I in 604 to an account of a lawsuit concerning an estate of Bishop Godwin, ca. 995-1005. In his introduction. Campbell discusses the manuscripts on which his edition is based. All but four of the documents can be found in the Textus Roffensis, a manuscript in the Rochester Cathedral library, although copies of ten exist elsewhere as well. Campbell discusses the lands mentioned in the charters, locating most of them near Rochester or even within the city. He also discusses the authenticity of the documents, finding five of them to be likely forgeries. Subsequent volumes in the series must live up to a high standard that has been set by Campbell's scholarship in this first volume.

RALPH V. TURNER Florida State University

HANNA VOLLRATH-REIGHELT. Königsgedanke und Königtum bei den Angelsachsen: Bis zur Mitte des 9. Jahrhunderts. (Kölner historische Abhandlungen, number 19.) Cologne: Böhlau Verlag. 1971. Pp. ix, 240. DM 42.

Vollrath-Reichelt runs counter to most English scholars but is none the worse for that. Unlike those who see a straight development from Bede's seven Bretwaldas through Mercian hegemony to Egbert of Wessex, she distinguishes a special imperium, characteristic of the early

Bretwaldas and later of Egbert, that gave military leadership in the face of outside threat (Pictish-Scottish in the former period, Viking in Egbert's) from the expansionist domination of Anglo-Saxon kingdoms by Mercia and later Wessex. Both developments combine in Egbert. This major thesis is encased in a somewhat awkward division of the book: one part treats general concepts of Anglo-Saxon kingship, on which the author recognizes that English scholarship has had little to say (her approach differs greatly from J. M. Wallace-Hadrill's Early Germanic Kingship in England and on the Continent, which was published in the same year); a second part details specific political actions of rulers. In discussing Königsgedanke Vollrath-Reichelt analyzes Bede (for whom legitimation lay in right of blood combined with consent of the folk) and other sources to conclude that no concept of kingship as Christian divine mandate existed before Egfrid of Mercia's anointment (787) and the Synod of Chelsea. Also discussed in part 1 are the meaning of gens, natio, and populus, the role of the witenagemot in royal rule, and regnal rights in relation to Bretwalda-ship.

Part 2 grapples more directly with the main thesis. Bede's Bretwaldas, in contrast with the great Mercian rulers, are seen as exercising military command but not imposing tribute, granting bocland, or dominating ecclesiastical affairs in other Anglo-Saxon realms. After a contrasting Mercian overlordship, Egbert continues the line of Bretwaldas after a break of 150 years. Bede means what he says when he ends the earlier list of those bearing imperium with Oswiu. A long excursus returns to the time-honored problem of folcland (here seen as land possessed by the king, not privately but as ruler) and bocland.

The book has weaknesses: it omits nonliterary sources (for example, archeology and numismatics), neglects the written evidence of laws, poetry, and so on, and side-steps problems of both Germanic and Roman influences. However, it recognizes that different sources (for example, Bede and charters) require different questions. Its major strengths lie not only in suggestive textual analyses of charters and in fitting old problems (Wilfrid's fall, Offa's Kentish relations) into new contexts, but in raising questions neglected by almost all English scholars and showing how these and other problems parallel Continental rulership in less insular isolation than often appears.

WILLIAM A. CHANEY
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v. păsuta. Lietuvos valstybės susidarymas [The Formation of the Lithuanian State]. Vilna: Izdatel'stvo "Mintis." 1971. Pp. 423.

In this book a noted Soviet medievalist attempts a systematic explanation of the developmental process of a number of thirteenthcentury weak, obscure, and still heathen Lithuanian principalities into a major unified, dynastic power in Eastern Europe by the middle of the fourteenth century. The work, in which the standard Marxist conceptual framework is used, is a major effort to reinterpret synthetically the written sources and to bolster the argument with recent archeological and ethnological evidence. Since the available materials on the early socioeconomic and political conditions among the Lithuanians are very scanty, the author wisely does not shy away from interpolating and drawing useful analogies from known facts about Lithuanian kinsmen, the Prussian and the Latvian peoples about whom early sources happen to be more readily available.

The author's main thesis is that the development of a centralized Lithuanian state has not been the result of an internecine, dynastic struggle among the nobility, nor was it primarily connected with the unifying efforts of Prince Mindaugas, the first crowned king of Lithuania, or of his immediate successors. In Pašuta's view it has come about mainly because of "scientifically predictable" interplay of local economic and social developments taking place in the region during the twelfth and thirteenth centuries, developments that were also importantly affected by the drastically changed international situation of the area due to the arrival and ambitions of Germanic, Polish, and Mongol rulers toward the Lithuanian people. The immediate threat these invaders represented—as vividly exemplified by the fate of the Prussians, Latvians, and Estonians in the thirteenth century—made the establishment of a centralized, dynastic, and therefore stronger and more effective state a basic precondition for the continued independent economic and political existence of the Lithuanian people.

The concomitant expansion of this newly forming centralized state eastward and south-eastward into the principalities of Rus in the thirteenth and fourteenth centuries is interpreted as having been in part both an aggressive and defensive move. It aided both the power of the emerging dynasty of the Lithuanian grand princes and at the same time provided the country with needed additional resources

and manpower to counter effectively the constantly increasing pressure of Lithuania's most important and dangerous enemies, the Germanic knight orders.

Notwithstanding the strictures and limitations imposed by the framework used, this is still a very important, erudite, and synthetic interpretation. The documentation throughout is careful, and the accompanying bibliography is very exhaustive and useful. The work will have to be taken into account by all who concern themselves with the problems and evidence so ably marshaled here.

BENEDICT V. MACIUIKA
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M. H. KEEN. England in the Later Middle Ages: A Political History. London: Methuen and Company; distrib. by Barnes and Noble, New York. 1973. Pp. xii, 581. \$13.00.

Nearly two decades ago G. R. Elton's England Under the Tudors appeared. Now Methuen has added a second volume to its series, A History of England in Seven Volumes, with the publication of Dr. Maurice Keen's book. Purporting to have written a textbook, Keen has not in fact done so. Instead of the expected balanced rendering of political, economic, social, and intellectual accomplishments, which is found nowadays in textbooks, the discussion centers upon politico-military developments. Concomitant societal activities are considered only when they are relevant to the political events. This is not to say that Keen has returned to the nineteenthcentury manner of historical writing. While his focus is upon politics and war, his purpose is to explain their impact upon late medieval English society. War dominated the period. As a result the government and society functioned under severe pressures. Owing to an insufficiency of resources the English monarchy usually could not relieve these strains. The populace, confronted by a lack of leadership within both the monarchy and the landed aristocracy, initiated the major constitutional, economic, and social changes of the fourteenth and fifteenth centuries.

Edward I and his successors inherited the governmental system that had well served his predecessors. Despite its historical base, the structure proved unequal to the strains imposed by constant warfare. The failure of the system stemmed largely from two circumstances: first, the paucity of the monarchy's financial resources, and second, the independence of the

landed magnates. Consequently as early as 1297 Edward I discovered the general dislike of his wars fostered questions about his use of his royal power. The deterioration of the relations between ruler and ruled continued during Edward II's reign because of his youthful incompetence and his reliance on favorites. As the insecurity of the magnates increased so did their feuding, which further aggravated their discontent.

Temporary relief came with the accession of Edward III. Young Edward unified the realities of medieval warfare with the then current chivalric ideals and thereby firmly bound the nobility to him. By the time English enthusiasm for the Hundred Years' War was dampened by heavy taxation, disagreements over the maltoltes, and disruption of overseas commerce, Edward III was in his dotage with his great victories behind him. The war's cost, Keen feels, damaged the economy more severely than the plagues of the period. Dissatisfaction with the government came also from the decentralization of the administration of royal justice. Edward's alliance with the landlords allowed the administration of justice to become the province of the gentry, thus inextricably intertwined with bastard feudalism.

By 1381 the commons of England had had enough of inept and corrupt governance and had revolted. Although Richard II's councilors, being men of wealth and land, struggled to revive the monarchy's prestige, their solution of retrenchment through conservative definition of the royal prerogatives failed. Such behavior created suspicion rather than respect. Unfortunately for England's future the tensions of Richard II's day produced the political divisions of the next century. The aristocrats distrusted each other, while the commons feared social disorder internally and France externally, the result being that in Henry IV's reign England experienced political and social upheaval. It was, according to Keen, the response of the commons rather than the response of the monarchy to the anxiety of the period that created the so-called Lancastrian experiment. But, even with this, only luck saved Henry IV at the end of his reign from a civil war. His grandson, Henry VI, in a similar situation was to be less fortunate.

For a short time the accumulating problems of the Lancastrian monarchy were solved or at least diverted by the militarism of Henry V. Young Henry epitomized the pious, chivalrous, and just king that his great-grandfather, Edward III, had once been. The reopening of the

French war in 1415 received the approval of noblemen and commoners alike. In the thrilling conquest of northern France, Englishmen found reassurance. England, again, appeared well governed. With Henry V's death the successes in France, with which the English identified, decreased, ceased, and defeats followed. The increasing losses in France coupled with growing military expenses exacerbated the deteriorating domestic situation. Incapable of solving either problem Henry VI and his councilors lost first the loyalty of many magnates and members of the gentry and then the throne in civil war.

In the fifteenth century in France and England, emulative aristocrats clothing their ambition in legitimism vied for mastery of the king and thence the government. Royal illnesses compounded the confusion at court. Both situations produced civil wars that ended in the creation of strong monarchies. In England's case, Edward IV cemented the nobility, gentry, and commons to the monarchy by careful use of patronage, good lordship, and a magnetic personality. Prudent fiscal policies and reduced military expenditures ended the financial drain on the kingdom. With peace abroad England returned to a more ordered state. This, though, Keen believes to have been an accident caused by diplomatic ineptitude rather than by any conscious policy of the Yorkists.

Dr. Keen has rendered us a great service by pointing out the importance of foreign wars to late medieval English society. His thesis that monarchs who understood warfare and their fellow Englishmen's waging of it were the more successful rulers has considerable merit. His other view that the governed instead of the governors initiated the major changes of the period is less convincing and is, in fact, ignored by the author himself as his volume progresses. Whether liberal historians like to admit it or not, England did fight the first Hundred Years' War, and their condemnations of the bellicose English kings of the time does nothing to change this fact.

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HELENA M. CHEW and WILLIAM KELLAWAY, editors. London Assize of Nuisance, 1301–1431. (London Record Society Publications, volume 10.) London: the Society. 1973. Pp. xxxiv, 221. By subscription.

Both social and legal historians will find material to interest them in this book. It is a calendar of cases heard by the Assize of Nuisance in London from 1301 to 1431. The cases arose under the Assisa de Edificiis, a compilation of regulations the earliest of which were drawn up probably in the late twelfth and early thirteenth centuries. The regulations deal with walls, gutters, privies, windows, and pavements. The City was much troubled by fires and was therefore concerned not only to provide means for settling disputes but also to encourage the use of stone in building.

Most of the cases between party and party concern walls, especially party walls. The regulations provided that neighbors wishing to build such walls should each give 11/6 feet of land and share the cost of building a stone wall g feet in width. If one of the parties could not afford to build his portion of the wall, he was to give his land so that his neighbor might build on it. So there were many possibilities of conflict. A common one was that a neighbor had allowed his part of the wall to fall into ruin so that chickens, dogs, and children could come into the complainant's garden. Gutters also seem to be a common cause of complaint. They spilled water on the neighbor's premises causing rotting of timbers in walls or outhouses. Privies and cesspools were also a common cause of complaint, and windows on the side of a house had to be of a certain height in order to protect the neighbor's privacy.

Some complaints were brought to the assize by the commonalty. These complaints arose concerning overhanging walls, pentices, and solars that obstructed passage through a street, lane, or alley. The assize before whom the case was heard consisted of the mayor and twelve aldermen, although twelve were not always available. Sometimes a jury was called to deal with a point in controversy. Often also a panel of experts was called when the matter in dispute (such as a gutter that ran underneath a neighbor's house) could not be evident to the assize. Decisions were made promptly, and forty days was the usual time allowed for correction of the matter complained of. Enforcement was probably not easy, and sometimes the plaintiffs were responsible for delay. In one case they did not press for enforcement until five years had passed. Plaintiffs normally waited from three to nine months before complaining that the decision of the assize had not been enforced. On the whole the assize seems to have been a fairly effective instrument for remedying nuisance.

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ROBERT CHAZAN. Medieval Jewry in Northern France: A Political and Social History. (The Johns Hopkins University Studies in Historical and Political Science: Ninety-first Series, number 2.) Baltimore: Johns Hopkins University Press. 1973. Pp. xi, 238. \$12.50.

This is a very useful book. We have had no comprehensive history of the Jews in Capetian France; Professor Chazan has filled an annoying gap in our knowledge of medieval Jewry. His ability to use Hebrew sources adds greatly to the value of his work. We can see events from the Jewish as well as from the Christian side. He has tracked down every possible reference to Jews in the old royal domain and in Normandy, Anjou, Blois-Champagne, and Nevers. Thin at first, the sources become fairly abundant for the thirteenth century.

The most important contribution of the book is the list (and map) of Jewish communities in northern France. He has found 131 Jewish settlements plus 87 mentions of individual Jews in other towns and villages. There were Jewish communities in quite small towns, such as Caudebec-en-Caux, Chauny, St. Pierre-sur-Dives, Villers-en-Argonne, and Chaource. One can understand why the Church feared the impact of learned Jews on ignorant villagers.

The outlines of the story are familiar—obscure beginnings, a fairly tranquil, prosperous, and intellectually brilliant period in the twelfth century, increasing troubles in the thirteenth century when worldly kings (Philip Augustus and Philip the Fair) took the Jews' money and pious kings (Louis VIII and St. Louis) tried to deprive them of their livelihood and their scholarly tradition by forbidding usury and by burning the Talmud. The end of French Jewry came with the expulsion of 1306. Chazan has explained this rise and fall more convincingly and more vividly than any earlier writer.

Intellectual history was deliberately excluded from this work, but a page or two on scholarly contacts between Jews and Christians—for example, in establishing the text of the Old Testament—would have reinforced his appraisal of the twelfth century as a relatively tolerant period. And one can wish that the author had spent a little more time in correcting awkward sentences, eliminating repetitions, and checking his translations of Latin names (Daniel Brito, clericus, is hardly Daniel Cleric). But the scope and detail of this book will make it invaluable to any student of the problem.

JOSEPH R. STRAYER
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JOHN HOLLAND SMITH. Joan of Arc. New York: Charles Scribner's Sons. 1973. Pp. 232. \$8.95.

With this book John Holland Smith once again shows himself a master of the difficult craft of serious popular history. His narrative is terse, precise, and constantly in motion. Except for the first and last chapters, he tells the story largely in the words of Joan and her contemporaries. The text thus approaches the dryness of court reporting and the inherent intensity of good courtroom drama. The minor personalities-especially the evil ones-are sharply delineated: Georges de la Tremouille appears even more the villain of the piece than old Cauchon himself. Joan comes through as a driven and ruthless, but mysteriously attractive, late adolescent. On the more technical level, Smith describes the nature and purpose of medieval inquisitorial procedure with absolute fairness, stressing both its fundamental penitential purpose and its similarities to the political show trials and "self-criticism and confession" exhibits the twentieth century knows so well. The details of warfare are treated with somewhat less clarity. Smith is weakest in those few places (mainly in ch. 1) where he attempts to describe the general political and economic conditions in France at the beginning of the fifteenth century and to narrate in a few pages the Byzantine political machinations of the Valois court. For these topics, students will have to be sent elsewhere. But these are minor flaws indeed, for the remainder of the book is utterly captivating. I read it nonstop from beginning to

Beneath the polished surface of his narrative, Smith also makes one serious scholarly point, implicitly if not explicitly. Contemporary reactions to Joan—the way she was portrayed in 1451 in the company of Judith with the head of Holofernes, the way the inquisitors turned her male dress into the central issue at her trial (her return to it was their excuse to release her to the secular arm and the stake)—speak loudly of the very special nature of male fears in the fifteenth century. This underlying perception is important and deserves more extended attention than it has yet received.

In the last chapter, Smith turns his attention to the posthumous Joan: the fifteenth-century false Joans and the twentieth-century mythical Joan, saint, bastard daughter of Isabella of Bavaria, rescued from the stake. Such stories have founded a veritable industry in France during the last generation. (Polemics have inevitably followed. Those interested in the genre should turn to Regine Pernoud, Jeanne devant

les Cauchons Paris, 1970], who, at least, is quick and usually rapierlike. The more recent Yann Grandeau, Jeanne insultée [Paris, 1973], takes the romance industry all too seriously and at too much length.) Smith describes and dismisses them in six pages, which, in the absence of serious new evidence, is about what they deserve.

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LOUIS B. PASCOE, S.J. Jean Gerson: Principles of Church Reform. (Studies in Medieval and Reformation Thought, volume 7.) Leiden: E. J. Brill. 1973. Pp. xii, 233. 64 gls.

Gerhart Ladner opined that the "Idea of Reform" in the context of the Church's history "is characterised by the belief both in ineradicable terrestrial imperfection and in a relative perfectibility the extent of which is unforeseeable." Louis Pascoe acknowledges himself to be another disciple of Ladner. Instead of working backward from Jean Gerson's reformatory schemes to underlying, and perhaps developing, principles, he analyzes his subject's multifarious writings and from them constructs an integrated concept of reform. What emerges-and the methodology has ensured our preparedness is an essentially conservative and eclectic system, with few surprises. This is not Pascoe's fault: of Gerson much has been written.

Gerson, following a well-worn tradition, depicts the Church in hierarchical terms. It is the reflection of a celestial archetype, perfectly constituted at its inception. Even elements seemingly absent from the ecclesia primitiva existed in germ. But Gerson concedes that the Holy Spirit could create "new channels of authority and order within the Church," though not in his time! His expositor interprets this as "openness of mind," without seeming to realize its potential for demolishing the whole edifice.

Below the supercelestial and celestial hierarchies of the Trinity and the angels is the Church with its triple division of pope and cardinals, bishops and priests, and—in a passive role—laymen. The functions of Gerson's hierarchies, on the Dionysian model, are purgation, illumination, and perfection. Hierarchical activity serves to edify the mystical body of Christ—the Church.

As in Dante's Divina Commedia, the antithesis of order and chaos—similitudo inferni—is fundamental. The schism constitutes deformity and opponents of the hierarchy (Wycliffe and Hus, one supposes) impede man's approach

to God through its agency. With respect to the hierarchy itself, curia, bishops, and lesser clergy all merit condemnation for failure to execute their functions appropriately. A means of renewal, working through the hierarchy, is the semen vivificum et reformativum, identifiable as the Holy Spirit. Moreover, against the monastic claim to an exclusive status perfectionis, Gerson sets the "universality of Christian perfection." Both in his attitude to the mendicants and cloistered religious, and in his emphasis on the theologian's superiority as an interpreter of canon law, Gerson betrays the predilections of a secular clerk of his day nurtured in the theological faculty at Paris.

The historian will have some qualifications about this work. There are indeed fresh insights, but equally significant occlusions. Some of these arise, one feels, from the methodology employed. More generally, the book exhibits faults not anticipated in scholarly exposition. The style is pedestrian, and there are grammatical errors, infelicities, and even spelling mistakes. Some of these are as follows: "achieved little results" (p. 12); "particularly unique" (p. 19); "exigenicies" (p. 73); "sacriligeous" (p. 78); "he alone is most capable" (p. 93); regio egistatis (p. 198); "Trough" (p. 202). The recurrence of "our research" and "norm" irritates, and perhaps so eminent an international publisher as Brill could utilize a Greek font rather than bemuse the reader with such barbarisms as gnothi seauton.

ROY M. HAINES

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PAUL JOHANSEN and HEINZ VON ZUR MÜHLEN. Deutsch und Undeutsch im mittelalterlichen und frühneuzeitlichen Reval. (Ostmitteleuropa in Vergangenheit und Gegenwart, 15.) Cologne: Böhlau Verlag. 1973. Pp. xxiv, 555. DM 120.

Demographic and social histories of the Middle Ages are slowly becoming more numerous. This particular work attempts to study the demography and social stratification of medieval and early modern Reval (Tallinn), the capital of modern Estonia. As a trading center, this site dates back to prehistoric times, but in a modern political-legal sense, the town was founded in 1230 by a group of German merchants. Yet most of the inhabitants of the city remained "non-Germans," a term that in addition to the native Estonians also encompassed some Swedes, Finns, and Russians. Using a variety of extant town documents such as tax assessment rolls and court and guild records, the authors trace the geo-

graphic and national origins of the citizens of Reval and examine in detail the occupations and social life of the non-Germans. The study shows that to be non-German, generally a euphemism for Estonian, meant a slow but sure relegation to the less desirable and/or profitable occupations. Because of their ethnic origins, the Estonians soon became excluded from most of the guilds and frequently were deprived of the legal privileges accorded the German citizenry.

The work also includes chapters on the history of Reval, the religious life of its Estonian inhabitants, and a discourse on the assimilation and alienation of the natives and the Germans. There is an extensive selection of primary sources and a nineteen-page section listing the names of many non-German inhabitants of the town. The latter are grouped according to whether their surnames indicate their place of origin, occupation, or some distinguishing physical or personal characteristic. Author, geographic, and topographic indexes are provided, as well as a subject index and several maps.

Like the other volumes in the series Ostmitteleuropa in Vergangenheit und Gegenwart, this book is heavily documented. It treats the subject thoroughly, even ponderously, and is directed more toward the specialist than the general reader. While its dual authorship enlarges the scope of the book, it also leads to an unevenness in style and content. One wonders, for instance, if Chapter 2, "A Brief History of Reval in the Middle Ages," and Chapter 6, "Estonian Sermons and Estonian Spiritual Life," really belong in this work. This, of course, may be a matter of individual taste. The authors fail to take into account new theories published in Soviet Estonia concerning the founding of Reval that dispute Johansen's old claim that the town began around the present Old Market. As the authors themselves note, they have been unable to consult that part of the Reval city archives that were not removed to Germany but remained in Estonia after World War II. One also sorely misses a separate bibliography, which would ease the task of tracking down the full references hidden among the copious footnotes.

All in all, however, the book is a solid scholarly study, albeit written from a German point of view, about an infrequently explored topic and a less than well-known but important part of medieval Europe.

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ERNST BOCK, editor. Deutsche Reichstagsakten unter Maximilian I. Volume 3, 1488-1490. In two parts. (Deutsche Reichstagsakten, Middle Series, volume 3.) Göttingen: Vandenhoeck & Ruprecht. 1972; 1973. Pp. 984; 988-1469. DM 225; DM 125.

In 1928 the Historische Kommission der Bayerischen Akademie der Wissenschaften decided to publish the papers of the German Diet under Maximilian I as a separate series alongside the ältere Reihe (covering from Wenceslas to Frederick III), and the neuere Reihe (Charles V). This is the first volume of the series to appear in print, but several more volumes are to appear shortly. It is a good beginning to a long-awaited series. Although the Frankfurt Diet of 1489 was hardly the most famous of the imperial assemblies of the pre-Reformation era, it was the first such gathering in which the young king participated as a free agent. Since the Reichstagsakten have been collected to illustrate the history of the Reich as well as of the Diet, developments outside the assembly have also been given their due. At the outset of the coverage, Maximilian had turned his attentions away from exclusive preoccupation with the Burgundian inheritance. He headed off Wittelsbach expansion by saving the western lands of the House from alienation to the Bavarian dukes by the Archduke Sigismund as well as by fostering the Swabian League as an anti-Bavarian coalition. But the unification of the western lands under strong Habsburg rule and the maintenance of the League assured friction with the Swiss and their increased alienation from the Reich. In the context of these events, the Diet of Frankfurt in 1489, which dealt at length only with the question of a small aid for Maximilian's campaign in the Netherlands, pales into relative unimportance. But the constitutional historian is quick to point out that the Frankfurt assembly was the first such gathering at which the towns participated as full members, and that it was the first at which the three estates were formally grouped into separate chambers (curiae). From the 1490s on, the Diet was the greatest forum of German political discussion and communication, and Bock argues that this particular meeting set the stage for the great era of the German Diet that was to follow.

Bock contends that the proper approach to the affairs of the Reich is via foreign affairs, particularly the struggles of the House of Habsburg with France and Hungary. This inevitably biases the treatment in favor of Maximilian and against the more internally oriented interests of the estates of the Reich, which continuously opposed the "great power" aspirations of the Habsburgs. The desires of the reformers found in the estates had not yet been clearly formulated. The insufficiency of the existing machinery for supporting Reich goals was, however, patent in the fact that only about a third of the money levy voted by the Frankfurt Diet was ever paid. As Bock points out, the grand enterprise of the House of Austria was supported primarily by its own lands as well as by its credit dealings with the Fuggers, and only to a very modest extent did it draw on the Reich.

✓This double volume has over two hundred pages of clear, readable narrative introduction interspersed among almost twelve hundred pages of documentary material. The documents are subdivided into the prehistory of the Diet (roughly July 1488 to June 1489), the Diet itself, and the collection of the services and money voted at the assembly. The prehistory is treated with greater breadth than the other two sections, and in this section the documents are further subdivided by topic. As with the newer series of the Reichstagsakten, the texts are rarely given in full (though some of the protocols of the Diet are): rather they are summarized and excerpted with full bibliographical notes. In all, this will be a very useful tool for researchers seeking to understand the fifteenthcentury Reich on its own terms, and the introductions by themselves constitute an excellent monograph on the political life of Central Europe in the period. One cannot be called impatient for wanting to see the rest of the volumes printed as soon as possible.

STEVEN W. ROWAN
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JUAN CARRASCO PÉREZ. La población de Navarra en el siglo XIV. (Colección histórica de la Universidad de Navarra, 29.) Pamplona: Universidad de Navarra, Facultad de Filosofía y Letras. 1973. Pp. 703.

The major utility of this volume will be found in the 455 pages of previously unpublished documentary sources that it contains. These texts consist of five tax surveys, all deriving from the Archivo General de Navarra, of parts or the whole of the kingdom of Navarre between 1330 and 1366. They have been fully described and identified and laboriously printed.

The information they supply has been combined with that derived from five other Navarrese fiscal documents of the same period, all previously published, and distilled into a sixty-five page statistical appendix, which is the next most valuable part of the book. Much of this information is also presented graphically in the twenty-four maps integrated into the volume.

One can imagine the labor that went into the sheer collection and analysis of this material by the doctoral candidate (and this is indeed a published dissertation), but the remainder of this bulky tome is of quite uneven value. Two short sections on the population fluctuations and the social structure of Navarre during this period amount to about thirty-two pages combined. They are carefully and perhaps too cautiously done and yield some information valuable to the demographer about the effects of the Black Death, for example. They also add some interesting if highly localized, and thus particularized, data about population shifts in the fourteenth century both within the rural countryside and from the countryside toward the city. There are also some interesting figures provided on the numbers of Muslims and Jews in the kingdom and their geographical distri-

The real difficulty in finding a historical context in which to generalize this data is not addressed. The author was hardly ready to offer opinions as to the applicability of his findings outside the small, mountain kingdom of Navarre. But then it is precisely the inability of most doctoral candidates to handle such wider implications that makes the publication of most theses of doubtful utility. The author's brief sixteen-page essay on the development of historical demography and its methodological problems is little more than a survey of the literature. Similarly, his handling of his data is safe, sensible, and will add nothing that I can detect to the methodology of demographics.

BERNARD F. REILLY Villanova University

EDWARD A. ARMSTRONG. Saint Francis: Nature Mystic. The Derivation and Significance of the Nature Stories in the Franciscan Legend. (Hermeneutics: Studies in the History of Religions, 2.) Berkeley and Los Angeles: University of California Press. 1973. Pp. 270. §12.00.

The author of this book is a naturalist, ornithologist, and Anglican parson who has written books on bird behavior and on Shakespeare's imagination. Here, as before, he brings scientific knowledge, great learning, and an unusual ap-

proach to a well-worn theme. He is not a historian, but his scholarly instinct and his devotion to the subject make this not only a labor of love but a book from which the Franciscan scholar has much to learn. He describes the tradition of animal-loving saints before Francis's time, analyzes the various creatures and types of creature that enter the biographies and legends of St. Francis, and attempts, as many have before, to draw the whole theme together in a final chapter on the Canticle of Brother Sun.

There are many indications that Mr. Armstrong has not a historian's approach to this subject. He is brief and somewhat misleading on the Cathari, and fails to note the ways in which Francis propounded in thought, word, and action a direct denial of the Cathar doctrine that the material world was wholly evil (pp. 27, 135). Francis saw animals as God's creatures, and his treatment of them was often a parable to emphasize that the world is God's, and good. Mr. Armstrong's brief words on Joachim of Fiore (pp. 30-31, 63, etc.) are similarly superficial and ignore most of the recent literature stemming from Dr. Marjorie Reeves's seminal book. More serious, he confuses the sources, sometimes preferring the witness of the legendary Fioretti to Thomas of Celano (pp. 128-20) or the saint's companions (p. 42), or treating the Speculum Perfectionis as an original work; he makes little use of Francis's own writings. Yet he brings to his study an incisive and deep understanding of the natural world in which Francis lived, far beyond that normal in Francis's biographers or mere historians. He is an amateur in the best sense; his learning is genuine and wide, but it is the naturalist's patience and enthusiasm that breathes new life into many of these stories.

ROSALIND B. BROOKE London

v. d. Koroliuk et al., editors. Issledovaniia po istorii slavianskikh i balkanskikh narodov. Epokha srednevekov'ia. Kievskaia Rus' i ee slavianskie sosedi [Studies in the History of the Slavic and Balkan Nations. Era of the Middle Ages. Kievan Rus' and Its Slavic Neighbors]. (Akademiia Nauk SSSR, Institut slavianovedeniia i balkanistiki.) Moscow: Izdatel'stvo "Nauka." 1972. Pp. 288.

This volume is the first sbornik of the Division for the Study of Ancient and Medieval History of the Institute of Slavic and Balkan Studies at the Soviet Academy of Sciences. In the brief introduction the editors signal an intensifica-

tion of interest on the part of Soviet scholars in medieval studies of the Balkan Peninsula and the Danubian basin. The fifteen papers that follow are dedicated to the memory of B. D. Grekov, the first director of the Institute of Slavic Studies.

Six of the fifteen papers are strictly within the realm of archeology. With no exception they are concerned with an East Slavic ethnic and cultural presence in areas that are subject to a variety of interpretations. For those who are interested in the Slavic settlement of presentday Slovakia, a good recapitulation of literature on the subject is given by C. I. Peniak (pp. 68-77). Another six papers survey and reassess the historical problems. G. G. Litavrin suggests in his contribution that Yaroslav with his Russes raided Byzantium in 1043 in order to restore neighborly relations that had been disrupted by the participation of some Russes in an internal Byzantine struggle (p. 221). A different interpretation was given recently by Andrzej Poppe in Slavia Orientalia (1967). It seems that the controversy concerning the campaign of 1043 will continue because of the lack of convincing arguments or solid evidence.

Another topic that has evoked a large quantity of polemical literature is the problem of ethnic and political associations of the so-called Cities of Cherven/Czerwień and of the region of the town of Peremyshl/Przemysl during the ninth through the eleventh centuries. These regions were assigned by scholars either to Poland or to Kievan Rus, the division of opinion frequently reflecting national feelings on the part of the writer. However, Ia. D. Isaevich adds a new dimension to the controversy. He suggests a Czech component in the medieval history of the region and finds support for his reasoning in the description of the Prague bishopric from 1086, in which the eastern boundary of the diocese is given as running along the Bug and Styr rivers. Isaevich, furthermore, quotes the Chronicles of Dlugossius to show that the town of Peremyshl/Przemysl might have been founded by a Přemyslid (Boleslav II). Finally, Isaevich concludes on the basis of archeological material that the whole region was East Slavic. His reasoning would have be∈n more convincing had he brought forward some evidence to show that the Rus political superstructure in its material culture had more in common with the local population than the military-political superstructure of the Poles or the Czechs.

A significant contribution to the volume is by V. D. Koroliuk, the director of the newly created Division for the Study of Ancient and

Medieval (Slavic) History. His paper sets out the concrete tasks for research and suggests some methodological principles. Most interesting is Koroliuk's suggestion to treat the Balkans and the Carpathian basin as "contact zones" in which cultural, political, and ethnic encounters took place between the Slavs and non-Slavs. The idea of "contact zones" is not entirely new, but its concrete definition in this case contains some dangers of oversimplification. A "contact -zone" not only suggests but, in Koroliuk's words, presupposes the existence of a "nonsynthetic," culturally and ethnically pure zone -in this case, the area inhabited by the East Slavs, or, more precisely, by the Polane of Kiev (pp. 10, 16). The author stresses the active political role of the nomads in the "contact zone" between Eastern and Central Europe (p. 10). What had happened to the Northmen and Khazars in Eastern Europe? Another question presents itself when one reads that both the West European feudal states and the feudal system in Byzantium emerged as a direct result of Slavic pressure (p. 15).

The collection of studies concludes with a bibliographic survey of some 170 Russian and Soviet studies on relations between Kievan Rus and other medieval Slavic political formations.

The volume as a whole provides stimulating reading for scholars interested in Soviet scholarship devoted to the study of the proto- and early history of Eastern and Central Europe.

IMRE BOBA
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APOSTOLOS ATH. GLAVINAS. Hē epi Alexiou Komnēnou (1081–1118) peri hieron skeuon, keimēlion kai agion eikonon eris (1081–1095) [The Controversy during the Reign of Alexius Comnenus (1081–1118) over Sacred Vessels, Gems, and Holy Pictures (1081–1095)]. (Byzantine Texts and Studies, 6.) Thessaloniki: Center for Byzantine Studies; distrib. by Library Grigoris, Athens. 1972. Pp. 217.

Acceding, in 1081, to an empire that was paralyzed by economic dystrophy and military ataxia, Alexius Commenus resorted to drastic fiscal measures, among which his confiscation or compulsory borrowing of metallic church furnishings for coinage can be seen as relatively innocuous, from a purely economic viewpoint. Alexius was a pious ruler, and his embarrassment at the actions he was forced to take was felt to be genuine by many churchmen, but there was a more absolute party for whom reasons of state were insufficient, and in 1082 they exacted from

the emperor a chrysobull condemning his own actions and a promise of restitution. In the next military crisis, however, the emperor once again put his hand on church treasures. This blatant recidivism exasperated the metropolitan bishop, Leo of Chalcedon, into bringing against the emperor the awful charge of heretical iconoclasm.

Even his opponents admit that Leo had the virtues of saintly simplicity, but he also had the failings. He charged bullheaded through the subtleties of the traditional Orthodox defense of icons and produced one of the silliest arguments in the long history of Byzantine theological polemic. He proposed that substances in contact with sacred images were somehow altered so as to partake of something like a divine essence and that the substances themselves must therefore be accorded special reverence. That was simple idolatry and had to be suppressed. A synod was eventually called, and Leo yielded on every point, after which he was restored to his metropolitan seat. The members of the synod had not been afraid in the slightest that Leo might be a serious heresiarch, and if Leo really thought that Alexius was an iconoclast, he was even more of a fool than he appears. The quarrel and its resolution proved nothing, except perhaps that the Orthodox authorities, who could be pitiless when prosecuting a real case of heresy, were nonetheless able to be generous when dealing with a foolish old man.

To devote more than two hundred pages to such a case is an example of doctoral overkill, particularly when the bibliography shows that it has been thoroughly worked over already. Glavinas has gone after every possible thread of tenuous connection with a sad and rather trivial affair, and his book serves its primary purpose of introducing its author as a diligent and conscientious researcher. One would rather have seen half the number of pages devoted to a more significant subject.

PIERRE A. MACKAY
University of Washington

J. OTTO MAENCHEN-HELFEN. The World of the Huns: Studies in Their History and Culture. Edited by MAX KNIGHT. Berkeley and Los Angeles: University of California Press. 1973. Pp. xxix, 602. \$20.00.

The Huns are awesome adversaries, as formidable to chronicle today as to confront in battle at the time Attila scourged civilized society of the later Roman Empire. Having

left no written records of their own, they are known only from the hostile, often erroneous, accounts of contemporary and later writers in Latin or Greek, or from the tangible evidence of Hunnic artifacts: grave furniture, armor, utensils, ornaments.

The papers collected in this definitive synthesis bring together the fruits of a lifetime of research by a scholar who delved into classical, Slavic, and Asiatic sources and crisscrossed Europe, Russia, and Asia in pursuit of his "demonic" quarry. A model of historical detection, the book begins with a re-examination of such literary evidence as there is before turning to the political history of the Huns: their migration from the Don to the Danube, their subsequent rise to power under Attila, and their final collapse under his son Dengizich. Source criticism and discussions of chronology impinge without apology upon the narrative throughout. The remainder of the book consists of monographs on Hunnic economy and society, warfare and religion, art, racial characteristics and linguistic peculiarities, the whole enlightened by extensive use of archeological material-a benchmark in Hunnic studiesor advanced techniques of anthropology or linguistics. Maenchen-Helfen does not make easy reading and his exposition, for all its verve, will take some digesting even by specialists. But anyone willing to make the effort can hardly fail to be fired by the author's enthusiasm for his subject or to be impressed by the scholarship and good sense that has ordered, interpreted, and pieced together the myriad, fragmentary clues. The world of the Huns and their place in history, what manner of folk they were and how they lived, are here illuminated by a historian who had to be the master of so many other disciplines: archeology, art history-above all philology, with competence in the widest range of ancient and modern languages. His work must surely stand as the magisterial point de départ for all future study of this shadowy people.

At the time Maenchen-Helfen died in 1969 the manuscript was incomplete, its chapters in varying stages of completion with binders and folders bursting the seams of the author's study. The final version, indeed the appearance of any version at all, is due to many hands but particularly to the collaboration of scholar friends at the University of California, Berkeley, one of whom, Paul J. Alexander, has provided a helpful "background" to introduce the more general reader to the main body of the text. One shudders at the thought of

editing the Maenchen Papers, a task as intimidating as the Huns themselves.

DUNCAN FISHWICK
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FRANZ GEORG MAIER. Byzanz. (Fischer Weltgeschichte, 13.) Frankfurt am Main: Fischer Taschenbuch Verlag. 1973. Pp. 443. DM 6.80.

The Fischer Weltgeschichte consists of thirtyfour paperback volumes completed or projected encompassing the histories of the inhabited continents and the whole chronological range from prehistoric times to the twentieth century. The volume entitled Byzanz consists of seven chapters contributed by six scholars under the direction of Professor Franz Georg Maier. It covers, in some four hundred pages, the history of the Empire from Justinian I to the fall of Constantinople. The omission of the period from Constantine I to Justinian I is proper in the context of the series since coverage for the earliest period, which is otherwise usually treated in the general histories of Byzantium, can just as well be left to the volume dealing with the last stages of the Roman Empire proper. The argument that the period from Justinian to Heraclius constitutes the decisive phase in the evolution from Roman to Byzantine society is acceptable and implies no disregard for the elements of continuity. More general problems of the origins and nature of the Empire that are usually discussed in the context of the pre-Justinian transition period are adequately dealt with in the editor's excellent introductory chapter.

The chapters correspond to a widely accepted system of periodization: chapter 1, Justinian to Heraclius; 2, the Iconoclastic crisis; 4, Macedonian Renaissance; 5, Comnenian period; 6, Fourth Crusade and its consequences; 7, Paleologan period. Chapter 3, "Byzanz und die Slaven," carries up to the fifteenth century. In the context of the series the Byzantine impact on the Slavs is most conveniently dealt with in this volume. The Empire's relationships with its other neighbors are dealt with in other volumes of the series.

The bibliography is arranged under certain general headings as well as by chapter. It is quite complete down to 1972. There are no important omissions. The apparatus is otherwise nonexistent and little space is devoted to consideration of the state of Byzantine historiography and its outstanding problems. This is to be expected in a work of this kind, but

it would have been well to have included a brief essay on the quality and quantity of the sources. A smoothly flowing historical narrative can be deceptive to the layman, and some insight into scholarly process and analysis has an independent value.

The various contributions fit together well without redundancies or lacunae. The material is clearly presented in a manner that is simplified without being elementary. The material in each chapter is presented with due regard for the balance and interrelationship of the key elements. There is no tendency to include inessential and confusing detail. The analysis accords with the currently prevailing scholarly consensus and is presented with a minimum of qualification. There are no novel interpretations although the contributors naturally tend to have their own notions of priority with regard to the relative weight of the various elements of Eyzantine history and society.

Chapter 7 by Professor D. M. Nicol, the most distinguished and prolific scholar among the contributors, is perhaps the best. His narrative imparts a degree of vitality to the Paleologan era that other treatments often deny owing to an overwhelming impression of feebleness and the inevitability of final destruction. Nicol succeeds in conveying the more accurate impression of a continuous struggle for survival by resort to every available means.

Chapters 4 and 5 by Professor Winfried Hecht also stand out, although the emphasis on the strategic elements in the former chapter has not been carried over into the latter, in which the social and political conflicts pre-empt most of the space. This is the normal treatment of the period after Basil II, but it is not logical or consistent to neglect the military problems to such a degree since they have an independent importance. The chapter on the Slavs by Dr. Hans-Joachin Härtel is instructive although the Byzantinist will detect a number of minor errors (the title Caesar-Tsar obtained by the Bulgar Khans is not the equivalent of Basileus). Chapters 1 (Professor Maier), 2 (Dr. Judith Herin), and 6 (Hermann Beckedorf) are all adequate and professional.

In summary it may be said that Byzantinists and scholars in related fields will prefer Ostrogorsky's general history to this volume. On the other hand the nonspecialist would find it worthwhile, providing, of course, he reads German with facility. It would not be worthwhile to translate this volume out of the context of the series.

JOHN N. FRARY
Middlesex County College

NICÉTAS MAGISTROS. Lettres d'un exilé (928-946). Edited and translated with an introduction and notes by L. G. WESTERINK. (Le monde byzantin.) Paris: Éditions du Centre National de la Recherche Scientifique. 1973. Pp. 154. 53.50 fr.

This short book is the revised version of a course that Professor Westerink gave at the Collège de France in 1970-71. It is an excellent, careful, and thorough critical edition of the Greek text, a French translation and analysis of 'thirty-one letters of the former Byzantine Magistros Nicétas (born ca. 870, died no earlier than 946) while in political exile on the Bithynian Hellespont from 928 to 946. Westerink overcame many difficulties in preparing this edition. The letters, often without identification, were scattered in various manuscript collections of Greek letters. The editor established their authorship as well as the Greek text (primarily from Vienna, Vatican, and Bodleian manuscripts) and then placed them in chronological order. This was not an easy task, because the letters were written in a difficult and highly learned style, often with only the vaguest allusions to specific events. Westerink succeeded in identifying many literary allusions from Biblical and classical sources. This is the first critical edition of all known letters of Nicétas from all available manuscripts.

The editor has written a significant avantpropos that contains many valuable insights on general methodological problems and principles in editing Byzantine texts. In his introduction he provides a detailed biographical analysis of the career of Nicétas. He was an important Byzantine official whose daughter Sophia married Christopher, son of Romanos Lekapenos (emperor 919-44), not later than 912. After Romanos Lekapenos gained the throne, Nicétas was exiled and tonsured for allegedly urging his son-in-law, Christopher, to overthrow his father. Nicétas wrote most of these letters, in which he complained of the conditions of his exile, to prominent Byzantine officials, in particular Gregory Protoasekretis and John the Patrician and Mystikos, but even one to Emperor Constantine VII after the fall of the Lekapenid family. Westerink identifies this Nicétas as the author of the Life of Theoktistos of Lesbos. The detailed footnotes provide a summary of each letter as well as a conjecture about the date and elucidation of various problems. The editor has appended very useful prosopographical and philological tables, a list of Biblical and classical citations, and a list

of incipits. This book provides further documentation and clarification of the history of the early tenth century. It contributes to the understanding of the "Macedonian Renaissance," Byzantine epistolary style, and, of course, the career and mentality of Nicétas Magistros. Westerink has accomplished a difficult task in making these ornate and vague letters usable for other historical researchers.

WALTER EMIL KAEGI, JR. University of Chicago

Travaux et mémoires. Volume 5. (Histoire et civilisation de Byzance: Travaux et mémoires. Centre de Recherche d'Histoire et Civilisation de Byzance.) Paris: Éditions E. de Boccard. 1973. Pp. 410. 160 fr.

This volume is the fifth in a series that has been issued since 1965 by the Centre de Recherche d'Histoire et Civilisation de Byzance at Paris under the direction of Professor Paul Lemerle of the Collège de France. All told, it seems to conform to the excellent standards of scholarship set by the earlier volumes.

Like its predecessors, volume 5 includes works of various lengths, the longest one here (144 pages) being a study by Lemerle of the Paulicians in Asia Minor. This is followed by thirteen other articles among which are the following: Irène Sorlin discusses in a most illuminating fashion the transmission of Byzantine historical literature into Russia from the eleventh to the thirteenth centuries. J.-A. Foucault gives twelve unpublished chapters from the work of Nicephorus Uranus in the original Greek and a French translation. Léonidas Mavromatis disputes the traditional date of 1282 for the fall of Skoplje to the Serbs; and instead he maintains without being able to be more specific that it took place sometime in the 1290s, before 1297. Bariša Krekić draws on a Venetian document of 1396 to extend our knowledge of the connections of the Bulgarian Asenid dynasty in Byzantium. The numerous question marks appearing on a genealogical chart accompanying this article (p. 348) point out dramatically how much is still unknown about descent in that family. Jean-Pierre Sodini investigates a fragment found on Cyprus, which he says has been wrongly attributed to the era of Justinian I whereas more likely it came from the reign of Tiberius I. In line with his investigation Sodini has included a most useful chart (p. 383) comparing the imperial titles and designations used by the emperors Justinian I, Justin II, Tiberius I, Maurice, and Heraclius.

The articles described above are examples of the wide variety of subjects that this volume covers; other articles, not cited here, range from such subjects as geography to a four-teenth-century document dealing with the pronoia system.

Of all the contributions to this volume the most significant is surely Lemerle's study of the Paulicians in Asia Minor. Basing his work in large part on texts printed in Greek and translated into French in volume 4 of the series of Travaux et mémoires, Lemerle has greatly elucidated some previously obscure aspects of the history and teachings of the Paulician sect. Fortunately for the scholar, the article has been separately indexed within the volume making it a much easier reference tool. Lemerle has also included a thorough, annotated bibliography in which he at times disputes the conclusions of other authors. For example, he takes issue with some basic matters in Nina G. Garsoïan's The Paulician Heresy: A Study of the Origin and Development of Paulicianism in Armenia and the Eastern Provinces of the Byzantine Empire (1967). This is particularly significant because the Garsoïan work is well on its way to becoming a standard source on the Paulicians, as is evidenced by Arnold Toynbee's heavy reliance on it in his Constantine Porphyrogenitus and His World (1973), pages 652-84. Briefly, Lemerle takes the stand, for example, that among the Greek sources Peter of Sicily (Petrus Siculus) and Peter the Abbot (Petrus Hegumenus) were the same person and that he really did visit Tephrike in 869, a fact that has been questioned by Garsoïan even though Peter himself wrote about it in his History. In another example Lemerle questions the reliance placed on an eighteenth-century document to form the basis for the work edited and translated by F. C. Conybeare, The Key of Truth: A Manual of the Paulician Church of Armenia (1898). Conybeare's book, it might be noted, is used much more approvingly by Garsoïan and Toynbee.

Scholars in the past have not readily agreed on some aspects of the history of the Paulicians. Lemerle's conclusions should in all probability lead to further discussion and debate. The point is that his views are expressed with clarity and care—the same kind of care that marks his overall direction of the entire volume of Travaux et mémoires in which this work appears.

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## MODERN EUROPE

LOUISE CUYLER. The Emperor Maximilian I and Music. New York: Oxford University Press. 1973. Pp. xii, 257. \$34.50.

The musical history of imperial, ducal, and princely courts of the Renaissance has so far not attracted a great number of scholars, in spite of the wealth of materials both artistic and historical. A certain kind of mind is required for research in this area: that of a historian with an impeccable technique in dealing with original documents, archival and civil, and with a motivation that stems from a knowledge and love of the music of some special epoch. It is for this reason that a musicologist usually attacks the problem, armed with such skills as may be available in a discipline other than that of political or social history. If the results are occasionally disappointing, in this particular case they are remarkably successful, for Dr. Cuyler has devoted the first half of her study to the life and personality of Maximilian I, reserving musical aspects of his reign to the second half of the book.

This proves to be a generally practical division of materials, allowing the author to concentrate on one topic at a time. She describes Maximilian's many journeys with accuracy and insight, filling in the political and cultural background in the Netherlands with a sure hand, and she rightly draws attention to the happiness of his marriage with Mary of Burgundy and the profound influence of northern customs upon the remainder of his life. Although his second marriage, to Bianca Maria Sforza in 1493, appears to have brought him little in the way of comfort or companionship, it considerably enriched the Habsburg coffers and may indirectly have contributed toward the establishment of an improved team of court musicians. Another powerful factor was undoubtedly the acquisition of the Tyrol, whose resources both fiscal and musical made a considerable difference to Maximilian's life-style. Woodcuts depicting the musicians have come down to us in the well-known publication, Triumphzug des Kaisers Maximilian I, and several plates serve to decorate the present book. Reference is made only to the reprint of 1883-84 (Vienna), a footnote on page 48 mentioning that detailed information is to be found in chapter five. But here the reader finds only a few lines on page 85, with no hint of the fact that the originals date from about 1516, when artists from Augsburg and Nuremberg,

such as Kölderer, Springinklee, Beck, and Dürer, united their talents in homage to a great ruler.

The musical section presents scores and commentaries for several representative works by Isaac, Rener, Senfl, and of course Paul Hofhaimer, the organist who ranked as one of the greatest of his time and who obviously plays a solo role in the organ-chariot of the Triumphzug. The inclusion of a small gramophone record makes it possible to hear some of the music discussed and illustrated. The Kyrie from Isaac's Mass Magnae Deus Potentiae is given in a version with alternating vocal polyphony and organ settings, the chant being present in both choral and instrumental parts. Polyphonic songs by Senfl and Isaac and organ settings of Ein frölich Wesen round out this useful compendium of scores and performances, which taken together with the book help the reader to understand the notable achievements of musicians half a millennium distant from us, yet close enough to enchant us still with their fine compositions.

DENIS STEVENS
Columbia University

HANS J. HILLERBRAND. The World of the Reformation. New York: Charles Scribner's Sons. 1973. Pp. x, 229. \$10.00.

In this engrossing interpretation of the Reformation, Professor Hillerbrand addresses himself to numerous questions being raised today by sociologists and anthropologists as well as theologians and historians. Fully aware of the complexities of the period, he avoids all monolithic explanations in his attempt to provide students of the period with an approach that is both perceptive and dispassionate. He states at the outset that his objectives are to provide an outline of the course of events of the sixteenth century and to put these events into an interpretive frame of reference. Although he considers the Reformation the most important development in Europe during this period, he insists that one cannot understand it apart from its interrelationship with society as a whole.

After examining the status of religion in society in the early years of the sixteenth century to ascertain weaknesses and tensions that shed light on the causes of the religious upheaval, Hillerbrand gives a spirited account of the rise of Lutheranism, Zwinglianism, Anabaptism, and Calvinism; traces the political events that ultimately led to war and the

Peace of Augsburg in Germany; discusses the Reformation in England to the rise of Puritanism under Elizabeth I: devotes a chapter to the Catholic reaction to the formation of the Society of Jesus; and concludes with a summary of consequences of the Reformation, particularly with reference to the role of women and changes in government, society, and culture.

Among Hillerbrand's most helpful contributions to our understanding of the Reformation are his discussions of the "deepened spirituality" of the leading reformers during the early Reformation, his evaluation of the radical movements, and his summaries of the interrelation of Reformation and society, especially of Reformation and "regime." He concludes that the Reformation was successful only in those countries in which the governments supported it, as in certain German states, England, Sweden, and Scotland, and that governmental support was the consequence of a great variety of forces. He maintains that the Reformation influenced society primarily by furthering poor relief, improving education, and stressing the importance of the laity, emphases that fitted well with the increasing importance of secular authorities in religious affairs in Catholic as well as Protestant countries. Scholars may raise questions about some of Hillerbrand's generalizations but they will not ignore them.

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SALO WITTMAYER BARON. A Social and Religious History of the Jews: Late Middle Ages and Era of European Expansion, 1200–1650. Volume 15, Resettlement and Exploration. 2d rev. ed.; New York: Columbia University Press; Philadelphia: Jewish Publication Society of America. 1973. Pp. 550. \$15.00.

The fifteenth volume of Salo Baron's history of the Jews deals with what he calls "Resettlement and Exploration" in the sixteenth and seventeenth centuries. Four developments are examined: the emergence of a free Jewish society in the Netherlands; the slow acceptance of Jews in France and England; the deterioration of the situation of those of Jewish ancestry in Spain and Portugal; and the role of New Christians and crypto-Jews in the Spanish and Portuguese Empires. These themes are all fundamentally related to the fate of the Iberian Jews, forced either into exile or into an internal exile in their homeland, hunted and haunted by the Inquisition. Their quest for

security and for an opportunity for religious and economic expression took various forms depending on the circumstances they found themselves in. The results of this interaction, especially in the situation dealt with in this volume, established the bases for the Jewish communities in Western Europe and the New World and led to the first significant mixing of Jews and Christians in the modern world. This interaction was of major importance in the making of the modern world, both economically and intellectually.

The so-called Marrano dispersion and explosion into Western Europe has been the subject of a great amount of recent research. Professor Baron has put a vast collection of this material, both recent and older, into a systematic account. He starts by showing how the Dutch Jerusalem came into existence and developed. The Amsterdam Jewish community, consisting mainly of Iberian refugees, was the first free Jewish community in the West. The inpouring of escapees from the Inquisition and from Christian intolerance produced tremendous economic and intellectual ferment. The roles played by Marrano merchants and bankers in the Dutch economic miracle of the seventeenth century are gradually being appreciated. The intellectual role needs much more examination. A few figures, Da Costa, Menasseh ben Israel, and Spinoza, have been studied. They and many others like Morteira and Orobio de Castro played seminal roles in the cross-fertilization of ideas that took place in Holland, which were then contributed to the modern intellectual world. They were a unique group in Jewish history—Jewish intellectuals formed in the general currents of European Christian ideas, attempting to comprehend Judaism in seventeenth-century terms. Some came to reject Judaism, while others tried to express what was viable in the Jewish tradition in terms of seventeenth-century thought. They provided the basic data on Judaism for their non-Tewish contemporaries and interacted with them. Further evaluation of the role played by the Amsterdam community in the history of ideas needs to be done. The role of some of its thinkers in the Messianic movement of Shabbatai Zevi will be treated later on in Baron's volumes.

The unofficial Jewish groups and individuals in France and England played a lesser, but sometimes very significant, role. Although both countries officially banned Jews, Spenish and Portuguese refugees in small numbers came, almost always as "New Christians." Some

played important commercial, financial, and professional roles (including, in the case of Dr. Rodrigo Lopez, Queen Elizabeth's doctor). Baron puts together evidence to show that though the medieval prejudices and regulations did not disappear, there was a gradual social and political acceptance of Jews in France and England, which formed the basis for the modern communities in those countries. The source material used by Baron is occasionally doubtful, and some intriguing and suggestive material is omitted (such as Pierre Bayle's many indications of who was of Jewish origin or a secret Jew in France, and Richard Simon's data, including information from his Jewish friend, Jona Salvador, of an underground yeshiva functioning in Paris). Documents I have examined indicate that there were more underground Jewish activities in Bordeaux, Toulouse, and Montpellier than Baron's account suggests, and that these activities influenced Montaigne, his cousin, Francisco Sanches, his friend, and Etienne La Boétie among others. Also some recent research suggests that Postel was the actual author of Bodin's Heptaplomeres. This would account for the rich knowledge of Judaism in the work. (Also Bayle states that Michel de l'Hôpital was the son of a Jewish doctor.) Jewish ideas carried into southern France probably played an important role in the French Renaissance. Also, the New Christians in France seem to have generated a genuine Marrano theology: that is, neither Jewish nor Christian but Jewish Christian. La Peyrère, who is briefly treated by Baron, seems to have had a purely Marrano vision, and his pre-Adamite theory and Messianism greatly affected other Messianists as well as the budding Bible critics in Amsterdam, including young Spinoza.

The role of the Iberian refugees in England is clearer in the political sphere but, except for a few cases, is known only episodically. The role of Jews in the growing Hebraic concerns of the Puritans and Dissenters is perhaps better known. Baron composes the available data into a picture showing gradual acceptance of Jews in England. The crucial debate about resettlement of the Jews under Cromwell is postponed until a later volume.

The shift from the glimmerings of hope in Holland, France, and England to the increasing oppression of the remnants of the Sephardism in Iberia makes a most depressing picture. The Inquisition pressed onward in its campaign to force the New Christians not only into rigid Catholicism but also into a permanent second-

class status because of the taint of their Jewish ancestry. The perseverance of the victims has been one of the thrilling chapters in modern Jewish history. Many contemporary scholars of Spanish and Portuguese thought are engaged in the attempt to assess the role of New Christians in Iberia's Golden Age.

A question of current and excited controversy is how Jewish the Marranos were. Baron ignores the debate and accepts the evidence of the Inquisition trials, reports from refugees, and comments in Spanish and Portuguese literature of the time. These suggest that a crypto-Jewish world persisted, though deprived of any serious direct contact with Judaic sources. Others argue that the Inquisition created crypto-Judaism by forcing people to confess that it existed. Some evidence definitely suggests there was such a reality, while other data indicate that it existed only potentially and flourished only when New Christians escaped to Italy, Holland, or Turkey. My own present view is that the Inquisition, by making it impossible for New Christians to be just Christians, created a special consciousness in its potential victims who then must have sought ways of making their situation meaningful. Examples are Marrano theologies in which being a Jewish Christian was central to God's World; crypto-Jewish theologies in which adhering to any fragment of Jewish tradition was of enormous significance; and liberal and secular views in which Jewish and Christian views could be joined or even dropped.

It is difficult to judge whether New Christians were participating in Jewish, Marrano, Christian, or secular history, except in cases where they were forced by the Inquisition to defend their beliefs or when they escaped to Jewish communities outside of Iberia. The Inquisition's attempt to suppress them en bloc forced them to act and think in many ways that were to lead to the creation of new intellectual and social worlds.

This comes out most forcefully in Baron's last section, where he discusses what happened in the Spanish and Portuguese Empires, especially in the New World. Although the number of New Christians who came to America was small, the Marrano drama was probably played out more completely here than in Iberia. The Inquisition was late in being established and was never able to set up as complete a spy and police system. People could drift into the remoter parts of the vast, newly conquered territories and could often disguise their past. The opportunities for economic improvement

were great and the need for people to play productive economic roles still greater. Our main glimpse of what was going on comes from the Mexican and Peruvian Inquisitions' attempts to stamp out secret Judaism. The aweinspiring, self-constructed Judaism of Luis de Carvajal is probably the best documented case of what crypto-Judaism was like. The Complicade Grande of Peru indicates the extent of latent or secret Judaism among the entrepreneurs of the New World. The emergence of the first free Jewish community in the Western Hemisphere, in Dutch Brazil, indicates the economic, political, cultural, and religious links that could quickly develop between the flourishing Jewish world of Amsterdam and the latent one of colonial Brazil.

Baron, who usually leans to the most cautious side in assessing the evidence, is willing to consider some of the more avant-garde views about the Jewish role in the New World. He pleads for an open mind about the possibility, being argued by Cyrus Gordon, of a Semitic role in pre-Columbian America, and he tends to accept Seymour Liebman's evaluation of the important role of New Christians in the development of the Spanish and Portuguese colonies.

One suggestion in the material about the New World that deserves more examination is that New Spain may have been seen as a crucial part of the Messianic drama by many New Christians both in the New and Old Worlds. Long before the New England theologians gave their Messianic interpretation to their colonization, Spanish and Portuguese thinkers were interpreting the amazing conquests as a crucial stage in the Divine Drama. The fascination with the possibility that the Indians were the Lost Tribes; the attempts to save the Indians by the New Christian, Las Casas, and his followers and to construct an earthly paradise in Chiapas; the establishment of the Shrine of Guadeloupe, the site of the first and most important Divine event in the Western Hemisphere—all of this may be part of a pattern of the Messianic Age being intimately linked with the discovery and conquest of America.

Baron's fifteenth volume contains a wealth of riches on the Marrano experience in Western Europe, America, and the rest of the overseas Iberian empires. He has magisterially pulled together an incredible amount of material, available in so many different languages. The area covered, essentially the basic Jewish contact and involvement with Western European intellectual, social, and economic forces

in the beginning of the modern world, is one that has not been generally treated or appreciated. The book teems with data and suggestive possibilities. Since much fundamental research is going on in this area, many corrections and re-evaluations may soon be needed. Baron's later volumes on the developments of seventeenth-century Jewish Messianism and on Jewish emancipation will probably reflect this newer scholarship.

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ALAN G. R. SMITH. Science and Society in the Sixteenth and Seventeenth Centuries. New York: Science History Publications. 1972. Pp. 216. \$8.95.

This little book describes the way in which scientific discoveries of the sixteenth century were refined, expanded, and synthesized in the seventeenth century, in that rising crescendo that we have come to know as the scientific revolution. Men steeped in the tradition of Aristotle, Galen, and Ptolemy suddenly discovered they could no longer "save the appearances" of a world with which they had long been comfortable. As Herbert Butterfield has put it, man began to pick up questions "from the other end of the stick."

This age saw the development of many modern mathematical tools—logarithms, analytical geometry, calculus, and statistical analysis. It witnessed the invention of modern instrumentation—thermometer, barometer, vacuum pump, microscope, and telescope. Contributions of Bacon, Descartes, and Galileo became the foundation for modern scientific methodology. And at the end Newton seemed to tie a tidy ribbon around the neat package of the mechanical universe.

The revolutionary impact of new techniques, instruments, methodology, and discovery severely attacked the centrality of man and his world in the scheme of things. It became more difficult to see man as the "apple of God's eye," and the world as "God's footstool." Scientific ideas of the period contributed greatly to la crise de la conscience européenne, described in Paul Hazard's discussion of the secularization of thought in the late seventeenth and early eighteenth centuries.

Dr. Smith's work provides an overview of the scientific advances and the creative genius that characterized this age of revolution. It is greatly enriched by the inclusion of 135 pertinent illustrations. A short bibliography serves as a beginning guide to those who wish to read further and at greater depth.

The scholar who is well read in the field will find nothing here that is startling or new. He may choose to quarrel with certain interpretations. I find too much of the "great man" concept present in the argument that if Newton had not been present in the late seventeenth century, history might have been quite different. There are those who will hold, without detracting from Newton's tour de force, that the times demanded a Newton and that if it had not been Sir Isaac it would have been someone else who provided the grand synthesis as well as those who will not agree that the scientific revolution was really made by a small handful of men.

Those seeking an introduction to the age of science will find this work useful, but at the same time it will pose some minor problems. Many names are dropped in these few pages with little or no biographical or other background information. Hopefully this will serve as a stimulus to wider reading.

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HOWELL A. LLOYD. The Rouen Gampaign, 1590–1592: Politics, Warfare and the Early-Modern State. New York: Oxford University Press. 1973. Pp. xv, 215. \$16.00.

This is diplomatic history with a difference. An account of a single episode, the siege of Leagueheld Rouen in 1591-92 by a combined Anglo-French force under Henry IV and the earl of Essex, it is, despite the author's claim to be writing "old-fashioned political narrative," an analytical exploration of the interplay of internal and external politics, of administrative and intellectual history, of military and political processes, of personality and institutional structures, all focused on an important event. As narrative it does not quite succeed, for the exposition is too compressed and the thread of development too often broken: the author has not taken his skills from a Mattingly. We get too much detail that is not relevant to the flow of events, not enough to make it transparently clear. Yet what emerges, if flawed as storytelling, is still perceptive, informed, and illuminating. Circling round the episode of the siege itself, Lloyd examines the diplomacy and the military activity of England, France, Spain, and the Spanish Netherlands, as they were reflected in deliberations and decisions culminating in the investment and relief of Rouen. He rejects the

recent denigration of Elizabeth's policy by Charles Wilson in his Queen Elizabeth and the Revolt of the Netherlands (London, 1970), seeing her conduct in the Rouen affair as realistic in her evaluation of her resources, yet bold in using what she had to maximum effect. He also defends Henry IV against recent critics, not by making him out to be a great captain like his antagonist Alexander Farnese, duke of Parma, but as a master of politics who saw the defeat of Parma as the key to his own success or failure, and enough of a general to achieve by strategy what he could not do by tactics. The siege of Rouen failed thanks to Parma's relief, yet, Lloyd shows, it was his inability to follow it up with destruction of the French royal army that led up to his withdrawal and thus to Henry's reconquest of his capital and kingdom. The final chapter of the book is an examination of the significance of the campaign that emphasizes both the limitations of the sixteenth-century state in the conduct of diplomatic and military affairs, and the deep concern on the part of all the major rulers for legalism and legality alike. The former conclusion emerges directly from the content of the book; the latter, important as it is, is an obiter dictum that deserves a separate study in itself. Perhaps Lloyd will undertake it; this book suggests he would do it well.

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LUZI SCHUCAN. Das Nachleben von Basilius Magnus "ad adolescentes": Ein Beitrag zur Geschichte des christlichen Humanismus. (Travaux d'Humanisme et Renaissance, 133.) Geneva: Librairie Droz. 1973. Pp. 254.

It is one of the paradoxes in the history of literature that Basil the Great has been better known to a very wide circle of readers by one of his minor, nontheological works rather than by his major publications on theology on which his greatness rests. This minor work, "On How Young Men Could Profit from the Study of Greek Literature," is the theme of Schucan's treatise. This study was originally presented in 1971 as a dissertation at the University of Basel. It constitutes an important scholarly contribution to Christian humanism.

St. Basil, a deep student of both Christian and secular literature, thought that he could offer good guidance to the youth in the reading and appreciation of classical Greek literature. In a way this short but brilliant booklet marks

a sort of revival of the interest in classical antiquity. Schucan undertook to trace the manner in which this work was accepted in Byzantium, then transmitted into Western Europe through Italy. At first translated into Latin by Leonardo Bruni Aretino (1374–1444), it soon appeared in an Italian translation and was then followed by an editio princeps of the Greek text (1495). Eventually the work was received and studied fruitfully in Germany, the Lowlands, Spain, France, and England. According to Schucan, the booklet proved to be most influential and useful during the Renaissance, and quite helpful in the nineteenth century during the conflicts of school curricula.

In an appendix Schucan gives some manuscript statistics, which more than substantiate his thesis on the extent of the acceptance of Basil's work in Western Europe. Between the thirteenth and the seventeenth centuries 65 Greek manuscripts appeared. Of the Latin translation of Leonardo Bruni there are 306 manuscripts spread all over Europe and the United States.

In his closing paragraph Schucan expresses much surprise on discovering in the Mt. Athos manuscript catalogs an unusual phenomenon with respect to the number of manuscripts produced of this work between the seventeenth and nineteenth centuries: none in the sixteenth century; one in the seventeenth; twenty-five in the eighteenth; and two in the nineteenth. He asks the question whether the intellectually very conservative Mt. Athos had a humanistic movement during the eighteenth century. He finds the answer in the intellectual activity of Eugenios Voulgaris, to whom he attributes the founding of an academy. A more recent source than those Schucan cites, however, states that the Athonias Academy was founded in 1743 "thanks chiefly to the initiative of ... Neophytos Kafsokalyvitis . . . who became its first director." According to this source (C. Cavarnos, Modern Orthodox Saints: St. Cosmas Aitolos [1971], 61), Eugenios Voulgaris directed this academy from 1753 to 1758. The fact is that the Greeks in the eighteenth century were undergoing an intellectual reawakening under inspiring teachers in preparation for the Revolution of 1821, and the work of St. Basil proved to be most helpful for this unusual task.

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wolfgang müller et al. Die Kirche im Zeitalter des Absolutismus und der Aufklärung. (Handbuch der Kirchengeschichte, volume 5.) Freiburg: Herder. 1970. Pp. xxviii, 669.

A handbook of Church history in six or seven large volumes seems to be something of a contradiction in terms; and the valuable series in which this work appears, sponsored by German Catholic scholars, invites comparison with the standard French Histoire de l'Église, which it is now approaching in scope. Thus the present volume corresponds almost exactly to the nineteenth in the French series, by E. Preclin and E. Jarry, Les luttes politiques et doctrinales aux XVIIe et XVIIIe siècles, which appeared almost twenty years ago. It is less rich bibliographically, and it lacks the remarkable architectonic structure that makes the French series so convenient a work of reference. In the present more ecumenical climate it also lacks the implicit conviction of the earlier work that a history of "the Church" is simply a history of Roman Catholicism, and it includes chapters on Anglican spirituality and Russian Orthodoxy; on the other hand it gives almost no attention to the Protestant churches on the Continent, especially outside France. As its title suggests, it is also more oriented to politics than the French work. which found more space for Catholic spirituality, the religious orders, and the encounter with secular culture. On the other hand it gives fuller treatment to Catholicism in Central and Eastern Europe. Though it gives substantial attention to France, it is thus, in some respects, a more balanced work.

It also seems clearer about the general character of the period and the problems of presenting the history of the Church during this time. The century and a half from the Peace of Westphalia to the French Revolution is not, on the whole, an inspiring age in the development of Christianity. The influence of churches—the need to speak now in the plural is itself indicative of a new situation—over political, social, and cultural life entered into a decisive retreat that has continued to the present. Indeed, churches everywhere now tended to be subordinated to the interests of particular governments, as much in Catholicism through such movements as Gallicanism and Febronianism as in the Protestant state churches or in Russian Orthodoxy. The papacy itself was engaged in a constant and only partly successful effort to avoid becoming an instrument in the rivalries among the major Catholic powers. Thus, even in this work of Roman Catholic scholarship, the papacy fades into the background; no longer at the center of religious and ecclesiastical life, the popes are relegated to a few short and separate chapters.

The present volume is therefore not so much a history of "the Church" as a collection of the histories of particular churches, chiefly Roman Catholic. Accordingly it required a wider collaboration than its predecessors in the series, and its discrete chapters are the work of specialists of various national backgrounds. The late Louis Cognet was responsible for the chapters on France (nearly a third of the whole), Heribert Raab for the Church in Germany, Burkhart Schneider for the papacy, Quintín Aldea for Spain and Portugal, Patrick J. Corish for Britain and Ireland, Bernhard Stasiewski for the churches of Eastern Europe, Johannes Beckmann for overseas missions, and Wolfgang Müller for religious life and culture. As a result of this fragmentation, the editors found it necessary to provide a synoptic introduction to give some sense of the unity of the whole, though their emphasis on the underlying continuities of Catholic piety and on the need for a balanced estimate of the period that gives due place to its persistent spirituality is not altogether supported by the structure of the volume.

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KARL F. HELLEINER. Free Trade and Frustration: Anglo-Austrian Negotiations, 1860-70. [Toronto:] University of Toronto Press. 1973. Pp. viii, 152. \$12.50.

This book is a painstaking study, based on documents in both the Austrian State Archives and the British Public Record Office, of the negotiations leading up to the Anglo-Austrian commercial treaties of 1865 and 1869. Karl F. Helleiner here, as in his earlier book Imperial Loans: A Study in Financial and Diplomatic History (1965), works in an area where economics and international relations overlap. Much of the frustration felt by the negotiators in Free Trade and Frustration seems to be due precisely to this fact, for the Austrians and British had very different ideas of what economic diplomacy involved. The Austrian government, fresh from its humiliation in Italy, and moving toward an even greater humiliation in Germany, was primarily interested in friendly political relations. The British, triumphant after the conclusion of the Cobden-Chevalier Treaty with France in 1860, wished to extend its benefits to an ever-widening area of

Europe. In Austria it was primarily the Ministry of Foreign Affairs that pressed for continued talks, in the face of the protectionist views of the Ministry of Commerce, the rest of the cabinet, articulate business opinion, and after its creation in 1861, the Reichsrat. On the British side, while most of the negotiations were carried on by the regular ambassador, Lord Bloomfield, the zeal for free trade that inspired the discussions came from the Board of Trade, which provided the second negotiator, Louis Mallet, and the Chambers of Commerce, led by an M.P. from Newcastle-upon-Tyne with the improbable name of Somerset Beaumont. The British desires were nothing if not concrete: reduction of the Austrian export duty on rags to help the paper industry, lower tariffs in general, and, failing that, at least reduced duties on cottons and woolens. The Austrians, on the other hand, operated in an atmosphere of confusion, wishing to sign a treaty and yet evade the consequences of signing it. Helleiner concludes that in the end Britain did not fare too badly, considering how little she had to offer as a result of prior tariff reductions. The whole experience was enough, however, to dampen British enthusiasm for commercial conventions, and to convince free trade advocates of the tenacity of protectionism.

The book is charmingly written. With Somerset Beaumont charging into Vienna, first for free trade and then for the Barings, with such chapter headings as "A treaty at last," it creates a Trollopean atmosphere in which Phineas Finn and Madame Max Goesler would have been thoroughly at home.

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KEITH MALLORY and ARVID OTTAR. The Architecture of War. New York: Pantheon Books. 1973. Pp. 307. Cloth \$15.00, paper \$6.95.

Historians of military affairs will find little solace in the fact that not only are political scientists, sociologists, and anthropologists generally in the van of the so-called new military history but they have been joined recently by architects and city planners. As the trend toward an ecological approach to the field continues, Cliophytes would do well to pay attention to such works as Horst De La Croix's Military Considerations in City Planning: Fortifications (1971) and The Architecture of War. The latter work especially illustrates the influence of mil-

itary architecture on warfare and a contradiction between the applications of sophisticated technology and weaponry and the misapplications of discredited military strategy.

This work evolved from an initial study done at the University of Bath in 1970. The authors originally intended to identify a relationship between the development of modern architecture and military development in terms of both military construction and weaponry "spin-off." They readily admit that the dearth of secondary sources led them to restructure their outline and point toward a basic description of the development of military architecture in the twentieth century. Still, the authors admit that this remains anything but a definitive work. It treats Northwest Europe exclusively and only the period 1900-45. Such pertinent subjects as factories, granaries, and power stations during World War II are neglected, and military bridges remain absent from the discussion. In terms of fortifications, however, flak towers, bunkers, etc., the authors have contributed a pathfinding work. We see, for instance, how fixed fortifications in World War I were first discredited and then, with increased sophistication in weaponry and military theory, rehabilitated to the point where they dominated defense budgets between the wars. The fiction of the impregnable fortress-from Verdun through Maginot to the Atlantic and West walls deluded planners throughout Europe. Nonetheless, the orthodox fortifications led to unique and experimental structures—concrete bunkers for the citizenry, deep pens for submarinesand military utility even contributed to prefab housing. The superb illustrations show how many designs of fortifications have led to what might be termed the "bunker school" of architecture—an interesting facet of war's impact on civilization.

One of the study's conclusions literally begs for further study by interdisciplinary teams: "From an architectural point of view the interesting thing about military architecture is its responsiveness and adaptability to events—some would say more so than its civilian counterpart." Thus, if military-affairs historians should pay more attention to the mysteries of technology and technicians when analyzing policy and operations, one need but surmise similar dividends that can accrue for architectural, social, entrepreneurial, nay, even political and diplomatic practitioners in the discipline.

B. FRANKLIN COOLING
U.S. Army Military History
Research Collection

OSWALD HAUSER. England und das Dritte Reich: Eine dokumentierte Geschichte der englischdeutschen Beziehungen von 1933 bis 1939, auf Grundunveröffentlichter Akten aus dem britischen Staatsarchiv. Volume 1, 1933 bis 1936. Stuttgart: Seewald Verlag. 1972. Pp. 317. DM 38.

This first volume of a projected two-volume documentary history of Anglo-German relations throughout the period 1933–39 is divided into three main sections dealing respectively with the disarmament question in 1933–34, British efforts in the following year to negotiate a modus vivendi with Nazi Germany, and finally the reoccupation of the Rhineland and its consequences. An appendix reproducing thirty relevant documents, some translated into German, but most in the original English or French, occupies some fifty-four pages at the end of the volume.

Although we are promised a fresh appraisal of Anglo-German relations in the light of material from recently released papers in the Public Record Office, London, it is disappointing to record that very many of the British government and other documents cited by the author have already been published in such well-known collections as Documents on British Foreign Policy, Documents diplomatiques français, and not least Documents on German Foreign Policy. Professor Hauser's collection of lengthy German translations of British cabinet and Foreign Office papers offers little that is new concerning the origins and execution of British policy toward the National Socialist regime; nor will readers acquainted with the carefully detailed works of Jacobsen, Hillgruber, Hildebrand, or Wendt, to mention but a few who have déalt with aspects of German foreign policy in the interwar period, find significant additional insights on Anglo-German relations as seen from the German side. The author's contribution to the large and steadily growing literature on appeasement is unexceptionable but again scarcely novel: there were proponents of appeasement in all sectors of British public life, he concludes; their motives were often mixed; and England's overall weakness dictated both support of French demands for security and a simultaneous attempt to reach an accommodation on the entire range of contentious issues that envenomed relations with Berlin.

The book's episodic treatment of individual subjects makes it difficult to descry the outlines of any wood amid the familiar trees: even in a documentary collection such as this some consideration of nonofficial sources would have been welcome. It is, finally, somewhat less than

helpful to discover that many Foreign Office documents are cited solely by volume number or by incomplete document numbers, thus placing an unnecessary obstacle in the path of readers who may choose to attempt their own interpretations or indeed translations of individual documents or the often revealing minutes appended to them.

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E. E. RICH, editor. St Catherine's College, Cambridge, 1473-1973: A Volume of Essays to Commemorate the Quincentenary of the Foundation of the College. [Cambridge: St Catherine's College.] 1973. Pp. viii, 314. £6.50.

St. Catharine's College was established half a millennium ago. For most of its history it has been a small Cambridge college, much inferior in reputation to its wealthy and more glamorous neighbors. It began with a high-minded, spiritual-educational ideal, rather more high-minded than most educational ideals, but for a number of reasons was not always able to live up to the religious hopes of its founder. Today St. Catharine's is a flourishing college of moderate size. Probably at no point in its history has it been a more successful educational institution than in the twentieth century.

In commemoration of its five-hundredth anniversary, the retiring master of the college has edited a volume of essays covering various aspects of the foundation's history. He himself has contributed a long narrative, the longest in the volume, on the history of the college in the nineteenth century. Other contributions discuss the origins of the house and the reasons for its name, an interesting excursion in hagiography. There is an essay on the relationship of the college to the Church of England and one on the college silver, an indispensable part of all properly conceived Oxbridge college histories and usually containing surprises for the social historian.

Dr. Porter surveys the history of the college from the early sixteenth century to the midseventeenth century, making merry as he goes, scattering assorted jokes and allusions to the present. His essay roams widely and jumps rather too freely. But it contains many interesting asides and provides helpful references to current debates concerning the changing social structure of Tudor England. The final contribution by T. R. Henn brings the college history up to date. He has sensible things to say about

some of the developments that have altered the characteristics of student life in the past half century.

On the whole this quincentenary volume of essays follows closely the pattern of writing established for the college history genre. An exception is the analysis of Professor Oliver Mac-Donagh. The author of a notable contribution to the study of bureaucracy in Victorian England, MacDonagh has constructed a brief and valuable overview of the rise and fall of St. Catharine's in the nineteenth century. Rise and fall, he reminds us, are not solely attributable to internal vices and virtues. The historical factors favoring success in one period are different from those of another. Sometimes institutions are at fault for willfully refusing to take cognizance of changing circumstances and sometimes they are staffed by incompetents; but at other times their resources are simply inadequate to cope with fundamental departures from established practices and conditions. Seen in this light, state support for weakly endowed institutions like St. Catharine's has been essential to its survival in recent times.

The essays comprising this volume appeal to particular interests. Because an alumnus of Oxford or Cambridge remembers his college more than he recalls his university, the compilers of college history have always had a special public and can therefore dwell affectionately on the details of internal history. In addition college histories have always been invaluable to historians studying the development of universities. Oxford and Cambridge are the oldest surviving collegiate institutions in Europe, and the colleges, being separately founded and endowed, are sufficiently different from one another to justify separate treatment. But historians who have no familiarity with the peculiarities of Oxbridge local history will probably not be as interested in miscellaneous biographical information or discussions of building sites, college infighting, sermons in chapel, and the value of fellowship dividends as those whose lives are in some way tied up with the history of about half a hundred unique institutions.

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Berkeley

A. P. McGowan, editor. The Jacobean Commissions of Enquiry, 1608 and 1618. (Publications of the Navy Records Society, volume 116.) [London:] the Society. 1971. Pp. xxvii, 319. £4.50.

In early modern England the condition of the Royal Navy was a useful indicator to determine whether a regime was unduly corrupt by contemporary standards. For Elizabeth, as for Charles II and Walpole, the army, the legal and ecclesiastical hierarchies, even the fiscal administration, might legitimately be employed as sources of the patronage essential to the smooth operation of government. But the navy was a different matter, for upon it "under the good providence of God, the wealth, safety and strength of the kingdom chiefly depend." The navy was too vital to the defense of the realm and its burgeoning trade for its functional efficiency to be sacrificed, however importunate the creditors, the favorites, or the party leaders in the Commons.

Since the appearance of Oppenheim's study in 1896 it has been well known that between 1603 and 1618 even the navy lost its special status, and was corroded by the rampant favoritism and pervasive corruption that tainted every facet of James I's government. With Dr. McGowan's edition of the reports of the 1608 and 1618 Commissions of Inquiry the damage wrought by the heads of the naval administration, Sir Robert Mansell and Sir John Trevor, and their bevy of parasitic henchmen, secured from punishment by the benign and complacent protection of the king himself, can be documented in detail. It is a lurid record of peculation and swindle that is epitomized in the affair of the Resistance. The ship, though built and rigged at the king's expense, was owned by Mansell and Trevor, who leased it to the navy to be part of the fleet carrying the English ambassador to Spain in 1605: it was victualed from official stores, its crew was put on the navy payroll—and then it was employed in a highly remunerative mercantile venture by its owners. Besides its cargo of lead, surplus royal provisions and a couple of pieces of the king's ordnance were sold in Spain.

But, while the Commissions' reports make fascinating reading, the editor might have provided an even more illuminating account of the state of the navy in this period had he chosen to include transcripts of, or at least references to, some of the correspondence of Secretary Coke and, more particularly, of Cranfield, relating to the 1618 investigation. The temperate, even tame, official account takes on a new dimension if read in conjunction with Cranfield's scathing commentary. The rather narrow choice of materials is mirrowed in the introduction. McGowan has provided a very thorough survey of Jacobean naval administra-

tion, but some attempt to relate this to the more general investigations of early Stuart government undertaken by Menna Prestwich and Professor Aylmer, whose works are strangely neglected in the commentary, would have been welcome.

CLIVE HOLMES
Cornell University

ANTONIA FRASER. Cromwell: The Lord Protector. New York: Alfred A. Knopf. 1973. Pp. xx, 774. \$12.50.

This extremely detailed biography of Oliver Cromwell will come as something of a disappointment to that legion of admirers, both in the profession and among the general public, whom Lady Fraser acquired with her life of Mary Queen of Scots. It is not merely that the book is long. It is that it fails to do what Fraser did so skillfully in her previous work, to get inside her subject and allow the reader to see the world through that subject's eyes. Admittedly, the Lord Protector had a far more difficult and complicated personality than the Scottish queen, and, of course, his early years are far less well documented. The consequence is that with Cromwell we are always on the outside looking in. The portrait that emerges is one that is not unfamiliar: the hot-headed believer in a cause, who in middle life developed the attributes of statesmanship and military genius, and who was humane, tolerant, fond of music, an excellent family man-anything but the grim bluenose of the Puritan stereotype.

The portrait is convincing enough; I, for one, am prepared to accept Fraser's view of Cromwell's personality. The weakness of the book lies in its methodology. Fraser has opted to paint her picture in detail and at length, by telling us every single fact she deems relevant about Cromwell-warts and all; she does not gloss over Cromwell's blunders and faults, notably in Ireland. She sticks so closely to her subject, indeed, that until she reaches the last third of the book, which deals with the five years of the Protectorate, there is no room for the wider context-we know why Cromwell disliked the government of Charles I, but there is no discussion of the broader issues involved in the coming of the Civil War. The author in fact takes for granted a great deal of knowledge on the part of her readers. This is perhaps justified for her fellow Britons, but since the book is aimed at the general public, one wonders if the ordinary reader across the Atlantic will understand such things as the casual reference to the gentry controversy on page 13.

The book of course has many good features. It is well written and factually accurate. There are not many slips, and they are on peripheral matters like the massacre of Amboina, which is given two different dates, both wrong-though it is surprising to find the biographer of Mary Queen of Scots twice referring to the General Assembly of the Church of Scotland as the National Assembly (pp. 57, 68). The descriptions of Cromwell's battles are excellent, though that of Dunbar would have benefited from a chart. And for me the best chapter in the book, called "Grandeur," is a really fine description of Oliver's court as Lord Protector-with a stress on the trappings of royalty-and of society and culture in his capital. This is one chapter that all students of the period can read with profit. But on the whole the book seems to me to fall between two stools. Its interest for scholars working in the field of Civil War history is not very great, and in fairness to Fraser she is not writing for them. For her intended audience, the cultivated general reader, there is simply too much matter and not enough psychological excitement. Even granting the average man's appetite for history, particularly his own country's Civil War history, it seems likely that a much shorter study would have accomplished Fraser's purpose of "humanizing" Cromwell (p. xiii) much more effectively.

MAURICE LEE, JR.
Rutgers University,
New Brunswick

GERALD M. and LOIS O. STRAKA. A Certainty in the Succession. (The Borzoi History of England. Volume 4, 1640–1815.) New York: Alfred A. Knopf. 1973. Pp. xvii, 235. \$7.95.

According to the general editor, the volumes in this series have "taken shape around two concerns: giving scope to narrative, where the story of change was itself dramatic in social terms rather than in dynastic ones; and allowing room for more analytical work, where this seemed to point to an understanding of why changes took place." One could wish that the authors of this book had pursued these precepts with more rigorous attention to the data. Their grand strategy imposes an artificial distinction between two eras dividing at 1714, one described as "the epoch of the landed aristocracy and gentry," the second as "the epoch of the merchant prince and the industrial pioneer."

As their first chapter indicates, however, the aggregation of estates and the development of a wealthy landed oligarchy continued unchecked through the century after 1714—although, overlooking Mingay's splendid study of English landed society, they miss the chance to sketch its nature in firm outline. In other similar ways they fail to give shape to their story. For instance, there is no exposition of the constitutional and the commercial theories of the eighteenth century as the keys to understanding both the breach with the American colonies and much else of British history under George III; there is no clear explanation of what naval power meant to Great Britain during the French wars, 1793-1815, or of the ways in which the navy was used; the whole point of the Trafalgar campaign is missed. And not only is there a lack of major guidelines in understanding. Inaccuracy in detail abounds. Thus (pp. 124-25), in no English shire except Cumberland could "a lord . . . ensure both the nomination and election of the county's two representatives"; many rotten (and pocket) boroughs with small electorates had not "declined" but had been so since the time of their enfranchisement; not all English boroughs apart from London sent two representatives: five sent one only; Scottish and Welsh constituencies are overlooked; the borough of Haslemere was not in Lord Lonsdale's hands in Walpole's day; the duke of Newcastle (according to Namier's careful investigations) could nominate conditionally to about twelve parliamentary seats, not twentytwo, and only three of these were completely in his gift. Again (p. 160), the Society for Constitutional Information was set up in 1780, not 1770; neither it nor the Society of Supporters of the Bill of Rights included "working class people." Now and then the authors lapse into imaginative romantic absurdity. England's checkerboard of enclosed fields, though underexploited after the agricultural slump of the late nineteenth century, had not "reverted" to forest by 1900 (p. 19). The swinging phrase, "Wilkes, Wyvill and Reform" (p. 160), was never the "ear-rending chant" of a London radical rally: I am in the best position to know that it was coined in a London publisher's office in 1961; and this error displays elementary ignorance about Wilkes's career. Finally, the phrase "in the [House of] Commons" is correct usage: "in Commons" and "in Representatives" are alike ugly solecisms.

IAN R. CHRISTIE
University College London

ANTHONY ARMSTRONG. The Church of England, the Methodists and Society, 1700–1850. Totowa, N.J.: Rowman and Littlefield. 1973. Pp. 224. Cloth \$7.00, paper \$3.50.

The eighteenth century remains one of the "dark ages" of English religious history, and it may be a long time before a work of synthesis can be attempted. Anthony Armstrong's book has a more modest aim; apparently intended primarily for sixth formers and undergraduates, it is a short introductory account of the churches in England between 1700 and 1850, emphasizing the Methodist and Evangelical movements and their "impact on society."

While not based on new research, it draws skillfully on the accumulated research of the last few decades; the result is thoughtful and wide ranging. Thus the discussion of the Church of England in the eighteenth century takes up its doctrine, clergy, and churches as well as its constitutional position; the section on the Methodist revival explores the origins of the movement, its organization, chapels, worship, relations with the Church of England, opposition, and social influence. And the author continually exceeds his brief and offers his own assessments of debated questions: these, often shrewd and debatable, are the most interesting parts of the book.

Thus while it does not aim high, it has its virtues. Unlike most works in the field it deals with the whole array of churches (apart from the Roman Catholics) and not just one, and it is refreshingly free from denominational bias. It is pleasantly written, well illustrated with quotations from contemporary sources, and equipped with bibliographies. Yet it has serious shortcomings that cannot simply be ascribed to lack of space. The author views the churches from so great a psychological distance that he fails to enter their mental world: he misses the importance of the Methodist doctrine of "Christian perfection," and the Oxford Movement to him is merely the Church's "most curious defence" against the Whigs. Bunting and the high Methodist doctrine of the pastoral office fare little better. Despite the title of the book, society has only a walk-on part in the story—to receive the impact of religion; missing here are the newer conception of religion in society and the attempts to plot its social coordinates and to re-create its Sitz im Leben. And in treating the churches as primary, Armstrong ignores most of the world of popular religion. He deals faithfully and interestingly with the old questions and the old narrative but seems not to have noticed how recent

writers in this tradition have themselves undermined its assumptions and prepared the way for a new dispensation in religious history.

JAMES OBELKEVICH Princeton University

R. F. BRISSENDEN, editor. Studies in the Eighteenth Century. Volume 2, Papers Presented at the Second David Nichol Smith Memorial Seminar, Canberra, 1970. [Toronto:] University of Toronto Press. 1973. Pp. xiii, 419. \$15.00.

The rediscovery and revaluation of the literature of eighteenth-century England that began some forty of fifty years ago show no signs of flagging. Indeed, if we are to judge by the number of organizations, conferences, and seminars now devoted to the subject and still being formed and scheduled (one thinks of the healthy proliferation of regional branches of the interdisciplinary American Society for Eighteenth-Century Studies), there is little fear that the eighteenth century will ever again suffer the condescending dismissals of critics and observers like Macaulay, Arnold, and Thackeray, to name but a few. The David Nichol Smith Memorial Seminar met for the second time in Canberra in 1970 (the first meeting was in 1966, the third in 1973) and heard, among others, the twenty papers here printed, written by scholars from Australia and New Zealand, the United States and Canada, England, Scotland, and Germany, and proof not only that the revival of interest is international in scope but that the period has lost none of its attractions for some of the brightest minds in the world of scholarship. Although the majority of contributors are students of literature (there is a scattering of historians and librarians), the collection is gratifying proof of the continuing breakdown of older barriers between the disciplines. The writers here are seldom narrow in their interest or approach; even the topics that could easily be confined to purely literary matters are handled with a full sense of literature as an aspect of a larger context of philosophical, scientific, political, social, and economic thought and action.

In a short notice, one can do little more than point to a few highlights. Among those on the novelists I would place C. J. Rawson's perceptive analysis of the complex relationship of author and characters in *Jonathan Wild*, a work of art about life in which "life is imitating art"; John Carroll's examination of Richardson's revisions of *Clarissa*, a superb example of the light that textual analysis can throw upon meaning and authorial intention; and Roger

Robinson's evaluation of the "exuberant copiousness" of Fielding's narrative and descriptive techniques. Three essays deal particularly with matters in France: Robert Shackleton demonstrates the important influence of Pope's Essay on Man on the natural religion of the philosophes; J. J. Cashmere emphasizes that Bayle's skeptical tolerance was intended more to subvert the intolerance and dogmatism of formal religion than to support the radical criticism of society of some of his admirers; and James A. Leith's analysis of the arguments for and against mass education in France from 1750 to 1789 reads like a recapitulation of arguments we have all heard in our own time. Louis Landa continues his witty and learned explorations of the interplay between moral, economic, and literary concerns in the early eighteenth century, and four essays on Swift, by Paul Korshin, Michael Wilding, Gardner D. Stout, Jr., and Donald J. Greene add to some old controversies and open up some new. All in all, a most satisfying collection.

JOHN H. MIDDENDORF Columbia University

NEIL R. STOUT. The Royal Navy in America, 1760-1775: A Study of Enforcement of British Colonial Policy in the Era of the American Revolution. Annapolis: Naval Institute Press. 1973. Pp. ix, 227. \$12.50.

The focus of this monograph is accurately described in its subtitle. Professor Stout has analyzed and described the British navy as an instrument for enforcing imperial policy during the events that culminated in the War for American Independence. He provides another in a continuing series of monographs (such as John Shy's Toward Lexington: The Role of the British Army in the Coming of the American Revolution [1965]; Thomas Barrow's Trade and Empire: The British Customs Service in Colonial America, 1660-1775 [1967]), each of which, by close examination of a part of the whole, has enhanced our understanding of the formal apparatus Britain employed in managing her colonial affairs. This study primarily rests on the reports and correspondence of the officers who commanded the North American squadron of the British fleet, particularly Admirals Alexander Colville and John Montagu, and Commodore Samuel Hood. There is not-in fact, could not be-much here not already familiar; others have mined the Admiralty and other records Stout utilizes. It is good, however, to have the basic narrative of the navy's activities in American waters carefully related, and Stout offers some conclusions about the effectiveness of the Sea Guard in its enforcement activities. He believes that "as a deterrent to illicit trade, the Royal Navy easily outranked the customs service." He is less certain about the role the naval officers may have played in exacerbating relationships between the mother country and the provinces. He is convinced that the reports from those who served on the American station helped shape attitudes and ideas among the men who were directing Britain's policies. Stout assigns the British navy to a less-than-major position among the complex of irritants, institutions, and ideologies that generated rebellion in America. That seems reasonable from the evidence he presents. But he confuses that evaluation when he asserts: "If there had been no naval enforcement the revenue laws would not have worked, and the empire would have gone on as it had before 1763, with the colonies enjoying local autonomy. But the laws were enforced, and the Royal Navy was the most effective enforcement agency." Thus, while Stout explains what the British navy did in America, there is ambiguity in his assessment of what consequences should be attributed to those actions.

CARL UBBELOHDE

Case Western Reserve University

DOROTHY MARSHALL. Industrial England, 1776–1851. (Development of English Society.) New York: Charles Scribner's Sons. 1973. Pp. ix, 242. \$10.00.

This is a foundation volume for a series entitled "Development of English Society," produced in Great Britain. According to the editor, "It is intended to be a background book, not a text book, and as such the series should appeal to that increasingly wide circle of readers who, while not wanting to be bombarded by too much detail and too many facts, are interested in tracing back to its roots English society as we know it today" (p. vi). She adds, "It is designed for students," and it is appropriate here to consider the book for use in the classroom.

A good series for students would be very welcome, but this volume, at least, has three major flaws: parts are not helpful or meaningful to American students, it is dull, and it conceals rather than reveals the diversity of interpretation of the period.

It is not very useful because it does not seem to be written with the needs of American

students in mind. It has no map; it names places without explaining their location or even whether they are villages, towns, cities, counties, or regions; it casually names men without identifying them; and in other ways it assumes that very background it should be providing.

Dullness is embodied in the question the series is designed to answer, "How did we get from there to here?" (p. vi). The prose has some moments of real immediacy, and a few of the illustrative stories are very good. But mostly the book is as crisp as wet flannel. It does not give the sense of an act of creation but rather of a boiling up of old notes with a few suds of recent scholarship. The result is a very old-fashioned text dominated by the topics and approaches of two generations ago.

Finally, the book is disconcerting because it gives the students an interpretation of the period without being explicit about it and without letting them into the intellectual excitement of contending interpretations. It offers selected evidence while concealing the arguments. Moreover, the interpretation is an unreflective assertion that industrialization was making everything better and that any misery was purely coincidental and unavoidable. The implicit assumptions behind this book are so old that Dickens parodied them in the "Nobody's fault" of Little Dorrit; this is history the Circumlocution Office would have written.

THOMAS MILTON KEMNITZ
University of New Hampshire

ELEANOR FLEXNER. Mary Wollstonecraft: A Biography. New York: Coward, McCann and Geoghegan. 1972. Pp. 307. \$8.95.

Eleanor Flexner, author of the first scholarly account of the women's rights movement in the United States (Century of Struggle [1959]), has turned her fine talents to a study of Mary Wollstonecraft (1759-97), who is generally regarded as the intellectual inspiration of feminism both in Britain and in this country. "I must be independent . . . freedom, even uncertain freedom is dear," she declared. To escape a desolate home life with a thriftless, drunken father and an unsympathetic mother, she worked as paid companion, school teacher, and governess before embarking on a literary career in London. There she met some of the noted intellectuals of the time and published several important works, pre-eminently the classic Vindication of the Rights of Woman (1792), before venturing alone to Paris to observe the Revolution firsthand. Yet she also

said that "without someone to love this world is a desart [sic] to me," and she was ever seeking the love denied her in childhood, as in her frustrated affection for the painter Fuseli and her liaison with the American adventurer Gilbert Imlay, whose eventual rejection of her drove her twice to attempt suicide. With her, reason, "the heaven-lighted lamp in man" she called it, was constantly at war with her affectionate and possessive nature. At last she found an ideal partnership, both intellectual and passionate, with William Godwin, but a few months after their marriage she died in childbirth at the age of thirty-eight. The Wollstonecraft story has been told before by several writers, and Flexner acknowledges her debt to them. But a new study is justified, she says, because "so much new evidence has turned up to enrich our knowledge of Mary" (p. 268). Flexner makes good use of the Wollstonecraft-Godwin-Shelley family papers, from which she quotes extensively and effectively. She has also searched out other widely scattered sources, turning up some hitherto unpublished letters by Mary and other new material such as that relating to the Wollstonecraft family's finances. (A minor criticism here: some of this new material, relegated to the notes and six appendixes, could just as well have been incorporated into the text.) Still, the value of Flexner's work is due not so much to new material as to the interpretations and insights that she offers. Without burdening the reader with psychological jargon she gives an adept and thoroughly satisfying evaluation of her subject's character and conduct. Her work is meticulously researched, is written with compassionate understanding and in a style of rare grace, and is handsomely produced, with attractive illustrations and an eye-catching dust jacket portrait. It is altogether a fine book and a fitting tribute to an extraordinary woman.

LEE HOLCOMBE
Mystic, Connecticut

F. L. VAN HOLTHOON. The Road to Utopia: A Study of John Stuart Mill's Social Thought. (Speculum Historiale, 7.) Assen: Van Gorcum & Comp. N.V. 1971. Pp. 212. 36 gls.

This book, like most books about John Stuart Mill, testifies to the problematic nature of his thought. Even those commentators, and Professor van Holthoon is among them, who insist upon the "organic unity," the "extraordinary consistency," of Mill's work, have some difficulty in defining the unifying principle that

will make sense of the whole of his work and resolve the apparent inconsistencies. A few years ago J. M. Robson found such a principle in the idea of the "improvement of mankind." Now van Holthoon carries this idea one step further in the idea of "utopia." To be sure, he hastens to assure us, Mill had no blueprint for the future; but he did posit the unending progress of mankind, and he did have a vision of a "consensus" or "utopia" awaiting us at the end of the road. This utopia, as the author describes it, appears to be a liberalized version of the Comtean model. It would be a harmonious society with no unnecessary conflict, certainly no struggle for material goods; yet the harmony would not be so perfect as to foreclose further progress. Most notably it would involve a "consensus about the modern Art of Life." But this consensus would not preclude "experiments with the Art of Life"; indeed, the consensus itself would be the "basis for such experiments."

It may be that this is the best one can do by way of a single principle that will establish the unity and consistency of Mill's thought. The question is whether it does justice either to his individual works or to his thought as a whole. Does it, for example, adequately describe the most influential of his works, On Liberty? The author tries to bring this book within the compass of his thesis by drastically limiting Mill's idea of liberty, giving to the sphere of liberty "about equal weight" with that of society, or duty. At another point he limits liberty by the supposed "dictate" of truth: "It is clear that man should accept the dictate of truth—whether by friendly persuasion or despotic control-as soon as it is proved that his nonacceptance will do harm to others. In other words: liberty reigns where truth is not." This may be consistent with the author's idea of "utopia" and "consensus." But is it consistent with Mill's "absolute freedom of opinion and sentiment on all subjects," his insistence upon the right and need to dissent even in those cases where the truth has been absolutely established, or his repeated denunciations, in On Liberty, of conformity, custom, received opinion, and conventional behaviorin effect, of consensus?

GERTRUDE HIMMELFARB
City University of New York

R. J. OLNEY. Lincolnshire Politics, 1832–1885. (Oxford Historical Monographs.) New York: Oxford University Press. 1973. Pp. xii, 284. \$13.00.

Although Sir Lewis Namier urged us many years ago to look for the dynamics of English political life in the constituencies no less than in the House of Commons, he has not been that much heeded. Searching local studies have been few. For the nineteenth century one thinks of Norman Gash's chapter on Berkshire in his *Politics in the Age of Peel* (1953), of Richard Davis's study of Buckinghamshire 1760–1885, and of some important articles like T. J. Nossiter's on English urban constituencies from 1832 to 1868. Dr. Olney's book is therefore a welcome addition. It is hoped that others will follow him.

Nineteenth-century Lincolnshire was a large agricultural county, second in size only to Yorkshire among English counties. It was not, however, an overwhelmingly aristocratic countyabout twenty-eight per cent of its land area being taken up by estates of over ten thousand acres. Landowners, therefore, did not have it all their own way. "Lincolnshire politics between 1832 and 1885," Dr. Olney writes at the end of his book, "were farmers' politics. The real themes of those five decades were ones of direct relevance to the farming communityprotection, local taxation, the malt tax and so on" (p. 243). In the course of coming to this conclusion, and almost en passant, Olney sensibly takes issue with that current orthodoxy, the "deference" school (if one may so label it)-especially with Professor D. C. Moore-on such questions as the general nature of deference and the protectionist inclinations of highfarming agriculturists.

One cannot, however, refrain from uttering a grumble or two about this useful book. It is regrettable that Olney somewhat mars his case in presenting it. For one thing, his writing bears the leaden imprint of the Ph.D. The political narrative is dull. It could have been enlivened by making the author's thesis—that Lincolnshire farmers were not political ciphers -clear at the start, instead of burying it in the small print of the bibliography. For another thing, Olney somehow forgets that his thesis is by no means original. Two decades ago Norman Gash argued in his Politics in the Age of Peel that political influence, unlike water, flows upward as well as downward, that English tenant farmers were therefore active politicians, and that the force of public opinion was a rural no less than an urban phenomenon.

DAVID SPRING

Johns Hopkins University

L. F. TUPOLEVA. Sotsialisticheskoe dvizhenie v Anglii v 80-e gody XIX veha [The Socialist Movement in England in the 1880s]. (Akademiia Nauk SSSR, Institut Vseobshchei Istorii.) Moscow: Izdatel'stvo "Nauka," 1973. Pp. 261.

Neither the breath of life, nor the clash of debate; neither the radiance of socialism's ultimate vision, nor the noise of tumult in the streets, shows through here. The 1880s abound in high drama: H. M. Hyndman founding the Social Democratic Federation; riots terrifying the West End; the dockers' strike; the emergence of Keir Hardie. Between that scene, however, and the drab stage here toured under heavy ideological chaperonage, any resemblance is chronological. Led past fascinating organizations great and small—with pauses only for labeling—the reader encounters only names, dates, and Lilliputian feuds.

Though preoccupation with organizational trivia seems this work's first fault, its worst is "missed opportunity." Manifold chances ariseand each time are lost: to match argument with argument, in the authentic word of the contemporary; to portray actuality in the round, as many (not just Engels) saw it. But the tour must be spared contact with the natives; yesterday disappears behind today's safe clichés. The stock judgments, screening reader from event, make up in weight and monotony for lack of subtlety or depth. The recurring scenarios must be endured to be believed: entrenched opportunist reformist meets revolutionary; revolutionary explains the impossibility of reform, the inevitability of building a proletarian genuinely independent masses understand; cut off from the struggling workers by elitism, sectarianism, and dogmatism, reformist loses.

One remarkable scholarly breakthrough, however, suffices to make this book required reading: "proof by quote." Substantiation—whether of fact, causality, or evaluation—reduces to citation of Engels, Marx, or Lenin. How happily the presentation of truth has advanced, when one need but cite its guarantor! Offered this time-saving substitute for tedious argument, what churl would object that "in a dictatorship one does not think; one quotes"?

Comparisons, in a crowded field, must be selective. East Germany's Siegfried Buenger, in his Friedrich Engels und die britische sozialistische Bewegung von 1881-1895 (1962), seemed more analytical, more sensitive to human nuances. Stanley Pierson's new Marxism and the Origins of British Socialism: The Struggle for a New Consciousness (1972), bypassing ex-

ternal narrative (Tupoleva's staple), captures the inwardness: he sees Morris's ambivalence toward Marxism; he credits Hyndman with linking SDF and the new unions. Tupoleva's condemnations of Hyndman may soon be outmoded orthodoxy; praise-grudging, still heretical, but praise—is rising now for Hyndman. Can loyalism leave one stranded on tomorrow's deviationist siding? Meanwhile E. J. Hobsbawn concurs with Tupoleva that the SDF branches' vigorous autonomy saved them from Hyndman's domineering. Comparisons with E. P. Thompson's views must wait; his conclusions on Marx, Morris, and Engels seem in flux. The closest analogues are B. E. Kunina's Karl Marks i Angli'iskoe Rabochee Dvizhenie (1845-1883), and Natalia Prozorova's Bor'ba K. Marksa i F. Engel'sa protiv anarkhizma. This book adds nothing. It would break a genuine socialist's heart.

PAUL B. JOHNSON
Roosevelt University

FRANK MILLER TURNER. Between Science and Religion: The Reaction to Scientific Naturalism in Late Victorian England. (Yale Historical Publications, Miscellany, 100.) New Haven: Yale University Press. 1974. Pp. x, 273. \$12.50.

This is not a general history but rather particular studies of six thinkers (Henry Sidgwick, Alfred Wallace, Frederic Myers, George Romanes, Samuel Butler, and James Ward) who rejected Christianity and "scientific naturalism" also. Each essay is good in itself. I always wanted to know something about Sidgwick, and Turner tells me about as much as I wanted to know. He has bibliographies if you want to go further.

Turner's useful catchall, "scientific naturalism" (for Huxley and Co., Morley and Co., the anthropologists, the physiologists, and so on), corresponds to a reality of sorts: all were the enemy in the minds of the six. But it can lead to problems. Thus if Ward in 1896 attacked a "mechanistic and positivistic analysis of nature" (p. 228), he had done nothing to Darwinism, which is neither mechanistic nor positivistic.

What Turner's treatment makes clear is that each of the six was motivated emotionally, not intellectually. Each had his own strong desire: for Sidgwick, establishing Victorian morality logically; for Wallace, man's progress in humanitarianism; for Myers, his own immortality; for Romanes, the loveliness of the universe viewed religiously; for Butler, the recognition

of his own scientific theories; for Ward, the dominance of mind over environment. When each saw that Darwinism gave an answer he did not like, he did not modify his desire but rejected the received notion of science and its relation to Truth. Three (Romanes, Butler, and Ward, pp. 146, 182-83, 221) rejected the possibility or desirability of Truth itself. Each retreated to some Romantic position that had been argued in the first half of the century. Hence, no one since about 1914 has paid much attention to their positive work, except for the founding of the Society for Psychical Research. They functioned as wreckers, to prevent science from playing the same role in determining Truth that it had done since the time of Newton.

Thus Turner's treatment leads to quite important conclusions. His initial chapter on "scientific naturalism" itself is less satisfying, in part because historians of science have not re-done the subject. Annan's Leslie Stephen (1951) is the most recent necessary book, given that studies of Darwin himself and Robert Young's Mind, Brain, and Adaptation (1970) are only tangential to the subject. Turner believes that "scientific naturalism" is known to be wrong and wicked and that its pretensions were largely destroyed by Ward (pp. 202, 210, 246). This is an odd belief. It may hinder him in seeing still further into the significance of his subject.

W. F. CANNON Smithsonian Institution

EDWARD SCOBIE. Black Britannia: A History of Blacks in Britain. Chicago: Johnson Publishing Company. 1972. Pp. ix, 316. \$7.95.

This episodic book offers virtually nothing that is new for its first seven chapters, then for six chapters provides the fullest factual account yet available on the black experience in Britain, and then lapses for its final five chapters into summaries of events that are well known, but summaries nonetheless, informed by the author's own participation in some of the events described. The result is less a history of blacks in Britain than a hit-or-miss accumulation of interesting tidbits of information, personal opinions, and contemporary quotations, none of which are footnoted. There is no sustained analysis, and what there is is journalistic rather than historical; the conclusion on Enoch Powell is that "Britain's blacks are not taking [him] with passive indifference. Powell's anti-black

speeches have caused an awakening among them. They have now arrived at the bitter truth that all blacks are in the same racial boat." Virtually no manuscript sources are used, and tiny errors of nuance abound (St. John does not exist, Saint John and St. John's do). First names often are not given for the many figures that run upon this stage, and secondary material is used in unprofessional ways: we are told that an American sociologist, R. T. Lapiere, showed that in 1921 four per cent of the English were without prejudice, while sixtyseven per cent of the French were, and the source for this parody of sociological scholarship is shown to be an article not consulted but quoted from the Negro Yearbook for 1931-32. Compiling a list of stylistic solecisms, minor errors, and shopping lists of notable firsts would not serve to undermine the conclusion that the middle chapters of this book warrant its publication, and that the patient reader may gain valuable information from it, as well as insights into an interesting and dedicated man who has not decided whether he is writing for a scholarly or for a popular audience. For the former, one should turn to the two recent books by Jay Walvin; for the latter, Edward Scobie's effort may very well serve.

ROBIN W. WINKS Yale University

A. K. RUSSELL. Liberal Landslide: The General Election of 1906. (Elections and Administrations Series.) Hamden, Conn.: Archon Books. 1973. Pp. 260. \$12.50.

The general election of 1906 was one of those rare elections in which all the factors favored one party: the Conservatives had annoyed everyone in their last years in office but the Liberals had just formed a very talented government, the defense of free trade had united all the Liberals but tariff reform had had no unifying effect on the Conservatives, the 1902 Education Act had outraged the Nonconformists but the Liberals were able to conciliate the Catholic vote by some very mild and unalarming references to Home Rule. Dr. Russell's detailed study of the election confirms the impression that the Unionists did about as badly as possible; he shows that a great number of new voters came to support the Liberals, that many people who voted Conservative in 1900 must have changed their minds, and that the Labour Representation Committee drew an even larger percentage of the vote away from

the Conservatives than the Liberal candidates did. There is nothing to suggest that tariff reform was of any general benefit to the Conservatives: Russell shows that Conservative candidates did no better where Chamberlain had spoken than in other constituencies, that in the months before the election relatively few of them shifted to become clearly tariff reformers, and that whole-hoggers did not do particularly well in the election. It is hard to make so one-sided an election seem interesting in retrospect, even though it clearly gripped the public imagination at the time. Dr. Blewett has shown what a revealing study of the whole political scene can emerge from electoral study of a year of crosscurrents. If the election had been dominated by a great struggle of party leaders, this would have provided its own excitement, but 1906 was more of an aftermath of the Campbell-Bannerman v. Rosebery and the Balfour v. Chamberlain struggles than a contest of leaders in its own right. Russell has done the best he can with the election itself, but the real story of 1906 begins well before the dissolution (as is true of many other elections). Several recent books have helped clarify the intraparty struggles of 1901-05; there is now room for a book that brings all this together. It would be pleasant if Russell could write it, though he may not be able to spare the time from his present work in the Overseas Development Administration.

TREVOR LLOYD
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L. P. CARPENTER. G. D. H. Cole: An Intellectual Biography. (Conference on British Studies Biographical Series.) New York: Cambridge University Press. 1973. Pp. 271. \$14.95.

Cole is one of the small handful of socialist intellectuals who have achieved a significant influence within the British labor movement in the twentieth century. Thiry years younger than Sidney and Beatrice Webb, he was initially attracted to the Fabian Society at Oxford, but by the time of the First World War he had rejected its distinctive collectivist approach in favor of the ideas of guild socialism, which he did much to develop and for which he is chiefly remembered. But his disillusionment with these utopian hopes brought him back to a reformist position, from which he tried to influence the making of Labour party policy. The verse in which his friend Maurice Reckitt commented that he had "a Bolshevik soul in a Fabian muzzle" is not only witty but just; and Mr. Carpenter makes the most of it in his analysis of the tension between, on the one hand, Cole's lifelong adherence to the visionary ideals that lay behind guild socialism and, on the other, the resolute practicality with which he sought to ground the aspirations of the labor movement upon academically reputable social research.

The present book is devoted to a study of Cole's ideas and is based primarily upon his extensive published writings. It traces and expounds the several stages of his social and political thought with clarity and economy. These are not negligible virtues; and the author is at his best in the convincing and sympathetic chapter on guild socialism and in the percipient survey in the final chapter on audiences and values. The book, however, has some curious features that ought not to pass unremarked. It is odd that the author should have been content to publish "an intellectual biography" in 1973 that evidently has not benefited from consultation of Dame Margaret Cole's Life of her husband, which appeared in 1971; and this draws attention to a deficiency in respect of relevant personal information that has not been adequately remedied by the references (sometimes careless) to her own autobiography. For example, Cole's diabetes is clearly pertinent to his prolific output. Here the author is somewhat uneasy in his insistence that quality was not sacrificed to quantity, and he reproduces on no less than three occasions a passage in which Cole extenuated William Morris's voluminous production-presumably on the principle that "what I tell you three times is true." Readers of what Dame Margaret has to say will not be so easily convinced. The treatment of Cole's economic theories, notably in the fifth chapter, is also defective. The relationship between planning and Keynesian ideas could profitably have been discussed, possibly with reference to the work of Professor Donald Winch, which does not appear in the bibliography; and it is unfortunate that this chapter should conclude its remarks on Keynes with a reference to "J. A. Hobson's earlier explanation of the gap between savings and investment," which is neither accurate nor what Cole claimed.

These caveats do not detract from the substantial merits of the book, which will prove a serviceable guide to many aspects of Cole's work. In particular, it brings out the emphasis that Cole put on liberal values as intrinsic to the ends of socialism, and it is therefore able to make a plausible case for the continuity

of his thought through and beyond the guild socialist phase.

P. F. CLARKE University College London

A. J. SHERMAN. Island Refuge: Britain and Refugees from the Third Reich, 1935–1939. Berkeley and Los Angeles: University of California Press. 1973. Pp. 291. \$11.50.

This book should remain the most authoritative account of British policy toward the refugees from the Third Reich. It is well researched, relying in great measure upon the Cabinet Papers. Sherman is concerned with the several waves of refugees who desired entry into Britain, from the small contingent after 1933 until the floodgates opened with the destruction of Austria and Czechoslovakia. All along British policy was governed by several immutable principles: no public financial assistance, immigration must be limited with a view to national unemployment and pressure groups like the medical profession (which was similarly selfish in the United States), the wish not to offend Germany, and, finally, the desire to separate the Palestine problem from any discussion of refugee policy and settlement.

Sherman contends that Britain was not ungenerous toward the refugees. Certainly after July 1939 the public purse was finally opened and thousands of endangered children were admitted. Disenchantment with Germany apparently combined with hypocritical pressure from the United States, which did very little itself, to push Britain to revise previous attitudes. But it also emerges that men like Lord Halifax and Sir Samuel Hoare, about whom little good has been said since the war, showed a genuine humanitarian concern for the plight of the homeless. Britain admitted proportionately more refugees than France, not to mention the United States.

Sherman's treatment of the cabinet and civil service as largely faceless men is a handicap when it comes to R. M. Makins or Lord Winterton, who were crucial in the formulation of policy. What were their prejudices and attitudes, and beyond this, how did the only Jew in the cabinet, Leslie Hore-Belisha, react to this problem? We get glimpses of prejudice through memoranda (thus the governor of Kenya wants to admit only "nordic Jews"), but this is a history without people in an area where they do matter. For example, one can read into government actions and memoranda

that the English Jewish establishment behaved with a true concern for refugees, something entirely lacking in France, but the records of the Jewish organizations themselves, not to speak of private papers, have not yet really been exploited. Perhaps more seriously, within the book's context, little is said about non-Jewish political refugees other than those driven out by the Munich agreement. Again, we get a tantalizingly brief glimpse of attitudes toward political refugees when the Home Office admitted some Jews in a hurry so as to have an excuse to refuse several Communists.

This book, then, is a competent discussion of public policy, and it is good to have it. But it does not exhaust the topic, and it slights the complex level of motivation and behavior. In the end, I suppose, the tragedy that the book chronicles is relatively simple to explain: in a world of nation states there can be no room for those who have lost their old nation and not gained a new one in return. The exceptions to this rule are the rich and famous, but this book rightly deals with the many and not with the few "illustrious immigrants" who soon managed to find themselves a nation to belong to once more.

GEORGE L. MOSSE
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W. N. MEDLICOTT et al., editors. Documents on British Foreign Policy, 1919–1939. Second Series. Volume 13, Naval Policy and Defence Requirements, July 20, 1934–March 25, 1936. London: H.M. Stationery Office; distrib. by Pendragon House, Palo Alto, Calif. 1973. Pp. lxxviii, 945. \$42.00.

Faced with nearly a thousand pages of Foreign Office documents, I can only echo the pertinent observation of Professor Warren Kimball when dealing with Foreign Relations volumes in the December 1973 issue of this journal: that what follows must be "vague, impressionistic, and all too uncritical." It is also necessary to follow Kimball in warning that volumes of Foreign Office papers, no more than those of the State Department, cannot provide a complete understanding of the foreign relations of the country concerned. One is reminded of a portion of the eager youth toiling away in the Public Record Office whose theses should be headed, not "Anglo-Ruritanian Relations," but "Some Foreign Office Aspects of British Policies toward Ruritania." In the present case

students must, for example, turn also to the records of the cabinet and its committees, to the Board of Admiralty, numerous public and private individuals, and, not least, to that area where domestic and foreign policy issues are closely interwoven—to the League of Nations Union and Peace Ballot, say, and to Lancashire, with its demands for protection in Asian markets. Forthcoming secondary sources will also make an important contribution for those unable to reach the archives: Captain Stephen Roskill's third volume of Hankey, for one, together with the second volume of his Naval Policy Between The Wars and the new life of Chamberlain by Professor David Dilks.

Despite these qualifying observations, however, one must at once go on to emphasize that this is an extremely important and admirably produced volume—and one that prints, incidentally, a useful number of minutes as well as telegrams. It covers a crucial period that saw the beginning of what Professor Coral Bell has called the "crisis-slide" toward World War II. Above all, it provides excellent material on the interrelationship of the problems facing Britain in Europe and in the Far East.

The volume has two main themes: the naval negotiations, mainly between Britain, Japan, and the United States, which culminated in the patched-up London Conference of 1935-36, and the attempts, also involving naval issues, to restrain by sober parley and civilized pacts the primitive threat of Nazi Germany. Within this broad pattern one can only summarize some of the more important individual threads. On the naval side there is, for example, the vain hope of the Admiralty and Foreign Office that Japan might agree to the principle of "equal power of defence in the Pacific" following her denunciation of the Washington agreement; the anxious and sometimes acerbic discussions with Norman Davis and the State Department over the possibility of harmonizing the British and American approaches; the juggling with quantitative and qualitative limitations both before and after Japan's withdrawal from the conference in January 1936, with qualitative restrictions being embodied in the Anglo-French-American treaty of the following March.

Behind such technical issues there lay, of course, political maneuverings of potentially enormous significance. The Japanese were making a play for either what their foreign minister called "an essential understanding with Britain and the United States" or what another Tokyo official described as "a political agreement with

Great Britain." In London this latter possibility was highly attractive to that formidable Treasury partnership of Neville Chamberlain and Sir Warren Fisher. To the Foreign Office, on the other hand, such a pact seemed likely to "increase the chances of a Russian-Japanese war and of a weakening of Russia, and will entail . . . violent Chinese resentment against us, a diminution of the authority of the League [sic] and most likely a worsening of our relations with the United States." However, Chamberlain's hand was strengthened by Roosevelt, who, while declaring privately that the Japanese "were pursuing a very long-distance expansion in Asia," warned that "anything in the nature of a definite commitment on the part of the United States was impossible." Eventually a compromise proposal in the shape of an Anglo-Japanese nonaggression pact was presented to the cabinet by Chamberlain and Simon jointly, although this blithely ignored the trade war with Japan in which Chamberlain was a leading general.

Meanwhile naval negotiations had also to embrace the troubled French, the posturing Italians, and, bilaterally, the Germans: without an agreement on limitation for Germany the Admiralty was not prepared to sign elsewhere with the United States, and when Berlin offered a thirty-five per cent ratio with the Royal Navy it was seized upon with relief, "having regard to past history and to Germany's known capacity to become at will a serious naval rival of this country." "I earnestly hope," wrote Sir Eric Phipps from Berlin around this time, "that His Majesty's Government will not be deterred by the mere contemplation of Herr Hitler's past misdeeds or breaches of faith. After all, he now leads nearly 70 millions of industrious, efficient and courageous, not to say pugnacious, people." An air pact with the man was seen as perhaps an even greater prize, although it was an issue that gave rise to sharp disagreements over Germany's current and potential strength between the Air Ministry and Sir Robert Vansittart, who fiercely rejected the notion "that Germany will not be ready for war before 1942."

To preoccupations such as these must be added the approaching Rhineland crisis and the repercussions of the Franco-Soviet pact, French hostility toward the Anglo-German naval agreement, and Anglo-French divergencies over Abyssinia. Seldom has there been a more rapidly shifting, perplexing, and ominous period for those charged with the conduct of British foreign policy. When studying it, the

present volume will henceforth be an indispensable, though incomplete, source.

CHRISTOPHER THORNE University of Sussex

SIR LLEWELLYN WOODWARD. British Foreign Policy in the Second World War. Volume 3. (History of the Second World War.) London: H.M. Stationery Office; distrib. by Pendragon House, Redwood City, Calif. 1971. Pp. xiv, 620. \$20.00.

Woodward's third volume (review of volume 1 is in AHR, 76 (1971): 1553-54) is largely devoted to Britain and Europe in the last two years of the war. As a matter of exception, the treatment of Britain's Greek policy, detailed in some fifty pages, begins in 1941 and continues until mid-1945. Greece is exceptional as well in that it did not pass under Soviet or local communist control: British authorities, who had long contemplated the growth of communist influence in Greece, acted there with decisive repression when British forces were challenged. That simple statement, of course, has been voluminously disputed in all of its terms and from a variety of perspectives.

Woodward wrote these volumes originally for use by the Foreign Office. The period of composition, extending from 1942 to 1950, is very close to the events. Given the professional experience, scrupulousness, and intelligence of the author—he claims to have had free access, though I doubt this without some limitations, to the Foreign Office's papers and, as needed, those of the cabinet and other departments—closeness and near contemporaneity are the unique qualities of this story of Britain's adaptation to war, revolutions, and the superpowers.

Ceaseless and hopeful striving characterize the British effort to establish harmonious relations with the Soviet Union on Poland and Eastern Europe. In Yugoslavia the Chetniks and for that matter King Peter lose out to Tito and the Partisans of whom the British representative, Fitzroy Maclean, wrote accounts that were compared to prospectuses for a company. And French problems were hardly less refractory: de Gaulle, himself without power and claiming independence, outraged the stubborn "new-boy" Franklin Roosevelt, whose views Britain would out of necessity or dependence support, as Churchill argued to de Gaulle. This support of Roosevelt by Churchill prepared the general's future blackballing of Britain as a member of the Common Market.

This is a Foreign Office view of the world

and as such the volumes are valuable. As a period-piece of history the present volume is a humbling and instructive revelation of the limitations of human action and judgment. The tragic element encompasses the sufferings of victims as well as the frustration of the finite actors who somehow must balance and live with policies shaped by dictates of honor, interest, morality, responsibility, and considerations of power rarely adequate for the task. I found the present volume very moving reading because of its dated quality. Woodward's volume may also serve as a guide to Foreign Office, Cabinet, and Churchill Papers and offer a multitude of leads to the study of British policy and its agents as well as to their foreign counterparts and the situations of which they formed part.

MATTHEW A. FITZSIMONS
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F. X. MARTIN and F. J. BYRNE, editors. The Scholar Revolutionary: Eoin MacNeill, 1867–1945, and the Making of the New Ireland. New York: Barnes and Noble. 1973. Pp. 429. \$13.50.

When Eoin MacNeill wrote: "Neither apathy nor antipathy can ever bring out the truth of history," he expressed a devotion to Gaelic studies that made him both an outstanding scholar and nationalist leader in early twentiethcentury Ireland. This collection of twelve articles by historians and philologists, some of whom knew MacNeill well, is an impressive but not wholly successful effort to evaluate his academic and political careers. The essays on academe offer a well-rounded description of MacNeill's contributions as a linguist and historian, although some of them are obviously intended for Gaelic specialists. But the treatment of politics leaves much to be desired. Only one of three articles considers MacNeill's role during the revolutionary decade from 1914 to 1924, while the other two take half the book to explore the opening and closing events of his political life. Geoffrey Hand's discussion of the Boundary Commission fiasco contains much valuable information, but is this the place for such a detailed study, especially when its focus is the commission rather than MacNeill himself? The same criticism applies more strongly to F. X. Martin's discursive account of the foundation of the Volunteers, which draws heavily on his Irish Volunteers (1963).

Despite this imbalance, the book does make some important points. Taken together, the articles provide a good explanation of MacNeill's nationalist philosophy, emphasizing his insistence on the priority of cultural over political nationalism—the "nation" over the "state." The authors also make a convincing case that Mac-Neill cannot justly be regarded as a failure, notwithstanding his political misfortunes and his disenchantment with the "new Ireland" that emerged from the revolution. MacNeill's intelligence and energy made him a pioneer of the Gaelic Revival and Irish historical studies, while his political commitments helped to create an independent and stable Irish state. Furthermore, although MacNeill has often been censured because of his stand on certain critical issues during his political career, these essays remind us that debate on such questions is still open. W chronology of the subject's life, a bibliography of his published writings, and selected extracts from them enhance the usefulness of this work.

> JOSEPH M. CURRAN Le Moyne College

PIERRE ROCOLLE. 2000 ans de fortification française. Volume 1, Texte; volume 2, Croquis—bibliographie—index. Paris: Charles-Lavauzelle. 1973. Pp. xxiii, 365; 262. 180 fr. the set.

Colonel Rocolle, continuing a nineteenth-century tradition in which histories of fortification were generally written by military men, proves that soldiers can also be good historians. For his history of French fortification, Rocolle has collected a vast amount of material that he manages to present in a systematic and well-ordered manner. Very little of the French literature on his subject seems to have escaped the author's attention, and his rich footnotes and extensive bibliography make this work a dependable reference for future students in his field.

The book's early chapters, dealing with ancient fortification, are perhaps its weakest, as they are based largely on secondary, and not always the most recent, sources. This can be excused since the material is marginal and introductory to the main subject of the book, namely French fortification. From the Middle Ages on, the author finds himself on solid ground and, with few exceptions, traces vividly a continuous and convincing development from crusader's castles to the Maginot Line. Technological changes and advances in construction methods and fortification designs are presented in a rich framework of historical settings, and with constant references to contemporary changes in strategy, weaponry, and siege methods. All this is reinforced with rarely found and fascinating references, often in tabular form, to construction times and costs (calculated in percentages of ducal or royal incomes), methods of financing, garrison sizes, types of armament, and siege durations.

Of course not everything written by Rocolle is beyond debate. His attempt to deflate the still current nineteenth-century theory that many of the innovations of the twelfth century in the French art of fortification were imported from the Near East by returning crusaders fails to convince me. And a lengthy discussion of the possible origins of the machicoulis gallery, which Rocolle wants to derive from wall articulations of Romanesque churches, is interesting but inconclusive. Also, while the author gives full credit to Italian engineers for the revolutionary fifteenth- and sixteenth-century innovations in fortification design, his section on these Italian developments seems a little spotty. Some transitional Italian monuments (for example, Soncino and Bracciano) are misdated and introduced out of chronological context. And with the citation of Francesco de Marchi's important treatise only to discredit the Italian claim of his influence on Vauban, the author exhibits a bit of national bias that, fortunately, is very rare in this otherwise remarkably objective book.

But with Vauban, Rocolle re-enters his own backyard, and his analysis of Vauban's great fortification projects for Louis XIV is exemplary in its circumspect treatment of the monuments within and as part of their political, strategic, and logistical determinants.

The last third of the book lucidly deals with the nineteenth and twentieth centuries. Complex problems of design arising from such inventions as the rifled gun barrel and developments of reinforced concrete and armor plate are described in nontechnical, easily understood language. Toward the end Rocolle becomes an apologist for the Maginot Line, and (perhaps a surprise to a reader living in the atomic age) he concludes that permanent fortification is not obsolete despite the apparent failure of both the Maginot and the Siegfried Lines during the Second World War. No fortress has ever been able to resist a determined aggressor indefinitely, Rocolle writes; it is successful if it fulfills the purpose for which it was designed; and the purpose of permanent fortifications never was more than to delay an aggressor until sufficient forces could be mobilized to repel and defeat the enemy in open field battle.

Rocolle's illustrations are a real labor of love, as most of them seem to have been hand-drawn by the author himself. A substantial number of these drawings appear to be copies from photographs. To be sure, drawings can be more instructive than photos, as unimportant features can be deleted and important ones emphasized. But the interested reader cannot help but wonder how many, if any, of the structures shown are still standing to be seen and studied in situ. While an inventory of extant monuments certainly was not the purpose of Rocolle's work, a few photographs might have given a more topical flavor to an otherwise excellent and most valuable study.

HORST DE LA CROIX
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YVES CAZAUX. Jeanne d'Albret. Paris: Éditions Albin Michel. 1973. Pp. 412. 35 fr.

It is only a slight oversimplification to say that two distinct historical cultures coexist in contemporary France. One is marked by a self-consciously scientific outlook and has adopted the most advanced scholarly techniques; the other tends to be absolutely retrograde both in terms of method and analysis. Cazaux's book on the life of Jeanne d'Albret is representative of the latter school and one of a seemingly endless parade of popular biographies that dramatize, but do not illuminate, their subjects.

Although Cazaux has explored some of the archival materials relating to the career of the queen of Navarre and lists the principal secondary sources in his bibliography, he has not managed to break away from the standard formula that is imposed upon popular French historical writing—a thin narrative line, a shallow psychology ("With the exciting revelation of carnal love, Jeanne had just regained her total liberty."), and, at the end, a small moral lesson on the human condition. Significant discussion of the great issues of religion, politics, and war never really penetrates the smooth surface of this superficial work. Nor does the author probe deeply the character, motivations, or qualities of leadership of this shrewd and courageous woman. Time and again thoughts and feelings are attributed to the queen that are not supported by direct references to the sources.

One can only conclude that this biography, which is totally sympathetic to its subject, detracts from Jeanne's historical reputation. Seri-

ous students of the period will continue to rely on Nancy Lyman Roelker's outstanding study of the queen of Navarre.

RAYMOND F. KIERSTEAD University of Texas

JULIAN DENT. Crisis in Finance: Crown, Financiers and Society in Seventeenth-Century France. New York: St. Martin's Press. 1973. Pp. 288. \$14.95.

This book deals with the role of financiers in Old Regime France, especially in the middle years of the seventeenth century. The author asserts that by 1661 the older financial machinery had become in essence simply a front behind which "financiers came to dominate the financial administration of the state, parcelling it up into private fiefs . . . redolent more of a low species of bastard feudalism than of an absolutist monarch." The book is in two parts. The first describes the structure of finance, both the fiscal forms inherited from the mid-fifteenth century and the newer bewildering assortment of expedients contrived by corrupt financiers to raise funds for hard-pressed monarchs. The second, and original, part presents the results of a computer study of 744 people involved in seventeenth-century finance, along with 810 people related to the first group by blood, marriage, or business interest. Some 750,000 separate questions were said to have been asked of the people involved. To a considerable extent the answers give flesh and bone to a very ghostly occupational group. The chapters "Success and Failure," "The Uses of Power," "Relationships Between the Financiers and the World at Large," and "The Family, Marriage and Social Mobility" will perhaps not make financiers any more attractive than they have been in the past, but they will at least give them substance. For example, data are presented confirming the suspicion that a financier's life was very hazardous: forty-six men in the sample group suffered "grave misfortune" and had their careers terminated by "ruin, imprisonment, exile, or a combination of these things." Probably because of the uncertainties of the business, financiers spent their earnings at a frenetic pace, which, of course, helps explain their unpopularity in contemporary society. Dent provides data on the construction of Parisian houses and rural châteaux, favorite outlets for the gains of the financiers. Of the artistically significant houses built in Paris from the later sixteenth century to the later seventeenth, Dent's figures show that about half were built

by persons involved in some way in the royal finances. Many other interesting nuggets are yielded by the computer, such as the revelation that a daughter of high finances was six-and-a-half times less likely to enter the religious life than was a daughter of a modern administrative man, and ten times less likely than was the daughter of a magistrate of one of the sovereign courts (the author sees no obvious explanation).

Dent's book is a welcome contribution to a very neglected field. Although it has its share of tables and perplexing pie charts, it is nonetheless a readable book.

LEON BERNARD
University of Notre Dame

G. R. R. TREASURE. Cardinal Richelieu and the Development of Absolutism. New York: St. Martin's Press. 1972. Pp. 316. \$11.95.

JEAN-LOUIS THIREAU. Les idées politiques de Louis XIV. Preface by ROBERT VILLERS. (Travaux et recherches de l'Université de Droit, d'Économie et de Sciences Sociales de Paris. Series Sciences historiques, 4.) Paris: Presses Universitaires de France. 1973. Pp. 126.

Similar to G. R. R. Treasure's earlier work, Seventeenth Century France (1966), the book under review is a competent, clearly written account of the principal movements and events of Richelieu's ministry. It is not a biography. As a text it has the advantage over C. V. Wedgwood's Richelieu and the French Monarchy (1954) in that it summarizes much of the recent research in economic and social history, particularly on the origins of popular revolts. Unlike Wedgwood, however, Treasure lacks clarity and synthetic coherence: Greater detail is Treasure's principal strength, but in exchange the personalities and sense of drama associated with the figure of Richelieu fail to come alive and become as vivid as in Wedgwood.

The work by Thireau, while not in appearance a textbook, has that function to commend it. Recognizing that the primary advances in the scholarship on Louis XIV's Memoirs have been made recently by the American scholar, Professor Paul Sonnino, Thireau's purpose is to summarize Sonnino's work and give a textual explication of the Memoirs. A gloss of a difficult philosophical text is always useful when it is prepared by someone with mastery of the field in which the work is written, but Louis's Memoirs are neither difficult nor very philosophical, and the author has a minimal control over the literature of political thought regarding the French monarchy. Where Thireau adds

a perspective from a legal or philosophical tradition regarding Louis's words, he is almost always correct but very superficial. Except for beginners, if only for reasons of the sonorous and elegant style, the *Memoirs* themselves should still be preferred reading over this commentary.

OREST RANUM
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PAUL W. BAMFORD. Fighting Ships and Prisons: The Mediterranean Galleys of France in the Age of Louis XIV. Minneapolis: University of Minnesota Press. 1973. Pp. x, 380. \$16.50.

Louis XIV spent lavishly on his galleys-tens of millions, according to Bamford. Fifty per cent of the navy's budget went to the galley fleet in 1663 alone, two years before the Galley Corps was officially created. By 1600 the royal fleet numbered over fifty galleys employing 15,000 men. The officer class, made up in large part of French-born knights of Malta, received the highest pay in the navy. Under the supervision of the galley intendants a magnificent arsenal was constructed in Marseilles whose merchants grew rich from building and maintaining the fleet. One would surmise that with all its prestige and munificent royal support, the Galley Corps must have played an important military role. Yet, Bamford shows that it did not. The galleys were too light to carry heavy ordnance and were highly vulnerable to cannon fire; they had a limited range and were difficult to row in stormy seas. Bamford pointedly reveals that the fighting ships in fact did not engage in a single combat between 1660 and 1690.

Why, then, did Louis spend so profusely to amass a fleet of veritably obsolete vessels? Bamford's answer lies in part in the utility of the galleys as prisons, a subject he explores in depth. Deserters from the army, debtors, criminals of every sort, beggars, and Protestants were among those sentenced to the oar. Infidels mostly from Muslim countries were enslaved there, about 2,000 in the 1670s. Although the author does not minimize the rigor of the galley prisons and the economic exploitation of oarsmen labor in the prison workhouses on shore, he stresses the comparative advantages of galley prisons. Convict craftsmen could practice their trades in the arsenal; some oarsmen were employed in the guilds of Marseilles; and many learned vocational skills in the bagne built at royal expense for invalid oarsmen.

Still, the utility of the galleys as prisons only partially explains their value to the monarchy. The central, compelling thesis of Bamford's

book is that the Galley Corps was a useful tool of propaganda and public relations. It symbolized the king's role as a Christian crusader against the infidel and helped to inspire respect for his power and grandeur. Louis used his colorful fleet ostentatiously in the war against infidels in order to mask his policy of cooperation with the Ottoman state, to impress Gallican churchmen, and to soften public suspicion of his aims during his struggle with the papacy. It was through a "shrewdly self-interested policy," therefore, that Louis mounted a fleet of militarily useless vessels where Turks and Protestants were enchained and crews were commanded by knights who had to swear allegiance to the French monarch. Like Versailles, the galleys were a mark of Louis XIV's power. Bamford's thesis is convincing and brilliantly articulated.

One of the great strengths of this volume is the author's ability to relate galley history to the more familiar aspects of French history. The intendancy system, the Crown's efforts to discipline the nobility, the clash of centralization with local authority, venality, the revocation of the Edict of Nantes—all are impressively woven into the fabric of Bamford's book. The volume is enhanced by the skillful drawings of galleys by John W. Ekstrom and by a helpful glossary.

ROBERT M. ISHERWOOD Vanderbilt University

J. Q. C. MACKRELL. The Attack on 'Feudalism' in Eighteenth-Century France. (Studies in Social History.) Toronto: University of Toronto Press. 1973. Pp. xiii, 215. \$12.50.

Feudalism, according to the late Alfred Cobban, was "a term invented to describe the social organization that prevailed in the Middle Ages." But by the time of the French Revolution of 1789, he argued, feudalism "as a government based on the ownership of land . . ." had long ceased to exist in France. In Cobban's view the notion that the French Revolution destroyed feudalism was, therefore, a myth perpetrated mostly by historians.

Now J. Q. C. Mackrell, who studied under Cobban, has described for us with convincing detail the eighteenth-century attacks on feudalism. Was the attack only shadowboxing, something akin to the formal exercises of a karate expert who, grunting and bellowing, strenuously kicks, punches, and chops at a nonexistent enemy? Not at all, Mackrell demonstrates. While feudalism, as the medievalists define it,

certainly did not exist in eighteenth-century France, remnants of the feudal regime were everywhere. Seigniorial courts still dispensed justice, banalités still aroused the peasants' deepest resentments, the military virtues of the old feudal warrior still sustained a value system that left the commercial bourgeoisie ashamed of his day-to-day transactions and determined to escape, if possible, into the ranks of nobility. There were, still, droits honorifiques, feudal and seigniorial dues, corvées, and, yes, even serfdom, in France. Statistically, some of the remains of feudalism did not amount to much in France. But the critics of feudalism did not develop their concern for human justice from statistical tables.

• Many of these abuses, Mackrell freely admits, were, strictly speaking, not even feudal if one accepts a purist definition of feudalism. Nevertheless, Mackrell provides persuasive evidence that the historians, the jurists, the humanitarians, and the utilitarians who attacked feudalism in eighteenth-century France knew exactly what they were attacking and they knew that what they attacked existed, for the most part, either as a legal right, an ancient social custom, or as an attitude of mind. There were exceptions, of course. Mackrell recognizes, for example, that the so-called droit de seigneur was a legend that critics of feudalism used against the nobility.

By his careful reading of the writings of publicists who did not enjoy the status of a Voltaire or a Montesquieu, Mackrell confirms what Peter Gay has argued: eighteenth-century French thought was not unhistorical, abstract, and childishly idealistic, but, on the contrary, quite sophisticated when dealing with historical origins, realistic when suggesting reform, and above all, shrewdly polemical.

But what was their influence in actually bringing about reform and change? Extremely limited, Mackrell concludes. The government, whose members sometimes read the critics, suffered a failure of nerve when it tried to destroy feudalism. And when the peasants finally took matters into their own hands and attacked feudalism, they did not do so primarily because they had read the written attacks in books. Mackrell's conclusions, regarding the influence of the publicists' attack on feudalism is, therefore, deflating. For even if we accept his thesis that the intellectual attack on feudalism was often brilliant and to the point, we also come away from this book with the very live hypoth-

esis that their efforts had very little effect on the course of French Revolutionary history.

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MARK POSTER. The Utopian Thought of Restif de la Bretonne. New York: New York University Press. 1971. Pp. xii, 154. \$8.95.

Neglected ideas of the past too often become the subjects of mediocre doctoral dissertations, thus exchanging one form of oblivion for another. In a close-knit psychological, sociological, and historical analysis, Mark Poster has saved Restif de la Bretonne's utopian concepts from such a fate. In so doing the author has added an important new dimension to the historical image of a fascinating eighteenth-century Frenchman known heretofore principally as a popular novelist. What is more, Poster has given us instructive insights into the origins and relationships of some significant Western ideas about man and society.

Poster discovers the seeds of Restif's utopianism in the struggle that raged within him between destructive passions and the rational desire to control them. The central function of the ideal societies about which he wrote was always the essentially moral one of harnessing selfish individual passions for positive and constructive common purposes. However, his many specific proposals for curing society's ills lack unity and consistency. In short, the figure of Restif the social philosopher emerges from the pages of this carefully researched monograph as a credible and interesting but minor link in the utopian chain stretching from Thomas More to Charles Fourier.

Less convincing is the author's effort to show Restif's relationship to the prevailing intellectual currents of his century. Citing disagreements on a variety of issues with such figures as Diderot, Morelly, Helvétius, and Voltaire, Poster concludes that Restif's "specific utopian values tended to reflect his reaction against the Enlightenment and to anticipate Romantic strains of thought" (pp. 138-39). Perhaps such a judgment follows inevitably from Poster's surprisingly narrow, mechanistic-rationalistic notion of the Enlightenment, which relegates the masters of sensibilité to the historical twilight zone of "pre-romanticism." Yet even within the constricted framework of that approach he might well have accorded greater emphasis to at least one thing Restif, and his mentor Rousseau, had in common with all the great eighteenth-century philosophes: the pursuit of a new world view that could point the way to a "new morality" for mankind.

Scholarly readers will be grateful to find footnotes at the bottom of each page, underscoring development and changes in Restif's ideas as they appear at different times and in different publications. Not so welcome are the topical headings and subheadings within each chapter, which serve only to chop up a text otherwise free from stylistic flaws and annoying eccentricities.

J. ROBERT VIGNERY
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SHIRLEY M. GRUNER. Economic Materialism and Social Moralism: A Study of the History of Ideas in France from the Latter Part of the 18th Century to the Middle of the 19th Century. (Studies in the Social Sciences, 11.) The Hague: Mouton. 1973. Pp. 213. 28 gls.

In examining how the "natural man" of the Enlightenment was transformed by internal liberal development and moral-socialist criticism into "universal . . . proletarian . . . natural man," Dr. Gruner adds to our knowledge of postrevolutionary liberals, Saint-Simon and the Saint-Simonians, Fourier and Considérant, early socialists like Buchez, and even Karl Marx. However, there seem to be more generalizations, interpretive points, and suggestive ideas of the author's than there are concrete, balanced accounts of the individual thinkers and schools of thought she is synthesizing. Particularly irritating is Gruner's habit of discussing, labeling, and defining major ideas and terms in different parts of her book. And even though she is dealing with "connections," not the "connected" schools and theories, that is no reason for virtually equating the Enlightenment with what she calls "the Condillac man," as if the philosophes agreed with her label of "economic materialism" whereby "natural mar." would automatically labor for the prosperity and happiness of all if freed from state and church control. The chapter on Marx is also too brief to show his connection with all French liberal and socialist theories since Condillac or to justify divorcing him from his German philosophical context. Throughout the writing is involved, poorly edited, and often incomprehensible.

The book's strongest sections are in the middle. The reader begins to grasp the criginality of the author's approach as "liberals,' "economists," and "idéologues" add historical, economic, and scientific support to Enlightenment ideas of natural progress by an economically free society. Several superior chapters on social critics of this laissez-faire liberalism clarify the author's main thesis of the interplay of liberal "economic materialism" and socialist "moral socialism." Moral critics, bent on eradicating the egoism and disparity of wealth not eliminated by the automatic "laws" of liberalism, sought interventionist, though peaceful ways to a harmonious society: the abolition of property; distinguishing of a two-stage "bourgeois" class evolution and eliminating the first, "idler" stage; detaching the "exploited workers" (proletariat) from the "exploiting workers" (capitalists) and then reconciling the former to cooperative ways; infusing society with religious-moral principles; and so forth. Gruner concludes that Marx pulled together "economic materialism" and "social moralism" in a unique synthesis, though by sacrificing the "universal" nature of man and by oversimplifying the "bourgeois" class.

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STEVEN T. ROSS. Quest for Victory: French Military Strategy 1792-1799. South Brunswick, N.J.: A. S. Barnes and Company. 1973. Pp. 320. \$12.00.

Professor Ross in this short volume has provided a useful synthesis of the military and diplomatic history of the revolutionary decade of the 1790s, based, for the military aspect, on extensive work in the historical archives of the ministry of war in Paris. Twelve brief chapters take the chronological story from the diplomatic preliminaries leading up to the war that began so disastrously for France through the intervening years to the final victories that ended the campaign of 1799. The author reviews the Old Regime diplomatic setting in the early nineties, to which was added the ferment of political strife in Paris. The seesaw course of events of 1792-93, which led to the creation of the First Coalition, and the overwhelming threat that was posed to the First Republic by foreign and civil war are neatly outlined. Then came the "turn of the tide" as the Committee of Public Safety successfully solved the pressing problems posed in 1793, making possible the victories, both military and diplomatic, in the following years and the "muted victory" of 1797. For Bonaparte's brilliant campaign of 1796 had ended what was left of the First Coalition but had not brought peace. The quest for final victory then produced the aborted planning for a cross-channel

attack on England, Bonaparte's grandiose Egyptian expedition, and French involvement in the Irish rebellion of 1798. Failure in all of these led to the formation of the Second Coalition, the threatened invasion of France in 1799, and the end of the crisis by the end of the year with French victories. But the quest for victory had not been successful, and it would be left to General Bonaparte to continue the quest.

The author, however, wisely avoids viewing the story as merely prologue to the Napoleonic years, as a disastrous failure until the great leader took charge. In his conclusion he intelligently balances the Republic's failure to solve the problem of civil-military relations and its place in French public opinion with its having defeated the Second Coalition and made France the most powerful nation in Europe. Yet I wonder if too much has not been left out of this narrative. The quest for victory had begun in 1792 with a missionary zeal for the ideals of the Revolution, and the massive opposition to the Revolution in Europe in 1793 was motivated not only by traditional foreign policy objectives but for ideological reasons as well. The author's concentration on the traditional diplomatic and military motives should not have obscured that fact. Yet, given its limits of size and scope, this is a useful book.

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ÉPHRAÏM HARPAZ, editor. Benjamin Constant et Goyet de la Sarthe: Correspondance, 1818–1822. (Travaux d'histoire éthico-politique, 26.) Geneva: Librairie Droz. 1973. Pp. 758.

This heretofore unpublished correspondence has been drawn primarily from collections deposited in the Bibliothèque Nationale and the Bibliothèque Cantonale et Universitaire de Lausanne. It consists of almost four hundred letters exchanged for the most part between Benjamin Constant and the liberal elector Goyet de la Sarthe relating to the years 1818–22 when Constant represented the department of the Sarthe in the Chamber of Deputies.

The letters deal with a number of important political issues during that period of the Restoration when the liberal phase came to an end with the assassination of the Duc de Berry and reaction set in. Throughout the correspondence Constant and Goyet reiterated the need to defend the Charter against the subversive efforts of nobles, curates, and Ultra ministers whose apparent intentions were to modify the consti-

tutional structure in such a way as to facilitate a return to feudalism. Goyet undertook not only to reassure Constant of solid political support but also to urge the deputy to be ever more resolute in the defense of liberal principles. "Energetic measures are needed in times of crisis," asserted Goyet in 1820 during the debates on the law of the double vote, and his exhortations seem increasingly frantic as the reactionary tide grew stronger.

This correspondence is of importance because it reveals the tactical problems confronting Benjamin Constant, liberal politician as well as ideologue. What position, for example, were liberals to take with respect to the election of the regicide Abbé Gregoire to the Chamber in 1819? Was he to be encouraged to stand fast against the Ultras' efforts to exclude him from the lower house, or was he to be entreated to resign his seat in the interests of political harmony? Of particular importance in these letters is the nature of liberal politics at the local level. Goyet was bothered by the considerable number of minor officials who were removed from office because of their political associations or opinions, and he deplored the success of the Church in frustrating efforts to improve primary education in the department. In describing the political condition of the Sarthe, Goyet emphasized the fact that liberal sentiment was strongest in the rural areas whereas conservatism was more firmly rooted in the towns, particularly Le Mans. In sum, this collection of letters, capably edited by the Israeli scholar Éphraïm Harpaz, constitutes a valuable addition to the published source materials relating to a critical period of French history.

ALEXANDER SEDGWICK University of Virginia

Histoire de l'administration. (Institut Français des Sciences Administratives, Cahier number 7.) Paris: Éditions Cujas. 1972. Pp. 134. 18 fr.

ALBERT MABILEAU, editor. Les facteurs locaux de la vie politique nationale. (Bibliothèque de l'Institut d'Études Politiques de Bordeaux, Centre d'Étude et de Recherche sur la Vie locale. Series Vie locale, 2.) [Paris:] Pedone. 1972. Pp. 411. 45 fr.

Both of these volumes record the proceedings of academic colloquia held in France in 1969 and 1971 and published in 1972. Both also, in one way or another, are manifestations of the remarkable recent revival among the French of interest in the provinces and their relation to Paris. In addition, the fact that the much more

vigorous and stimulating of the two was the product of a provincial university offers an important clue to the reason for this new preoccupation with the provinces. Not merely are they the object of new interest, they have become much more interesting in their own right and to themselves, in some ways more interesting than even Paris.

Histoire de l'administration, produced by the Institut français des sciences administratives in its series of cahiers, is not much more than a large pamphlet. Following an introductory statement by the secretary general of the institute, M. Guy Braibant, there are ten brief papers including important surveys of the history of administration as taught in the faculties of law and economics by Pierre Legendre and in the faculties of letters and humanities by A.-J. Tudesq. There are then three papers on the state of the archives in the field, a history of administrative law in nineteenth-century France, and a report on the current state of the subject in Great Britain. After a brief discussion there are finally three helpful bibliographical annexes rounding out a useful, probably indispensable, survey of the field for anyone intending to undertake serious work within it.

Les facteurs locaux dans la vie politique nationale is a volume of over four hundred pages containing articles and comments by some fifty participants in a two-day conference sponsored by the Institut d'études politiques de Bordeaux. Unfortunately a brief review does not permit dealing with the contributions individually, and to single out one or two would in this case be impossibly invidious. The subjects covered were grouped under four main headings: the influence of local structures on national politics, the local ties of national political figures, voter behavior in local and national elections, and the influence of local and national politics in history (1830-1940). The participants, although predominantly from Bordeaux and the southwest, included guests from other provinces and even Paris, not to mention two from the United States.

While such general conclusions as can be drawn from this rich array might seem anticlimatic—local issues tend to dominate local elections, but national considerations become overriding in national politics—the vitality of the debate makes the volume exciting reading. Its principal, although not exclusive, focus is on the nonurban areas of the provinces, the endless communes with several hundred inhabitants and a maire that make up traditional agricultural France. It is hardly a new discovery

that these communities behave differently when electing representatives to departmental assemblies and deputies to Paris, but the roots and significance of this behavior have seldom been so effectively explored, even if they are not entirely resolved.

The limits of the study would seem to lie in its treatment of the administrative factor in the total governmental equation. It is not that this is ignored or played down, but rather that it is handled with too much traditional deference and too little uninhibited curiosity. Where future investigation will lead is hard to say, but two possibilities come to mind. First, it might be worth considering local elections (of departmental councilors who work directly with the prefects) as part of the administrative rather than the political process. This possibility would seem to suggest, in turn, that the election of deputies committed to a wholly disciplined majority in the assembly was in effect a form of political abdication to the administrative process.

Is it possible that the real problem lies in a general failure, by French as well as foreign scholars, to grasp the full implications of administrative government in France? Americans, of course, have little instinctive feel for the phenomenon and have to make a special effort to absorb it. But how could the French fail to understand the major framework of their public lives? Perhaps because they have known nothing else, they take it as much for granted as Monsieur Jourdain did his prose. Whatever the answer, the two volumes reviewed here will make a welcome contribution to a better understanding of the subject in France and abroad.

E. W. FOX
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THEODORE ZELDIN. France 1848-1945. Volume 1, Ambition, Love and Politics. (Oxford History of Modern Europe.) New York: Oxford University Press. 1973. Pp. 823. \$19.50.

Volumes in the Oxford History of Modern Europe series are of long gestation; readers' appetites and critics' knives have plenty of time to be whetted for the feast or the kill. Each new volume must also measure up to a painfully high standard set by the early authors in the series.

Theodore Zeldin's weighty work on modern France—only the first of two volumes that promise to total some 1600 pages—lives up to expectations. It is an important book, in part because it is so unorthodox. Zeldin has wisely

chosen not to write another detailed chronological history, one that would inevitably have inspired questions about the need to retrace a path so well marked out by Brogan, Cobban, and others. His subtitle, Ambition, Love and Politics, might seem more flippant than serious, yet the reader will find that it fits the content remarkably well. Zeldin is dealing with important problems of social history that most of us tend to neglect, mainly because they are so hard to get at. The central focus is on "the attitudes, the ways of thinking, and the human relationships which underlay the behaviour of Frenchmen." Anyone who has tried to generalize about a nation's mores and values, or even about those of a limited segment of the nation, is aware of the swampy nature of the terrain. Yet Zeldin plunges into that bog with energy and imagination, asking questions that we often tend to avoid and challenging assumptions that have often hardened into dogma.

Ambition is the central theme of part 1: the hopes and beliefs of the various sectors of society. Although the longest chapters deal with the peasants and the workers, where Zeldin synthesizes a great deal of recent scholarship, the most intriguing treatment is that of the bourgeoisie, which is examined not en bloc but in a series of eight chapters focused on various key professions or segments of modern France's social and economic elite. Doctors and notaries, for example, get chapters to themselves, along with bankers, bureaucrats, industrialists, and small commerçants. Part 2, keynoted by love, looks at marriage and morals, children, and women. These relatively brief chapters, which wrestle with the problem of the family as a unique shaping institution, are among the most stimulating and original in the book. Part 3 on politics is almost in the nature of things somewhat less innovative, since historians have swarmed about that fragrant field for years. Yet even here Zeldin offers many fresh perceptions and takes pleasure in "loosening up" many established beliefs.

No brief review can adequately summarize the content and thesis of so rich and complex a book. One striking aspect is the enormous mass of evidence on which the author has drawn, ranging from obscure provincial autobiographies, accounts of medical superstition and quackery, and popular guides to career choices, down to the latest specialized articles and even to unfinished research projects currently under way. At times one doubts that the more arcane sources will bear the full weight of the interpretation that rests on them; still,

they provide what meager evidence we have on certain interesting topics. One is struck, too, by the reiterated emphasis on variety and fragmentation; almost every chapter rings this theme, leaving the frustrating impression that nobody can generalize about anything French. The book thus resembles an infinitely complex mosaic whose interlocking patterns are beyond the power of the eye to grasp as a whole. Yet Zeldin hints at a generalization of his own: the crucial role of the intellectuals, which he suggests as the key to an understanding of modern French history. This theme he promises to develop in his second volume, along with such fascinating topics as "the history of anxiety," changing fashions in personal behavior, and the survival of independent regional cultures in this highly centralized nation. If Zeldin's treatment complicates rather than simplifies the task of historians who seek to interpret France to readers and students, if it provokes occasional responses of skepticism or dissent, it is also likely to lead many of us into rethinking our subject and following some of the signposts to future research that are scattered through the text.

GORDON WRIGHT
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J. P. T. BURY. Gambetta and the Making of the Third Republic. [London:] Longman. 1973. Pp. x, 499. £8.50.

The history of the formative years of the Third French Republic has frequently been written, but, save for a very few general works in French, a detailed account of Gambetta's rather heroic role in these lackluster years has been wanting. Bury, who many years ago began his Gambettan studies with the admirable Gambetta and the National Defense (1936), has now satisfied this need well and thoroughly. His extensive study, the second in a proposed trilogy covering all of Gambetta's career, is negotiated along the lines of Gambetta's complicated and extensive political maneuvering between the years 1871 and 1877. The narrative is nicely balanced by reference to the editorial opinion of the République française, Gambetta's personal journal, which presented in printed prose what the orator could only express in "vague but warm rhetoric." And with a fine touch Bury describes Gambetta's liaison amoureuse with Léonie Léon, of particular interest for the political insight revealed by Mlle. Léon, who regularly reviewed the public activities of her "sublime master."

The result is a detailed analysis that treats Gambetta with critically restrained admiration. Although the study offers little that might be called new or revisionist in interpretation, it provides an excellent appreciation of Gambetta's opportunist republicanism, with particular attention directed to his posture as a moderate who trailed a "tail' from the lefthis Belleville legacy. If Gambetta failed to create a united republican party and if, despite his efforts, the republic came in silently by the side door, not triumphantly through an "azure portal," he nonetheless succeeded in his major task, that of securing the republic. In describing these developments, Bury constructs a good political biography, occasionally illuminated by statements about Gambetta the private person.

On the whole, this is a carefully wrought study, well-documented and frequently graced with felicitously chosen quotations. Yet it is not without a few difficulties. Eury often writes unnecessarily intricate sentences and uncommonly long paragraphs, the combination of which does not facilitate the reading of the long narrative. Moreover, he violates the chronological order of organization, in which chapters span several months, when he includes two chapters devoted to brief but years-long surveys of Gambetta's attitude toward foreign affairs and the army. Neither of these chapters is particularly informative or purposeful to the definition of the study.

This said, one concludes that Gambetta and the Making of the Third Republic is a meaningful addition to the literature on the political history of the early Third Republic.

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FERNAND GAMBIEZ. Libération de la Corse. (La libération de la France.) [Paris:] Hachette Littérature. 1973. Pp. 318. 38 fr.

General Fernand Gambiez performed a labor of love in his minutely detailed, first-hand account of the liberation of Corsica from Axis occupation in September 1943. The "épopée" of Corsica might have been recorded in a few pages if the operation had occurred, for example, as part of the vast slaughter on the eastern front. But the miniature scale of the Corsican campaign could be deceptive, for it signified the first recovery of French metropolitan territory; it was a sort of laboratory testing of guerrilla warfare, which was soon to be of great use to the resistance movement on the French mainland; it provided a case study

of Captain Liddell Hart's "strategy of the indirect approach" to which General Gambiez pays tribute in his introduction.

Corsica, l'Ile de Beauté, is placed in its strategic and historical setting by General Gambiez, who then proceeds to the Armistice of June 24, 1940. It brought into the department a small control commission of Italian officers who, for twenty-eight months, exercised a lenient surveillance over the Vichy French civil and police authorities as well as the small armistice army. Ineffectual attempts at fascist propaganda were undertaken with the Corsicans, who were reminded of their historic Pisan and Genoese connections and their dialect so closely related to Italian. The Vichy authorities were concerned over "Italian irredentism," but the disembarkation on November 11, 1942, of an Italian army corps (eventually to comprise eighty thousand troops under General Magli's command), occurring several days after the Allied landings in North Africa, intensified the islanders pro-French sentiments. The stationing of a German brigade of seven thousand only increased the determination of the Corsican resistance network, which had been organized in April 1941 by Fred Scamaroni, who was destined for a death of Homeric fortitude.

General Eisenhower's Allied Command was confronted with an embarrassing dilemma on September 8, 1943, when the Corsican resistance, directed by the Front National, abruptly seized control of Ajaccio upon learning of the armistice between the Allies and Marshal Badoglio's antifascist faction. The Ajaccio uprising occurred at an awkward moment when all American and British shipping was committed to supplying the Salerno landings. But fortunately most of General Magli's occupation forces declared allegiance to Badoglio's government, the remainder opting for neutrality. Yet the German danger increased, for the Ninetieth Panzergrenadier Division was being ferried over the Strait of Bonifacio from Sardinia under Kesselring's orders to proceed up the east coast of Corsica to Bastia for transference to the Italian mainland at Livorno. General Henri Giraud, copresident with de Gaulle of the Committee of National Liberation, could not allow the Corsican resistance to be crushed. Without advance notification of de Gaulle (who was to be made irate), Giraud impulsively despatched to Ajaccio from Algiers a "shock battalion" (commanded by Gambiez, then a major), some Moroccan riflemen, mechanized Spahis, and support units. This small army, assigned to General Henry Martin, was transported to Ajaccio by six French warships and two submarines. The Corsican resistance fighters, General Martin's forces, as well as pro-Allied Italian units made the powerful German Panzer Division run an endless gauntlet of guerrilla attacks on its way up to Bastia and eventual embarkation by October 4.

General Gambiez has supplied a stirring campaign history, not of vast engagements of faceless hordes, but of small-scale actions even at the squad level, and of individual heroes, many of whom were his honored friends.

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FINN GAD. The History of Greenland. Volume 2, 1700-1782. Montreal: McGill-Queen's University Press. 1973. Pp. xviii, 446. \$27.60.

This volume continues the English edition of Finn Gad's definitive history of Greenland. Unlike the first volume, which covered several millennia of time, it is restricted to three quarters of a century that saw Greenland become definitely Danish and marked by actual European settlements along its western shores.

The story is told in detail by the author who explains its beginnings in the Bergen Company and the work of Hans Egede who planted the first mission station at Haabets Øe, through the vicissitudes that accompanied the slow growth of the Greenland Colony and its mission work down to 1782. And one cannot help being struck by the fact that whether this venture was supported by the private Bergen Company or its successor, the Trading Company of Jacob Sewerin, or by the Royal Danish government, in a number of ways it was always carried on at a loss to its backers. Greenland in this period was not a paying proposition.

Two things seem to account for this. First, from the start Danish colonization of Greenland was in no small measure a missionary enterprise dedicated to the Christianization of the native Greenlanders. Thus whether it was carried on by Danish-Norwegians like Egede, his sons and successors, or by Moravian brethren it was not intended primarily as a commercial venture. Second, Danish-Norwegian traders along its coasts were never able to compete successfully with Dutch whalers who profited most from contacts with those natives who gathered blubber, whalebone, sealskins, and furs for the European market.

Gad tells this story well. But even more fascinating is his account of the slow accultura-

tion of the Eskimo population during these years, as a new native language was born from the translation of the Bible into their native tongue; new singing rituals developed; the power of their shamans declined; and their economic, social, and family life changed under the influence of the Europeans who settled in their midst. Surely the account of these changes and how they took place is the author's most original contribution and one of interest not only to colonial historians but to linguists and anthropologists as well.

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A. P. LAIDINEN. Ocherki istorii Finliandii vtoroi poloviny XVIII v. [Essays on the History of Finland in the Second Half of the 18th Century]. (Akademiia Nauk SSSR, Karel'skii Filial, Institut Iazyka Literatury i Istorii.) Leningrad: Izdatel'stvo "Nauka." 1972. Pp. 158.

This short book is a definite contribution to the literature on Finland's position in the Swedish kingdom during the second half of the eighteenth century. The Soviet author, who demonstrates a good grasp of source materials in Russian, Finnish, and Swedish, presents in reasoned fashion his argument that Stockholm's political and economic policies were discriminatory against Finland and thereby resulted in a national awakening on the part of at least a small segment of the Finnish population.

The author shows a sensitivity to a variety of factors causing Finland's economic backwardness, although he places primary emphasis on Stockholm's restrictive practices in areas such as credit and investment. By the end of the century Finland had managed to move forward in the textile, sawmill, and metallurgical branches of industry, although the Finnish population, accounting for 20 to 27 per cent of the Swedish kingdom's inhabitants, contributed only 3 to 8 per cent of the kingdom's total industrial production.

In the political arena, residents of Finland filled no more than 10 to 17 per cent of the seats in the four-estate Riksdag (Parliament), and meetings of this body were limited in location to either Stockholm or other Swedish cities. When the Riksdag was not in session, interim power passed, at least in the pre-1772 period, to a sixteen-member Council of State, which rarely included Finnish representatives. Finns were also denied adequate representation on

important commissions, for example, the economic and trade commission, whose eighty members included one or two Finns, and the so-called "secret commission," which dealt with matters of war and peace.

Political and economic disenchantment with Stockholm reached a peak among Finns in 1788 when Gustavus III attacked Russia, an initiative in foreign policy that meant Finnish soldiers once again were called upon to shoulder arms against Russia. Finnish troops in the eastern part of the country, however, refused to bear arms against their neighbor. A group of 113 officers signed a platform calling for an end to the hostilities started by Gustavus III in violation of the Swedish constitution, a convocation of the Riksdag, and peace with Russia. Some of these officers went even further and sought Finland's separation from Sweden as an independent nation under Russian protection.

While accepting the argument that the movement for independence had a rather narrow social base, the author rightly rejects the notion that its leaders were ignorant or naive. Men like Sprengtporten and Jägerhorn were Finnish patriots in pursuit of a goal that evoked discussion of problems well known to students of twentieth-century Finnish history, for example, the proximity of the Finnish border to St. Petersburg, the removal of foreign troops from Finnish soil as a precondition for peace, and the necessity of good relations between Finland and Russia. The author's treatment of his subject is relevant, refreshing, and of real significance to the scholarly community.

JOHN H. HODGSON
Syracuse University

KEIJO ELIO. Otto Kaarle von Fieand!—Suomalainen upseerikouluttaja [Otto Kaarle von Fieandt—Finnish Officer Trainer]. [Historiallisia Tutkimuksia, 91.) Helsinki: Suomen Historiallinen Seura. 1973. Pp. 194.

Three names are intimately associated with the pioneering Haapaniemi military school, in operation 1780-88, 1791-1808, and 1812-19. Its founder was the well-known separatist Yrjö M. Sprengtporten (1740-1819). Samuel Möller (1743-1815) served as director from 1733 to 1788 and from 1791 to 1799. Otto Kaarle von Fieandt (1758-1825), the subject of Kzijo Elio's dissertation, was in charge from 1812 to 1981 Elio's account of the school under many sided. He discusses diffing teachers and cadets (four 1

teen cadets in 1813, sixty cadets and seventeen teachers in 1817), their social origins (of ninety-three cadets, sixty came from military backgrounds, seventeen from civil officialdom, eleven from families of ministers), curriculum (which gradually expanded from topography and reconnaissance to that of a customary military institute), budgetary problems (perennial), physical plant (inadequate), and most alarming from Fieandt's viewpoint, the inexorable, step-by-step assimilation of the Finnish institution into the Russian system. The school's vulnerability was aggravated by its remote location, mounting criticism of Fieandt's administration, the inability of Finnish leaders in St. Petersburg to influence decisions, and a tragic fire, which destroyed the main building in 1818. The doom of the Haapaniemi institution was sealed. Fieandt's resignation was accepted in May, 1819, and shortly thereafter, as Elio writes, "The Topographical School's operations at Haapaniemi ceased when its possessions were packed into eighteen boxes and the shipment left by way of Lappeenranta for Hamina [where a Cadet School had been established]" (p. 156).

The story of the rise and fall of the Haapaniemi School will probably occupy only a footnote in the long sweep of Finnish history, but it is, as Elio tells it, an interesting one, well worth the reading. There is an adequate English summary.

JOHN I. KOLEHMAINEN Heidelberg College

MICHAEL NEUMÜLLER. Liberalismus und Revolution: Das Problem der Revolution in der deutschen liberalen Geschichtsschreibung des 19. Jahrhunderts. (Geschichte und Gesellschaft: Bochumer historische Studien.) Düsseldorf: Pädagogischer Verlag Schwann. 1973. Pp. 312. DM 24.

A more precise title for this work would have been "The Image and Theory of Revolution among Liberal German Historians, 1830-70." The organization loosely follows three principles: criticism of all previous writers on German historiography; reproduction of the opinions of a number of historians about the great English, American, and French Revolutions and others through 1848; and reduction of these opinions to a set of statements for clearer analysis ("theory").

Neumüller's findings will not much surprise those who know the writings of his subjects (principally Gervinus, Rotteck, Droysen, Sybel,

Haüsser, Zimmerman). Most accepted revolution as a historical dynamic but hoped total revolution would prove unnecessary to achieve their own political aims for Germany. Their vision of revolution was thus shaped in part by their political and cultural viewpoint. The ability to see the Protestant Reformation as an early Germanic "revolution" (obviating the necessity of further violent change as in "Latin" countries) was a hallmark of much liberal historical writing. The "excesses" of the Jacobin phases of many revolutions worried these historians, who desired only the bourgeois, moderate, initial changes of the typical revolutionary cycle, not radical social change. Neumüller argues that the liberal historians' ideas were full of contradictions and their hope for a "reform-revolution," possibly from "above" (shades of Bismarck), were doomed.

The author thinks a good deal in categories and antinomies, which lend this book both a useful rigor and misleading stylization. Precisely because he attempts to reduce the often muddled and contradictory descriptions found in narrative histories to clear and unequivocal "theory," he overlooks much of the subtlety and forces the authors into narrow Procrustean beds. The description of Gervinus's admiration for the American Revolution (p. 72), for example, is overstated. Furthermore, one author is often made to speak for an entire cohort, which can be very misleading. Even expecting just one author to speak unequivocally for himself-in the sense that these men often wrote something very different a few years later-would trouble all but the most determined reducers to "theory," such as Neumüller.

The bibliography betrays a rather narrow dependency on German works and translations and betrays little knowledge of the latest international literature on the concept of revolution itself. Finally, despite the series title, there is little attempt to relate the historians to their social context.

Aside from these weaknesses, this is a narrow but often enlightening monograph on a subject that few scholars have analyzed before.

CHARLES E. MCCLELLAND University of New Mexico

ADAM MANDRUSZKA and PETER URBANITSCH, editors. Die Habsburgermonarchie 1848–1918. Volume 1, Die wirtschaftliche Entwicklung, edited by Alois Brusatti. (Vienna: Verlag der Österreichischen Akademie der Wissenschaften.) 1973. Pp. xxii, 666. Sch. 690.

The plan to publish a history of the Habsburg monarchy from 1848 to 1918 in several volumes to be written by scholars from East and West has finally reached the stage of realization. The originator of the project, Professor Hugo Hantsch of the University of Vienna, had worked for this objective indefatigably for two decades, major difficulties notwithstanding. He passed away just about a year before the first volume of the work, properly dedicated to his memory, came out under the editorship of Professor Adam Wandruszka and Dr. Peter Urbanitsch; Urbanitsch had ably assisted the first editor, Hantsch, for several years.

The difficulties inherent in the publication of collective studies with different viewpoints and research methods of various authors might have been expected to be formidable in a work that presents Western and Eastern political philosophies. In this first volume of the set organized by Professor Alois Brusatti and introduced by Wandruszka, they are, however, hardly noticeable.

In the first chapter by Nachum Th. Gross on the position of the Habsburg monarchy in world economy, similarities to France and Russia in regard to the evolution of a slow and lasting process of industrialization are underlined, whereas the rapid industrialization process of Germany is viewed as an exceptional

The backbone of the volume is two essays: one by Herbert Matis on the principles of Austrian Cisleithanian economic policies and a more extensive one by the same author jointly with Karl Bachinger on Austria's industrial development. Matis conceives neoabsolutism as continuation and partial fulfillment of the centralization plans of Joseph II, supplemented and further developed by the objectives of Schwarzenberg and Bruck to create a Central European economic as well as political union. Yet such plans pertain, at least in practice, only to the first years of this era until Schwarzenberg's death in 1852. One might question also the strong influence of private economic initiative during that period and conversely the almost complete dominance of laissez-faire liberalism from 1859 to 1879, as Matis sees it. Furthermore, a more analytical approach to the economic aspects of the Compromise of 1867 for Cisleithanian Austria would have been welcome. A further interesting point would have been the question, what kind of experience justified Prime Minister von Koerber (1900-04) to assume that joint economic interests of the Austrian peoples might have blunted the nationality conflict? No correlation has ever been established to tie national conflict to either economic misery or affluence. Yet all things considered, Matis's essays, and in particular that written jointly with Bachinger, contain a wealth of clearly presented, valuable economic data.

Of the briefer chapters the majority are mainly descriptive, which, depending on the subject matter, more often than not does not diminish the value of the studies. This applies in particular to the chapters by Josef Wysocki on Austrian financial policy; by Richard L. Rudolph on quantitative aspects of industrialization in Cisleithanian Austria; on traffic by Karl Bachinger; domestic trade and its organization with a truly original appendix on tourist traffic by Ferdinand Tremel; and the economic development of Bosnia-Herzegovina by Kurt Wessely.

A delightful analysis in the field of social history is offered in Josef Mentschl's study of Austrian management. Critical in regard to the lack of systematic economic planning is the interesting essay by Eduard März and Karl Socher on currency and banking in Cisleithanian Austria. Karl Dinklage's essay on agricultural development in Cisleithanian Austria would have been stronger if it had emphasized the needs of the Cisleithanian peasants for more and better land. If these demands had been met even to a limited degree an important disintegrating factor in the dissolution process of the monarchy might have been eliminated.

Iván T. Berend and György Ránki in their joint essay on Hungary's economic development from 1849 to 1918 give proper attention to this factor for the eastern part of the Empire. Most interesting is the essay by Akos Paulinyi on the so-called joint economic policy in Austria-Hungary. He emphasizes that the unexpectedly quick rise of Hungarian industrialization in the last pre-World War I decades presented the orthodox adherents of the view that Austrian agricultural and Hungarian industrial needs supplemented each other ideally, with a problem they were unable to handle and perhaps even to understand.

The last essay by Alois Brusatti on the development of the economic disciplines and economic history in the period under discussion provides the reader with essential and interesting information.

One might have wished for a couple of pages listing the principal data and achievements of the contributors, but altogether this volume represents a major scholarly achievement on which Messrs. Wandruszka, Urbanitsch, and Brusatti are to be congratulated.

ROBERT A. KANN
Rutgers University

WERNER KAEGI. Jacob Burchhardt: Eine Biographie. Volume 5, Das neuere Europa und das Erlebnis der Gegenwart. Basel: Schwabe & Co. 1973. Pp. xx, 642. 65 fr. S.

The next-to-last volume of this monumental biography consists of two major sections: Burckhardt's academic career as unser großer Lehrer (as his colleague Nietzsche called him) during the three decades after his final return to his city and university of Basel in 1858, and his keen observation of, and reaction to, the realities and potentialities of the European scene from the Italian war of 1859, through the fateful quinquennium of 1866-71, to the climax and the decline of the Bismarckian era. Being the first to make full use of the vast material of Burckhardt's notes for his lectures, Professor Kaegi presents, in most painstaking detail and with literal quotation of significant passages, his selection of primary and secondary sources as well as his own lively critical and occasionally sarcastic comments. Thus we are enabled to observe, like witnesses, one of the great thinkers of the last century in his study turning the results of research and reflection into the final form of his lectures. In an age of increasing specialization this uomo universale covers the wide range of European discordia concors from the later Middle Ages to his own Revolutionszeitalter of the French Revolution and its aftermath in the present and the anticipated crises of the nineteenth century.

Since after the three great works of his earlier career Burckhardt considered his lectures both to his students and his fellow citizens the form of publication best suited to his genius, they represent in fact potential books, a counterpart to his Weltgeschichtliche Betrachtungen and Griechische Kulturgeschichte, posthumously published in that form. A master of the spoken as well as of the written word, he left a striking impact on his audience, which included everybody of any cultural and social standing in Basel. Because of his unique performance as a speaker, it was natural that some of his students wrote down not only what he said but how he said it. It is a particular merit of the author that, with the help of the decipherment of these lecture notes by one of his students, Dr. E. Ziegler, he has reconstructed the spoken form of representative passages of Burckhardt's most

interesting course on the age of the French Revolution. They show the colorful liveliness of his style, as we know it from his correspondence, and his characteristic blend of matterof-fact objectivity with the moralizing irony of his Alemannic temper. While in volume 7 of the standard edition of Burckhardt's Werke of 1929 E. Dürr published, significantly together with the Weltgeschichtliche Betrachtungen, a selection of Burckhardt's notes under the title of Historische Fragmente, now the entire immense material has been made available in this biography, fortunately not only for analysis, but for extensive quotation of heretofore unknown utterances. Kaegi's Burckhardt is not only the archetype of a great university professor, but the proud burgher of his city-state in the center of Europe, who, with the insight and occasional prejudice of a conservative critic, surveys personalities, forces, and problems of his materially so successful century in the siegesdeutsch angestrichene Reich, the Third Republic, and the pseudo-imperial Regno, including also, among other issues, the Kulturkampf, the ambiguous position of the Jews, the lonely crowd of industrialized masses, and the uncanny expectation of a "terrible simplificateur" as the savior of an uprooted society. Thus, this volume, like its predecessors, produces more than the biography of an individual no matter how representative; the constant references to to the interrelations between the sage of Basel and the contemporary world turn it into a cultural history of the later nineteenth century in the heyday of an apparently unshaken bourgeois civilization. The illustrations include some significant sheets from Burckhardt's lecture

FELIX WASSERMANN
Marquette University

NICHOLAS MARTIN HOPE. The Alternative to German Unification: The Anti-Prussian Party, Frankfurt, Nassau, and the Two Hessen, 1859–1867. (Veröffentlichungen des Institutes für europäische Geschichte Mainz, number 65.) Wiesbaden: Franz Steiner Verlag. 1973. Pp. 341. Cloth DM 68, paper DM 58.

The author has examined pro-Austrian and pro-Prussian political movements in Nassau, the Free City of Frankfurt, Hesse-Kassel, and Hesse-Darmstadt. In contrasting the goals of grossdeutsch and hleindeutsch political movements, he has made an incisive contribution to scholarship dealing with German unification. Problems common to these four geographical areas in Germany included the potential effects of the Franco-Prussian Commercial Treaty of 1862. Heavy industry tended to ally with Prussia. Light industry, craftsmen, and the Catholics tended to support Austria and the Zollverein. Local conflicts such as differences between Church and state in Nassau and disputes over alternative versions of the constitution in Hesse-Kassel intensified regional differences.

August Metz in Hesse-Darmstadt and Friedrich Oetker in Hesse-Kassel were leaders of kleindeutsch political organizations associated with the pro-Prussian Nationalverein and with the Prussian Progressives. The author, relying on party newspapers and on the memoirs of those like the moderate Bavarian, Gustav von Lerchenfeld, has brought these leaders to life. After reading these pages, others may share this reviewer's feeling that, even without the Prussian new era, Metz and Oetker would have made their causes known.

The final section of the book deals with the grossdeutsch Volkspartei. After 1864 this organization supplanted and gradually replaced the earlier Reformverein. Leadership included not only the self-made businessman, Leopold von Sonnemann, but also the socialist leader, Johann Baptist von Schweitzer. In joining this grossdeutsch movement, the two leaders switched from formerly kleindeutsch outlooks. Hope shows that these two leaders, alike in their grossdeutsch orientations, differed with each other over the role of the working class in German politics.

This volume would have benefited from a more painstaking job of editing. For example (p. 94), Hope notes that the Lossen brothers, operators of a Nassau iron foundry, opposed the Franco-Prussian Commercial Treaty. This opposition "was conditioned by the state of the Taunus iron industry." However, this reviewer never found, anywhere in Hope's work, an assessment of the conditions of this particular industry.

The habit of keeping quotations in the original German will disturb some readers. Translations would have rendered a well-written book still easier to comprehend.

These are minor problems in a work that capably analyzes some of the "losers" in the German unification movement from 1859 through 1867. Hope's book is indispensable for an understanding of the period and is not likely to be superseded for many years.

JOHN W. CRANSTON
Rust College

WALTER STRUVE. Elites against Democracy: Leadership Ideals in Bourgeois Political Thought in Germany, 1890–1933. Princeton: Princeton University Press. 1973. Pp. xi, 486. Cloth \$20.00, paper \$9.75.

Professor Struve examines the elite concepts of nine German academics, publicists, and political leaders of the Wilhelmine and Weimar eras. The men in question are Friedrich Naumann, Max Weber, Walther Rathenau, and Leonard Nelson on the liberal side; Oswald Spengler, Count Hermann Keyserling, Edgar J. Jung, Hans Zehrer, and Ernst Jünger in the conservative camp.

Beyond all differences between the two groups, Struve finds some important similarities: all nine men developed their elite theories in an attempt to preserve the rule of the upper classes and keep the masses from gaining access to the political life of the nation. At the same time all of them called for "open" elites that admitted qualified individuals from the lower strata of society. In this manner they wished to pay tribute to the rising demands for greater democracy without endangering the existing order. For them democracy was to mean the "absence of class restrictions on elite recruitment" (p. 339). In a concluding chapter Struve explores the connection between these views and the "Nazi concept of democracy of personnel selection rather than decision making" (p. 18).

If Nazi elitism won out over the liberal and conservative versions, one significant reason for the Nazi success is pointed out by Struve in the course of his investigations: on closer inspection most of the liberal and conservative elitists are found to have been vague as to how their specific concept of democracy was to be implemented. When they were more precise, their proposed mode of implementation in some cases all but barred lower-class individuals from rising into the elite. The Nazis, on the other hand, took their "democracy of personnel selection" very seriously in the building up of their party and its affiliated organizations. (Once they had seized power, however, the principle of personnel democracy was applied only in a limited way on the government level.)

Struve's main theme is suggestive, and the book contains many perceptive insights based on an impressive amount of reading. Unfortunately, the treatment is often discursive and hard to follow; an involved style adds to these difficulties. All in all, however, this is a valu-

able contribution to German intellectual history, as viewed in its social and political context.

ANDREAS DORPALEN
Ohio State University

SEBASTIAN HAFFNER. Failure of a Revolution: Germany 1918-19. Translated by GEORG RAPP. [La Salle, Ill.:] Library Press; distrib. by Open Court Publishing Company, La Salle, Ill. 1973. Pp. 205. \$7.95.

This essay, by the German journalist Sebastian Haffner, must be regarded as a dramatized account rather than a full-fledged history of the German Revolution of 1918–19. It lacks not only the trappings of scholarship (neither footnotes nor bibliography accompany the text), but also the depth and perspective of an authoritative work.

Within these limitations Haffner has produced an engrossing and provocative narrative. He concerns himself chiefly with two persistent myths of the November revolution: first, that there was no revolution from below at all but merely the collapse of the bankrupt imperial order; second, that the ill-fated alliance between the majority Social Democrats and the military establishment was needed to thwart a "bolshevik-led" civil war. In response, Haffner demonstrates the extent of the popular upheaval that swept Germany in November 1918 and forced a reluctant socialist leadership to accept the reins of the revolutionary government. He also argues that the pact between Friedrich, Ebert and the high command was designed to stifle this mass movement rather than simply to crush the inept and unorganized Sparticists. Indeed, the original title of the book, Die verratene Revolution, much better expresses Haffner's animus against Ebert's alleged betrayal of an aroused working class.

In these respects Haffner's approach is similar to that of revisionist historians of the past decade who have maintained that a genuine revolutionary rising did occur in Germany, even though it largely failed in its democratic as well as its socialistic objectives. Yet Haffner is too preoccupied with personalities rather than policies to lend any weight to this argument. He blames the failure of the revolution squarely on Ebert, whom he accuses of a grovelling identification with traditional authority and a paranoid fear of disorder. In stressing Ebert's shortcomings, however, Haffner neglects such vital factors as the parliamentary tradition of the Social Democrats, the

vacillation and division of the Independent Socialists, and the administrative inexperience of the socialist leadership in general.

In contrast to the treason of Ebert and his colleagues, Haffner contends that "the workers and soldiers who had carried through the Revolution knew instinctively that as long as the old bureaucracy and the old corps of officers retained their power, the Revolution was lost" (p. 107). Yet he fails to account for the acquiescence of the soldiers' and workers' councils in allowing the existing administrative apparatus to function almost entirely unchallenged or their readiness to hand power back to an elected constitutional assembly. In sum, Haffner's book does as much to obscure as to illuminate the troubled birth of democracy in Germany.

LEWIS D. WURGAFT
Massachusetts Institute of Technology

RALF-RAINER LAVIES. Nichtwählen als Kategorie des Wahlverhaltens: Empirische Untersuchung zur Wahlenthaltung in historischer, politischer und statistischer Sicht. (Beiträge zur Geschichte des Parlamentarismus und der politischen Parteien, number 48.) Düsseldorf: Droste Verlag. 1973. Pp. 194. DM 54.

HERBERT KÜHR. Parteien und Wahlen im Stadtund Landkreis Essen in der Zeit der Weimarer Republik: Unter besonderer Berücksichtigung des Verhältnisses von Sozialstruktur und politischen Wahlen. (Beiträge zur Geschichte des Parlamentarismus und der politischen Parteien, number 49.) Düsseldorf: Droste Verlag. 1973. Pp. 309. DM 64.

Although quite different in conception and theme, these two monographs have a number of traits in common. Both are somewhat revised doctoral theses published in one of the most distinguished German series devoted to that genre; they are stylistically spare, one might say clinical, in their analysis; and they display a methodological sophistication on a par with the best in either the French or Anglo-American traditions of political science.

The subject of electoral abstention is no novelty, and Dr. Lavies makes little attempt to strain for originality. But it is certainly useful to have the matter spelled out in specific German terms and to place his work on the shelf beside Alain Lancelot's L'abstentionnisme électoral en France (1968), which performs a comparable service. Inevitably there is some belaboring of the obvious. The value of official statistics, we are told, depends on the rigor of bureaucratic reporting (Meldedisziplin). The

same tabulations are sometimes examined first in the text, then in a table, finally on a graph. The mathematical exactitude is often overwhelming and occasionally absurd: as when the total response in one public opinion poll reaches only 99.8 per cent, while in another it is 100.1 per cent. Yet such objections are innocuous. Lavies carefully traces the voting patterns in Germany since 1870, showing the steady decline in abstentions under the Empire, the erratic fluctuations during the Weimar Republic, and the extraordinarily steady and high (ca. eightyseven per cent) rate of electoral participation in the federal elections of West Germany since 1949. He sorts out the various factors that affect voting habits-sex, age, marital status, social standing, religion-and, not surprisingly, he concludes that a general homogenization of the German electorate is becoming apparent (whereas, in the French case, Lancelot also finds increasing homogenization but far less stability). About six per cent of German voters never cast a ballot and may be classified as Dauernichtwähler; the others who abstain do so for local, geographical, or accidental reasons. In short, turning out the vote is scarcely a problem in the Bonn Republic and, except for soliciting the young, who tend to fall below the national participatory norm, the parties have relatively little to gain from the frantic eleventh-hour tactics that characterize American elections. All of which is related with competence and unrelenting earnestness.

The Kühr dissertation was completed under Adam Wandruszka and belongs in a methodological genealogy beneath the names of André Siegfried and Rudolf Heberle. It is an excellent addition to the many studies of local history in Germany that have appeared in the last decade. Although he lacks the literary flair of a William Sheridan Allen, Kühr's story is no less fascinating or important. Essen is, after all, located at the heart of Germany's industrial complex; and the collapse of parliamentary democracy there was not an isolated or atypical incident. For the most part, the treatment is conventional political narrative that conforms to the usual chronological pattern of the Republic's fourteen years. There are, however, some characteristic details worth noting: the clear evidence that the conservative parties in Essen were undergoing a radicalization before 1928; that the Center party managed to remain the largest and most stable political formation; that the SPD was remarkably weak in a city where the working class could be estimated at one-third of the population, leaving the KPD as the second strongest party in the city from 1921 to the end of 1932; and that the NSDAP, despite the considerable exertions of its leadership, experienced a nearly complete lack of local success until 1930.

Neither of these accounts is likely to force a major revision of prevailing textbook interpretations, but they do add to the growing corpus of reliable monographic literature in recent German history.

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KARL HOLL and ADOLF WILD, editors. Ein Demokrat Kommentiert Weimar: Die Berichte Hellmut von Gerlachs an die Carnegie-Friedensstiftung in New York, 1922–1933. With a foreword by ALFRED KANTOROWICZ. Bremen: Schünemann Universitätsverlag. 1973. Pp. 268.

Hellmut von Gerlach (1866–1935), the grandson of an ennobled bourgeois, was a member of the circle around Friedrich Naumann who tried to combine conservatism with a progressive Sozialpolitik. The disappointments the group experienced in its social approach drove him—as told in his memoirs Von rechts nach links (1937)—toward the political left. As editor of the weekly Die Welt am Montag he became one of Germany's most discussed journalists, fled upon Hitler's coming to power, and died in Paris. The present volume contains the monthly reports on the political situation of Germany that, from 1922 to 1930, he rendered to the Carnegie Endowment for International Peace.

Writing for an American public, von Gerlach quite justifiably simplified the shape of some of the events he pictured, replaced the often cumbersome names of German parties and groups by less complicated concepts, and concentrated on what he believed would interest American readers. In doing so he drew what in retrospect looks like an overoptimistic tableau of the chances of the Social Democratic party, reunited in 1922. Down to the very last moment he failed to see—the editors show—that Hitler would emerge victorious. I will never forget von Gerlach's amazed looks when fleeing—without any luggage-from Berlin to Munich, whence I and another couple smuggled him across the German frontier after the fatal Reichstags election of March 5, 1933, which supported Hitler.

The value of this volume consists in its being some sort of abbreviated manual illustrating in a concise fashion, and in brilliant presentation, the opinions and hopes of the non-Communist German leftists during the Weimar regime. During World War I these leftists had had to fight primarily the German Anglophobes, the champions of German Weltpolitik. In the time of Weimar they had to attack mainly the German enemies of France, then the mightiest power on the Continent. As an intrepid leader in this fight, von Gerlach was highly regarded in France, and especially in Paris, and it is almost symbolic that he died there.

GEORGE W. F. HALLGARTEN Washington, D.C.

KLAUS HILDEBRAND. The Foreign Policy of the Third Reich. Translated by ANTHONY FOTHERGILL. Berkeley and Los Angeles: University of California Press. 1973. Pp. ix, 209. Cloth \$10.00, paper \$3.95.

"The break represented by the year 1933 results from the continuity in Prussian-German history." With that provocative thesis Klaus Hildebrand concludes his interpretive essay on Hitler's foreign policy. Those seeking a narrative account of German foreign policy from 1933 to 1945 will not find it here. Rather, the author skillfully analyzes two fundamental questions: what was the relationship between foreign and domestic policies in the Third Reich, and to what degree did that foreign policy represent either a continuation of or a break with the past?

There existed for a time an identity between Hitler's ideas and the wishes of the old elites that had determined policy in the Reich since Bismark—the military, the diplomatic corps, and big business. The "dual state" was in effect a balance struck between these old elites and the new NSDAP elite, a balance that promised to preserve the existing social order by avoiding large-scale social reforms. The cement joining the two was Hitler's program, that set of Wilhelmine foreign-policy goals for German power, eastern expansion, and eventual global empire, which Hitler formalized in a calculated, graduated plan (Stufenplan). Hildebrand convincingly demonstrates the continuity between Hitler's objectives and traditional German foreign-policy aims, between his own diplomatic techniques and those of his

The discontinuity introduced by Hitler arose from the very continuity in the domestic imperative of his program: the demand for social stability. The advanced state of social polarization between the propertied classes and the

proletariat from 1929 to 1933 required an intensification of the mechanisms for social control. This task was facilitated by entirely new technical methods of manipulation. At the same time, the social enemy became the Soviet Union, the racial enemy the Jew. But the traditional anti-Semitic aspect of Hitler's appeal now burst beyond all past boundaries in the call for a new and biologically superior race. The new society would eventually replace the old elites, the "dual state" would be foreclosed. But in the excessive application of these mechanisms the original goal-the maintenance of the existing social order—was completely lost from view. Thus the teleological nature of the system whose goal was to draw the historical process to its end and to produce a biological culmination through the breeding of a new race represents a "revolutionary discontinuity in German history."

The continuity in German foreign policy seemed dependent upon calculation. From 1933 to 1940 Hitler shrewdly blended calculation and risk (another element of continuity in German history) until he had virtually completed the first phase of his *Stufenplan*. But war in the West rendered an alliance with Britain impossible; war in the East signaled an end to calculation. In Operation Barbarossa, in which a solution to the "Jewish question" was sought in conjunction with the conquest of Lebensraum, calculation gave way to dogma, sound military judgment to extermination.

No serious scholar can afford to overlook Hildebrand's important contribution to our understanding of Hitler's foreign policy. His perceptive analysis is based on a thorough command of the relevant literature; his case is stated clearly and persuasively. Unfortunately, the translation is uninspired and the index inadequate.

MARSHALL M. LEE University of Wisconsin, Madison

MARTIN L. VAN CREVELD. Hitler's Strategy, 1940–1941: The Balkan Clue. (International Studies.) New York: Cambridge University Press, for the Centre for International Studies, London School of Economics and Political Science. 1973. Pp. x, 248. \$13.95.

Hitler's Strategy, 1940-1941 is a careful study of German policy toward Yugoslavia and Greece. This thoroughly documented monograph makes good use of German and Italian wartime records as well as later studies and memoirs (less

so of East European materials). It provides many good insights into the complexities of a region where governments jockeyed uneasily among multiple enemies and unreliable friends. German-Italian rivalry, too, properly figures as a significant factor, as does uncertainty over British and Soviet responses to German bids for hegemony in southeastern Europe.

Van Creveld's evidence shows that the German attack on Greece was delayed because of Bulgarian, Yugoslav, and Turkish reluctance to cooperate, bad weather, and concern over the Soviet response. Perhaps the major novelty of his argument is to deny the conventional view according to which the campaign against Yugoslavia and Greece caused a postponement of the Nazi invasion of the Soviet Union from mid-May to June 22, 1941. His argument is interesting and serious, though in part circumstantial and less than watertight.

His search for an underlying calculus in German policy making leads the author to a neat periodization in Hitler's thinking—a direct attack on England, a "peripheral strategy" against it, and finally the decision to dispose of the Soviet Union in 1941, so as to be free to deal with the Anglo-Saxon world in 1942. While Hitler wanted Greece for his peripheral moves in the Eastern Mediterranean in late 1940, he failed to move against her. By the time Barbarossa was scheduled, the Italo-Greek war could not be permitted to linger on the Nazi flank. This is a sensible argument, but the resulting picture of frequent reversals, frantic vacillation, and wholesale shifts of Nazi objectives does seem exaggerated.

The author also gives the impression of unduly shifting responsibility for German moves onto the intransigence of others—e.g., Greek failure to accept Nazi mediation with Italy in December 1940, Molotov's demands on Germany in November 1940, and the Yugoslav revolt in March 1941.

Finally, Dr. Van Creveld appears to give excessive weight to the prospect of Soviet intervention in the Balkans in 1940-41 (for instance, in Yugoslavia, Bulgaria, or at the Straits). Given Stalin's overriding effort to gain time and sit out the conflict as long as possible—and especially after the miserable showing of the Red Army in the Finnish Winter War—the Soviet use of force to stop the Germans in southeastern Europe in 1940-41 was an utterly unrealistic prospect and amounted to little more than a rationalization for Hitler and the OKW.

ALEXANDER DALLIN Stanford University

WINFRIED SCHULZE. Landesdefension and Staatsbildung: Studien zum Kriegswesen des innerösterreichischen Territorialstaates (1564–1619). (Veröffentlichungen der Kommission für Neuere Geschichte Österreichs, 60.) Vienna: Herman Böhlaus Nachf. 1973. Pp. 292. DM 60.

This study, based on archives in Vienna, Graz, Lubljana, and Klagenfurt, is a meticulously detailed illustration of Max Weber's thesis that external pressure has an important determinative influence upon the evolution of a state's political structure. The pressure in this case was supplied by the Turks; the response of the Inner Austrian Lands had lasting effects upon their institutions and political thinking, some of which contradict conventional wisdom about the state-making process in the early modern period.

In 1574 and 1575 the estates of Inner Austria, motivated by the inadequate border defense provided by the Landesfürst Archduke Charles, took the initiative in proposing a new centralized military organization that would improve recruitment, supply, and command of border forces. This question was pushed urgently at the Generallandtag of Bruck in 1578 and led to the establishment of a Hofkriegsrats at Graz in the same year.

The first half of this study is devoted to these developments and to the council's activity once it was established. Mr. Schulze's more important point—elaborated in detail in the later chapters—is that the initiative of the Estates was not an expression of the customary polarization of politics in the territorial states of the period but a step toward overcoming it. They were not challenging the Landesfürst's prerogatives but providing him with a new instrument of centralized authority and with the means of supporting it, and thus were contributing to the process of building a more integrated state.

The military institutions that they favored worked towards the same end. The Landesfürst had preferred to rely on mercenary armies; the new system relied more on native musters and mobilization of local resources. For both the peasant population and the local nobility the result was a new identification with the state and—in Mr. Schulze's words—"a higher degree of administrative, economic, social and military contact that . . . created a new sphere of social communication" (p. 248). Along with this, he insists, came the first stirrings of a new political consciousness, which one finds, to take only one example, in the Estates' perception, in 1574, of their country as a "whole being, with your

princely eminence and the true lands as head and limbs" (p. 135).

GORDON A. CRAIG Stanford University

HORST BRETTNER-MESSLER, editor. Die Protokolle des österreichischen Ministerrates, 1848–1867. Part 6, Das Ministerium Belcredi. Volume 2, 8. April 1866–6. Februar 1867. With an introduction by FRIEDRICH ENGEL-JANOSI. Vienna: Österreichischer Bundesverlag für Unterricht, Wissenschaft und Kunst. 1973. Pp. lxxxviii, 478.

No short review can do full justice to the variety and richness of a documentary collection such as the present volume of protocols of the Austrian Council of Ministers. This volume, the second compiled by Brettner-Messler, completes the ministry of Richard Belcredi (1865-67) and is distinguished by the same editorial thoroughness as its predecessor (AHR, 77 [1972]: 1459-60). The same inherent limitations of the protocols also appear. The documents published are the final version presented to the Austrian emperor, after emendation and revision by the ministers individually. Some of the atmosphere in which deliberations occurred is probably lost as a result, and the documents are certainly encumbered by an added measure of official jargon and style, which makes them less than exciting reading. We know also that some ministerial councils met for which no records were kept, or for which no records have survived. The importance of such imperfections should not be overemphasized, however, simply because the Council of Ministers exercised no real authority. We again find the ministers—during a period of extreme importance for the Austrian Empire—discussing the details and ramifications of decisions and major policy changes in which the council figured only peripherally. As a governmental organ the council exerted slight influence upon the two critical decisions of the months covered in this volume: the war with Prussia and the Ausgleich with Hungary.

Most of the protocols dealing with the Austro-Prussian conflict have already been published, at least in part, by Heinrich Srbik (Quellen zur deutschen Politik Österreichs); Joseph Redlich (Das österreichische Staats- und Reichsproblem); Richard Blaas (Il Problema Veneto e l'Europa, vol. 1); and Angelo Filipuzzi (La campagna del 1866 nei documenti militari austriaci). Brettner-Messler adds the minutes of the important military conferences of March, April, and May 1866 and uses notes on the ministerial councils

and conferences taken by the adjutant general, Count Crenneville. The protocols (all of which are printed in full) also offer additional details on the compromise with Hungary. The Prussian and Hungarian problems have been extensively treated, however, and students of the domestic problems and developments of the Empire may find other subjects touched on by the councils more interesting and rewarding. (The status of Galicia after the Hungarian settlement, the expansion of the railway network, and economic developments are good examples.) In any case, the protocols are useful. Even though the ministers did not make the decisions, they did debate the nature of the problems confronting Austria as well as the range of alternatives considered by the government in dealing with those problems. For that reason alone the records of the ministerial meetings are valuable documents, and this collection of them is extraordinarily well done.

> RICHARD B. ELROD University of Missouri— Kansas City

B. N. FLORIA. Russho-pol'shie otnosheniia i baltiishii vopros v hontse XVI-nachale XVII v. [Russian-Polish Relations and the Baltic Question at the End of the 16th and the Beginning of the 17th Century]. (Akademiia Nauk SSSR, Institut Slavianovedeniia i Balkanistiki.) Moscow: Izdatel'stvo "Nauka." 1973. Pp. 220.

Floria's book is a study of Russo-Polish relations between 1587 and 1605. As the title suggests, the author believes that, in these years between the end of the Livonian War and the internal collapse of Russia in the Time of Troubles, the rulers of both countries concentrated their diplomatic efforts, above all, on improving their position in the struggle for possession of the Baltic States. On the Polish side, Sigismund III and his councilors aimed at conquest of all of the Baltic territories and, in the case of the king himself, all Swedish domains. The Muscovites' goals were considerably more modest. Boris Godunov and his diplomatic advisers wanted to acquire an opening on the Baltic for Russian diplomats and traders. Failing greater triumphs, Narva, which had served as a Russian port in the years of Ivan IV's Livonian triumphs, would suffice. Sweden stood between both powers and the realization of their Baltic ambitions. The logic of the situation, then, pointed to a common front of the two Slavic powers to drive the Swedes from their foothold in northern Estonia and release their strangle-hold on the eastern Baltic. In fact, of course, events did not follow so neat a pattern. In the last years of the sixteenth century, misunder-standings and clashing ambitions in other areas divided Russia and Poland as often as the danger from Sweden united them. And in 1605 Sigismund III gave Polish support to the first False Dmitrii, beginning a policy of armed intervention in Russia's internal affairs. Before long, Muscovy and Poland were mortal enemies, and Sweden was left alone to transform the Baltic into its private lake.

This, in rough outline, is the story that Floria tells, and he tells it well. His research is impressive; he has used published materials in an impressive range of languages and worked extensively in Soviet and Polish archives. Moreover, the author has assembled the scattered materials in a clear and cogent narrative. In most cases he explains the bewildering shifts of policy convincingly and with refreshing modesty. In seeking the roots of each nation's actions, Floria gives due weight to economic rivalry, political ambition, and religious strife. At times, as he points out, the mechanics of diplomacy themselves changed the course of negotiations; one power might miss a favorable moment for an alliance with another simply because its ambassador could not reach the court of the would-be ally quickly enough. Finally, the author admits frankly that the surviving sources do not suggest any reasonable explanation for a few of the most bewildering policy changes.

The book has its shortcomings to be sure. It is diplomatic history of a rather conventional type. At times the diplomatic struggle seems to take place in a vacuum. In all fairness, Floria links the changing policies of the Baltic powers with their domestic problems and with the general balance of forces in Eastern Europe. At times, however, I found his sketches of the political and strategic background of diplomacy too brief and perfunctory. Moreover, the narrative is a bit dry and colorless. In particular, the principal actors rarely come to life, and their individual contributions to the diplomatic struggle are not stressed enough. Even Sigismund III remains a two-dimensional figure even though Floria makes clear that it was the king who was personally responsible for undertaking the disastrous crusades to reconquer Sweden and subdue Muscovy. In spite of these limitations, however, Floria's study is an impressive and valuable treatment of an important

phase in the complex relations between Russia and Poland.

ROBERT O. CRUIAMEY
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Davis

ATHANASIOS A. ANGELOPOULOS. Hai zenai propagandai eis tēn eparchian Poluanēs kata tēn periodon 1870—1912 [Foreign Propaganda in the Province of Poluane, 1870—1912]. (Hetaireia Makedonikōn Spoudōn, Idruma Meletōn Chersonēsou tou Aimou, 137.) Thessaloniki: Institute for Balkan Studies. 1973. Pp. 175.

Focusing on Poluane, a present-day Greek province in Macedonia (an Ottoman region before 1912), Athanasios A. Angelopoulos catalogs in great detail, village by village and where possible household by household, the efforts between 1870 and 1912 of the Bulgarian Exarchate, the Serbian government, the Bulgarian Uniate Church, and the Protestant missionaries of the Levant, to "subvert the Hellenic Orthodox spirit" of the Greeks of the province. Relying almost exclusively on documents in the archive of the Greek-oriented Orthodox Metropolitanate of Salonika, Angelopoulos selects evidence in support of the traditional Greek position on the befuddled "Macedonian question" that still haunts, as the publication of this book shows, Greek, Bulgarian, and Yugoslav relations. According to this account, Greek speaking Greeks and "slavophone-hellenophrones" (Slavic speakers of Hellenic sentiment), targets of foreign propaganda, suffered coercion by "bribery, terror, anarchic acts, threats of assassination and assassination itself." Because of their "spiritual and overall superiority" they either publicly maintained their true identity "or else under force appeared to be subjected to foreign national enslavement."

About the Macedonian controversy, few things are certain—and Angelopoulos acknowledges none of them. Thanks to extensive tampering with statistics and elections by all sides, Macedonian statistics are useless. And of all the charges and countercharges of coercion by bribery and terror made by all sides, no party was fully exempt.

At best, Angelopoulos offers a guide to contemporary Greek sources that one hopes will some day be put to good use by scholars, free of the biases that too often mar studies of this and other sensitive topics in Balkan history. It is a pity that the monograph series of the Institute for Balkan Studies should allow a nationalist polemic in scholarly dress to occupy a place in its otherwise outstanding list of books.

GEORGE D. FRANGOS Vassar College

PATRICIA KENNEDY GRIMSTED. Archives and Manuscript Repositories in the USSR: Moscow and Leningrad. (Studies of the Russian Institute, Columbia University.) Princeton: Princeton University Press. 1972. Pp. xxx, 436. \$22.50.

This volume will be a boon to exchange students and others about to embark on a program of research in Soviet archives. Its core is an annotated bibliography of the most important published finding aids for each repository in Moscow and Leningrad; another volume, which will cover major provincial centers, is promised. The finding aids are general descriptions and surveys (either of a whole archive or of specialized collections within it), guides, catalogs, and inventories. They range from multivolume editions to pamphlets and brief articles in historical journals. The bibliographical entries (575 in all) are fuller than they tend to be in Soviet reference works, and the accompanying annotations indicate the relative strengths and weaknesses of each item. We are told in what respect the information they contain is outdated, inaccurate, or incomplete. It is duly stated where the entries give detailed references to fond numbers. Since many of these works are bibliographical rarities, it is particularly helpful to be given their call numbers in the Library of Congress and Harvard University Library collections.

Thanks to Dr. Grimsted the aspiring foreign scholar should henceforward be able, when submitting his application to Soviet authorities, to provide the specific information they require about the files he wishes to consult. The practical problems faced by historians from abroad when working in Soviet archives are sagely examined in the introduction to this volume (pp. 5-83). Also included here is an account of the current system of political-administrative control and a historical sketch of the way in which the country's national archive fund has been built up since the earliest times. Dr. Grimsted provides us with a veritable Ariadne's thread through the labyrinth of institutional changes and "archival migrations," which were especially frequent during the 1920s and 1930s. Her account is at times somewhat repetitious, and one would have welcomed a bolder effort to distinguish between those reorganizations that were simply nominal or administrative and

those that involved physical rearrangement. One could also have wished for a fuller analysis of the motivations behind the changes, although this is perhaps a task best handled separately. What emerges clearly is the extent to which Soviet archival organization, although more thoroughly centralized than that of any noncommunist country, has involved numerous practical compromises. As a result some of the principal holdings are based upon those of institutions established long before the Revolution. For this reason many guides published during the nineteenth century are still of value, and they have accordingly been included in this bibliography.

It is still current Soviet practice to deny researchers, especially foreigners, direct access to unpublished finding aids. It is therefore good news that over the next five years no less than forty-six guides and handbooks are scheduled to appear in print, as well as inventories of some of the most ancient documentary collections in Tsentral'nyy Gosudarstvennyy Arkhiv Drevnikh Artov (compare G. A. Belov in Arkheograficheskiy ezhegodnik za 1971 god [Moscow, 1972], p. 270). One hopes that these reference works will be published in sufficiently large imprints to meet the demand for them and that they may be followed by other bibliographical aids of a standard commensurate with that of this volume. Dr. Grimsted's study should also stimulate emulation among Western scholars. It would be worthwhile, for example, to compile a bibliographical guide to the numerous collections (or more accurately, selections) of documents published in the USSR since the Twentieth Party Congress. For all the political bias they display, they contain much useful information, not least concerning the location and nature of archival material.

JOHN KEEP
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Recent Studies in Modern Armenian History. (National Association for Armenian Studies and Research.) Cambridge, Mass.: Armenian Heritage Press. 1972. Pp. x, 141. Cloth \$6.95, paper \$4.95.

Literature dealing with modern Armenian history has been noticeably permeated by a characteristic proclivity to view regional concepts from the narrow perspective of the East-West ideological struggle. It is therefore mostly out of focus and insensitive to the patterns of social evolution peculiar to the Armenians. It is also somewhat superficial and simplistic in approach,

since material descriptive of modern Armenian history is normally marred with preconceived Western notions and ornamented with a few brief quotations. Ten individuals, mostly from the academic profession, contributed to make this publication somewhat more representative of current thinking about some aspects of modern Armenian history. The substance of the book was first presented in the form of papers by participants in the Conference on Modern Armenian History held at Harvard on October 29 and 30, 1970, and sponsored by the National Association for Armenian Studies and Research. Although many problems were left untouched, the publishers felt that a combination of general discussions with papers on special topics would provide a better understanding of modern Armenian history as a whole. Thus topics range from broad discussions of Armenian nationalism to detailed accounts of Armenian immigration to the United States during the first quarter of this century.

The questions of the emergence of the Armenian national awakening and the Armenian liberation activities in the seventeenth and eighteenth centuries are ably dealt with by Srpouhie A. Essefian and Manoog S. Young. While the former traces the pioneering efforts of Israel Ori in the West, the latter primarily concentrates on the activities of Joseph Emin in the Transcaucasus. Both contributors attribute the failures of those noble attempts at the liberation of Armenia from its Muslim rulers to a lack of genuine interest in the plight of the Armenians by the Western Powers and Russia.

Richard G. Hovannisian's article, "The Armenian Occupation of Kars, 1919," is a notable attempt at providing a sound, lucid, and detailed historical sequence from 1877 to 1919 for the occupation of this strategically important district by Turkey, Russia, and Armenia.

Three articles are valuable as cogent summaries of their subjects. John Richardson's article, "The American Military Mission to Armenia," gives a solid account of the investigations of the Harbord Mission and its official report to President Woodrow Wilson. The article by Joseph L. Grabill, "Protestant Diplomacy and an American Mandate for Armenia, 1914–1920," focuses, though somewhat overenthusiastically, on the American Protestant activities to create a more sympathetic American attitude toward Armenians and to convince Congress to assume a mandate for Armenia. Thomas A. Bryson's article, "Walter George Smith and the International Philarmenian League," describes the sincere efforts of a former president of the

American Bar Association to present the Armenian question to the League of Nations.

Vahakn N. Dadrian's article, "The Methodological Components of the Study of Genocide as a Sociological Problem: The Armenian Case," has the important value of a pioneering effort. The author not only discusses genocide in specific sociological terms, but he also draws an interesting parallel between the two major cases of holocaust in this century, namely, the Armenian during World War I and the Jewish during World War II.

The last three articles deal primarily with the Armenian community in the United States. Edward Minasian's article, "The Armenian Immigrant Tide: From the Great War to the Great Depression," not only provides valuable immigration statistical data, but it also cites some of the hardships that most Armenian immigrant aliens have encountered in the United States. The article by Robert Mirak, "Outside the Homeland: Writing the History of the Armenian Diaspora," adopts a refreshing and convincing approach to this subject matter. The author compares the economic, social, and political factors that forced the Armenian exodus from the Ottoman Empire to the United States with those that caused the emigration of Europeans to the New World. Jack Danielian's article, "Armenian Cultural Identity: Problems of Western Definition," discusses some aspects of the true nature of prejudice against the Armenians in the United States and reasonably concludes that the identity of a given minority can be achieved only through linking one's cultural past to the present.

All in all, this volume should be welcomed by the community of scholars. It is the product of a well-planned project. All who have collaborated in contributing to its publication will be rewarded in years to come by its great utility as a significant impetus to research on modern Armenian history by area specialists and social scientists alike.

VARTAN H. ARTINIAN
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Amherst

J. G. GARRARD, editor. The Eighteenth Century in Russia. New York: Oxford University Press. 1973. Pp. xiii, 356. \$21.00.

This collection of thirteen original essays by fourteen British- and American-educated specialists examines disparate facets of Russian cultural and intellectual evolution in the eighteenth century. If the authors follow no stand-

ard model, neither do their efforts encompass equal portions of space, chronology, or significance; nor, indeed, do they reflect a shared interpretation. The essays fall into four broad topical-chronological subdivisions: the history of ideas, Petrine Russia, Catherinian Russia, and the arts. Such thematic diversity is partly coordinated by a common concern with questions of borrowing, imitation, and autochthonous development.

Just as the different essays vary widely in focus-from sweeping surveys through period pieces to studies of special subjects—so they also differ considerably in quality and intrinsic interest. Marc Raeff's treatment of the Enlightenment, for instance, plumbs that immense phenomenon to offer both provocative generalizations and arresting asides. By contrast, Tamara Talbot Rice contributes an uninspired recitation of artistic trends that barely exploits the possibilities of her subject. Along with the talents of authorities like Raeff and Arthur Wilson, who meticulously reconstructs Diderot's visit to St. Petersburg in 1773-74, the collection exhibits work of younger specialists like James Cracraft's stimulating sketch of Feofan Prokopovich, Max Okenfuss's two brilliantly revisionist discussions of Petrine educational institutions, G. Gareth Jones's astute analysis of Novikov's early journals, Anthony Cross's impressive panorama of "the British in Catherine's Russia," and In-Ho Ryu's controversial exposé of "Moscow Freemasons and the Rosicrucian Order." Though interesting and original in conception, Robert Jones's survey of Catherine II's urban planning policies neglects some of the European sources of her ideas (for example, cameralism) and bears slight relation to the volume's cultural-intellectual focus. Compared to these fresh investigations, the remaining essays-Harold Segel on classicism in Russian literature, Alfred and Jane Swan on music, and Joel Spiegelman on keyboard music-add breadth but little depth to the work as a whole.

Nearly all the contributors concentrate upon "high" culture and the social elite that produced and consumed it; they scarcely consider popular and provincial culture. Nor is much attempt made to link cultural evolution to economic, social, or political developments. Essays on the impact of modern science, the theater, and other modes of literature would have bolstered the book's appeal and strengthened the editor's claim to have addressed "most, if not all, of the significant problems" of the period. While the contributors maintain a high level of factual accuracy, the volume repeats

some dubious generalizations, such as the alleged negative impact of the Pugachev Revolt on literature and thought, the citations could have been fuller in some instances (Professor Ryu is apparently reacting against, but fails to mention, the views of the late George Vernadsky and Gilbert McArthur), and there is a depressingly large number of typographical errors. Still, with forty-one monochrome plates enhancing the text, this book signifies a praiseworthy maturation of Anglo-American scholarship on heretofore neglected subjects.

JOHN T. ALEXANDER University of Kansas

MARC VUILLEUMIER et al., editors. Autour d'Alexandre Herzen: Documents inédits. (Révolutionnaires et exilés du XIX° siècle. Études et documents publiés par la Section d'Histoire de la Faculté des Lettres de l'Université de Genève, 8.) Geneva: Librairie Droz. 1973. Pp. 345.

Although this collection of documents is not without interest and is generally well edited, it gives the reader very little new information about Herzen the thinker and radical publicist. Any future biographer of Herzen will be grateful for it, but the student of nineteenth-century Russian radicalism will find the pickings rather slim.

The bulk of the letters in this volume were the property of Herzen's grandson Nicolas, who died in Lausanne in 1929 after a distinguished career as a professor of law at the university there. His inheritance also consisted of a part of his grandfather's library and such memorabilia as the menu from a dinner that Herzen gave for Garibaldi in April 1864. The entire collection was acquired by the Public and University Library of Geneva in 1967.

The heart of the book is the first full publication of the extant correspondence between Herzen and Carl Vogt, although all but one of Herzen's letters have appeared in the Soviet edition of his complete works. The introduction, correspondence, and notes all clarify Herzen's involvement in Swiss politics, particularly in the late 1840s and early 1850s; indeed there is much here for the student of Swiss politics. Moreover, Carl Vogt, who may now be chiefly remembered for Marx's attack on him, was a most accomplished letter writer: witty, of independent judgment, a masterful raconteur, and a delightful commentator both on the vagaries of his household and on the vicissitudes of Swiss and European politics.

The remainder of the book consists largely of

letters to Herzen from a variety of figures, most of them radicals. The Italians include Leopoldo Spini and Mazzini; the only German represented is Moses Hess, whose interesting criticism of Herzen's The Russian People and Socialism has been previously published, as have a number of other documents that the editors have seen fit to include, duly indicating the prior publication.

The content of many of these letters is disappointing. Four unpublished letters from Herzen to Louis Blanc is an exciting prospect, but they are of interest only for a defense of Bakunin from the charge, made by a Russophobe English diplomat, that he was an agent of the Russian government. Despite Herzen's considerable contact with the Swiss radical politician James Fazy, we are given only one letter, asking Herzen for a loan. The letters from Polish emigrés are somewhat more substantive, and Joachim Lelewel and Stanislas Worcell are important figures. But this group of letters is longer on noble hopes and grateful sentiments than on concrete information.

One may perhaps sum up by saying that this book was deserving of publication; still, it will be of circumscribed interest to a relatively small group of historians of European radicalism in the doldrums after 1848.

ABBOTT GLEASON
Brown University

ALLEN SINEL. The Classroom and the Chancellery: State Educational Reform in Russia under Count Dmitry Tolstoi. (Russian Research Center Studies, 72.) Cambridge, Mass.: Harvard University Press. 1973. Pp. xii, 335. \$14.00.

No one can ever again frighten naughty children with the spector of Count Dmitry Tolstoi -Allen Sinel has defused him for us. This book is corroboration of what many other recent studies of official Russia have been showing us—that the tsarist bureaucracy was no better and no worse than it should have been and that it operated rationally enough within its own frame of reference. This observation helps to explain the relative social stability of the system, at least until the time of Nicholas II when everything seemed to fall apart at once. To!stoi's problem as minister of education lay in the inherent contradiction of his task: to create a modernized (fully literate and partially learned) nation but not a critical one. The entire tsarist apparatus faced the

But that is not to say that Tolstoi had no

options. Sinel points directly to the major miscalculation of Tolstoi's career: he created a highly centralized system in which the government itself was responsible and held responsible by society) for every mistake. By curtailing any possible autonomy in the system, Tolstoi created a situation in which St. Petersburg took all the blame. As Kadet V. A. Maklakov, an alumnus of the Moscow Third Gymnasium, tells us, "Long after finishing the gymnasium, I could not keep myself from spitting if passing its building."

Sinel's book is not a rehabilitation of Tolstoi. The author allows himself to fall prey to the rationalizations in the vocabulary of Tolstoi and his associates, and he never loses sight of the political motivations. But he goes beyond the superficial hostility of the liberal critics and examines before he indicts. There is a certain restraint in the way in which Sinel, a university professor with deep concern for academic freedom and autonomy, ceals with the autocratic minister of education. "It was not a convincing thesis," he remarks, after describing Tolstoi's argument for total control over curriculum. Occasionally one feels that Sinel controls himself only by the use of outrageous extended metaphors, which use humor to deflect his own sense of outrage. The many medical similes are a case in point: Russian society is sick, Dr. Tolstoi prescribes a medicine, Russian society cannot swallow the pill, Dr. Tolstoi fears an epidemic and prescribes purgatives, and so on. The touch of black humor makes a point that Sinel, speaking soberly in his own voice, hesitates to make.

One of the strengths of the book is the broad perspective. Sinel, of course, comments on the contemporary European educational situation; he also gives his own view of the problems as seen from the perspective of a century later. He continually brings our attention to college trustees and state legislatures, student rebellions and the demand for relevance, and sc on. Such contemporary yardsticks have their disadvantages, of course; they become outdated very quickly. The concerns of the academic year 1970-71 are already almost as distant as 1870. In the midst of the strangulation of the academic marketplace of 1974, it is amusing to discover that "the Russian academic class [was] too small to furnish enough qualified lecturers. Statistics compiled by 1868 substantiated the rectors' complaints about the many empty professorial chairs."

LINDA GERSTEIN
Haverford College

M. G. VANDALKOVSKAIA. M. K. Lemke—Istorik russhogo revoliutsionnogo dvizheniia [M. K. Lemke—Historian of the Russian Revolutionary Movement]. Moscow: Izdatel'stvo "Nauka." 1972. Pp. 217.

v. s. vasiukov, editor. Kritika burzhuaznoi istoriografii sovetskogo obshchestva [A Critique of the Bourgeois Historiography of Soviet Society]. Moscow: Izdatel'stvo Politicheskoi Literatury. 1972. Pp. 410.

The historian Mikhail Konstantinovich Lemke (1872-1923) helped the Russian Revolution discover its past. He wrote or edited a number of scholarly articles and books on the nineteenth-century revolutionary movement, including Nikolaevskie zhandarmy i literatura 1826-1855 (St. Petersburg, 1908), Ocherki osvoboditeľ nogo dvizheniia shestidesiatykh godov (St. Petersburg, 1908), and a twenty-two-volume collection of Alexander Herzen's writings (1915-25). Now M. G. Vandalkovskaia, herself a Herzen expert, has written an excellent study of Lemke that shows how his views of the revolutionary movement developed with respect to other historians, his personal life, and political events around him.

Vandalkovskaia divides her book into three sections: a biography of Lemke, an analysis of his work on Herzen, and a study of Lemke's writings on the "men of the 'sixties," including N. G. Chernyshevski and N. A. Dobrolyubov. In them she shows how Lemke moved from provincial journalist and zemstvo liberal in 1905 to a "Red professor" and Bolshevik in 1922, only a few months before his death. She concludes that "politically Lemke accepted Marxism but his historical conception continued to exist in the framework of liberalbourgeois historiography and never reached the level of his political position." In other words, Lemke remained throughout his life a scrupulous editor and scholar who always returned to his primary sources and, when possible, interviewed the surviving relatives of his subjects. If Lemke's Herzen and Chernyshevski turned out to be liberals in 1905 and revolutionary socialists after 1917, this shift in perspective never compromised his scholarly standards. Vandalkovskaia, too, despite a bow to some similarities between Lemke's and Lenin's views of Herzen, has used Lemke's unpublished diary, essays, letters, and family archive with great care.

This book should interest anyone concerned with either the revolutionary movement or Soviet historiography. For Lemke ultimately helped legitimize the Revolution by elucidating its historical sources.

V. S. Vasiukov has edited a collection of essays whose content is less serious. His purpose is to demonstrate the anti-Soviet bias of Western scholarship, whether "reactionary-conservative" or "bourgeois-liberal." The book contains essays by different authors on the October Revolution, the civil war, the New Economic Policy, industrialization, collectivization, and World War II. There is no discussion of Stalin's rise to power, the purges, the Nazi-Soviet pact of 1939, or Stalin's behavior after the German attack in June 1941. Stalin himself appears very infrequently, and then as neither hero nor villain.

There is much criticism and occasional praise of various Western historians. Those who portray October as a fortuitous coup d'état (Daniels, Ulam) are chastised; those who stress the importance of the labor movement (Haimson, Liebman) are to be congratulated. There is too much sympathy for the White Army generals (Brinkley, Footman) and not enough emphasis on Western military aid to them. The New Economic Policy appears in Western historiography as a combination of exhaustion and concession, a "peasant Brest-Litovsk," but it was really a conscious socialist policy. Theodore Von Laue is "completely incorrect" to compare Witte's state capitalism with the Five-Year Plans (p. 150). Although there were some "mistakes," as in the winter of 1929-30, collectivization was no "revolution from above" but a cooperative movement. On the other hand, E. H. Carr is "one of the most objective bourgeois researchers," and American revisionist historians (W. A. Williams, Alperowitz, and Horowitz) are "realistic thinking bourgeois scholars" (pp. 151,

It is difficult to compare two books whose purposes are, respectively, historical and political. The Soviet historian, it would seem, may deal seriously with nineteenth-century revolutionary antecedents, but he must beware the pitfalls of twentieth-century postrevolutionary consequences.

ROBERT C. WILLIAMS
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STEPHEN F. COHEN. Bukharin and the Bolshevik Revolution: A Political Biography, 1888–1938. New York: Alfred A. Knopf. 1973. Pp. xix, 495, xvii. \$15.00.

For the general reader, if not for the scholar, the story of the Bolshevik Revolution, indeed of Russian communism at large, has been preempted by the towering figures of Lenin, Trotsky, and Stalin. This, while understandable, has not been altogether desirable. Indeed, even the careers of those "three who made the Revolution" can be seen in a true perspective only if examined against the life stories of others whose importance to the movement at times rivaled that of the famous trio. We have lacked a full-fledged treatment of the people who are as important to the understanding of the beginnings of Bolshevism as Krasin and Bogdanov. In the post-October period the fortunes of communism were affected, at times critically, by actions and/or writings of some half dozen people. To a Russian communist they would have seemed, at least until 1922, more important than Stalin and yet they remain, except to a scholar in the field, virtually unknown. Mr. Cohen's biography is thus doubly welcome: it is a first-rate book, and one hopes that it will start a trend.

To write a biography of Bukharin is to undertake an enticing, but by the same token a somewhat risky, intellectual enterprise. It is enticing because Bukharin was clearly one of the most attractive and intellectually interesting of the Bolshevik luminaries. A man of humane impulses and of a wide range of intellectual interests (George Gamov recorded how after a lecture of his in the twenties Bukharin quizzed him about possibilities of nuclear fission), he was clearly the most engaging figure among Lenin's lieutenants and, at the same time, a weightier intellect than most of those who later tried to bar the way to the full horror of Stalin's despotism. Yet by the same token the biographer runs the risk of succumbing to Bukharin's undoubted charm, of seeing in him not only a man of humane impulses, but a tolerant and wise statesman something, alas, he was not; not only an erudite and often scintillating essayist but a profound thinker, a verdict that cannot be endorsed. At times Professor Cohen appears quite conscious of danger on this count. In fact, one can hardly improve on his characterization of his hero, when in the beginning of the biography he sees the consciousness of the threat of the totalitarian state as contributing to some of Bukharin's "most dishonest and tortuous rationalizations of Soviet developments [even though] over the years it was a liberalizing element in his Bolshevism, part of what made Bukharin, despite his chronic public optimism, a man of private fears." The author catches on to the radical intellectual's fatal propensity, especially

in a crisis, to seek to be one of "the boys," not yielding in toughness to a Stalin or Trotsky when he acknowledges that during the civil war "Bukharin produced some of the most gruesome statements legitimizing Bolshevik violence." Yet at other times this insight and salutary caution abandon the author: Bukharinism is seen as a viable alternative to Stalinism, the true prefiguration of "socialism with a human face," which one hopes will inherit and transform the communist world. One does not have to subscribe to Solzhenitsyn's scathing characterization of Bukharin (where the great writer displays less than his usual compassion) to recognize such views as unrealistic. It is, in fact, inconceivable to imagine Bukharin as the leader of a communist regime rather than an ideological coadjutor and spokesman for some stronger man. Was there in reality such a thing as Bukharinism, the term that like Trotskyism was originally invented by the Stalinists as one of opprobrium? It is difficult to elevate Bukharin's sensible, even if not forceful enough, strictures on the folly of Stalin's war on the Russian peasant into an ideology. "Socialism with a human face" would, as a bare minimum, require the possibility of political dissent and free discussion within the general consensus on socialism, something that no Soviet leader, Bukharin included, was, after 1922, willing to concede except and until his own views were being repressed. And so the fundamental tragedy of communism was not that it eventually led to Stalin's despotism, but that it stilled or rendered ineffectual the humane impulses of people like Bukharin. We must be grateful to Mr. Cohen for tackling this complex problem. One must disagree with an occasional conclusion and question some of his interpretations, but one cannot fault his scholarship or his skill in telling this tragic story so well.

ADAM B. ULAM
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NICOLAS DE BASILY. Diplomat of Imperial Russia, 1903–1917: Memoirs. (Hoover Institution Publications 125.) Stanford: Hoover Institution Press, Stanford University. 1973. Pp. x, 201. \$6.00.

The author of this memoir, Nicolas de Basily, came from a Romanian-Albanian family whose members had been prominent in the Russian diplomatic service. After attending the Alexander Lyceum in St. Petersburg, he, too, entered the foreign ministry. The chief focus of this

account is on the war and, in particular, on the period leading to the abdication of Nicholas II. At this time the author was the director of the diplomatic chancellery at the army headquarters at Mogilev. In this capacity he drafted the document of abdication. The memoir gives a detailed description of the circumstances surrounding this event.

In many ways a typical memoir of the time, Basily's concentrates more on depicting court life and the personal characteristics of the leading statesmen than on political analysis. As a descriptive account of the general conditions and atmosphere surrounding the tsarist government prior to its downfall, this book can be recommended in particular for use in classes on Russian history. Basily was not, however, a keen or critical observer of the contemporary diplomatic scene. A large portion of this account is devoted to the question of the Turkish Straits. A lengthy memorandum (thirty pages) written in November 1914 is appended. Basily saw as the ultimate goal here the annexation of the Straits with a suitable hinterland as well as the acquisition of the Aegean islands of Tenedos, Imbros, Lemnos, and Samothrace, although he realized that such wide aims were not easily attainable. He also recognized that free passage through the Straits would give the Russian navy "the opportunity of becoming a menace in the Mediterranean, and that, if we possess important naval forces, would considerably strengthen our influence in the world. As already stated, the Straits are an excellent naval base for operations in the Mediterranean" (p. 158). What is remarkable about this memorandum is that it is written quite in the spirit of similar analyses made in the nineteenth century when Great Britain, with her preponderant sea power, was the great adversary. All of the standard arguments of the past are repeated with really no allowance made for the fact that by November 1914 Britain had become a wartime ally while the former tsarist partners, Germany and Austria-Hungary, had, in contrast, become enemies, a shift that profoundly affected the significance of the Turkish Straits for Russian security.

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ALFRED LEVIN. The Third Duma, Election and Profile. [Hamden, Conn.:] Archon Books. 1973. Pp. x, 210. \$8.50.

This monograph, like Professor Levin's previous work, The Second Duma (1966), is another of

the much-needed studies contributing to the history of the short-lived Russian attempt to set up a constitutional monarchy. The adoption of that new governmental form was neither easy nor successful, and this thorough study, stressing various aspects related to the election and function of the Third Duma, shows this to the satisfaction of an inquiring scholar. The more generous attitude of the tsarist regime toward the constitutional practices ceased with the dissolution of the Second Duma and the new electoral law of June 3, 1907. Although the law retained the principle of representation for broad categories of the population, in essence it aimed to produce a submissive and harmless body serving the regime, not the people. To this end the law established an elaborate system of indirect elections, multiplication of voter categories (curia), and other restrictions. To ensure success, the favoring of property owners, both rural and urban, and of Russian nationalists became the guiding policy of the premier, Peter Stolypin.

The scheme worked well, and the author did not encounter any difficulties in proving it from the large number of sources and publications available to him. Notes supporting his findings (496 in all) run through pages 153-86. The work, with hundreds of data and figures, offers in the first four chapters a profile outlining some aspects of all political parties. The chapter "The Restless Borderlands," the weakest in the book, deals primarily with the Poles, the Jews, and the Muslims and ignores Ukrainians altogether, despite their being the largest non-Russian nation (some thirty millions). Other chapters analyze the relationships among the political parties, the attitude of the voters, the elections, and the formation of factions together with their relationship in the new Duma. The victory of autocracy, orthodoxy, and Russian nationalism in the elections (301 deputies out of 442) reflected a retreat from the October Manifesto of 1905.

Levin corrects the belief of some historians that autocracy was totally alienated from Russian society. There was little disagreement across the whole spectrum of the Russian political parties about the question of the preservation of the Russian colonial empire at the expense of the forty-five per cent who were non-Russians.

Other features of Levin's findings reveal the ignorance and indifference of the peasantry, the inability of political parties to compromise on essential issues, and the ability of the traditional Russian statist system to recover

from the setbacks of the events of 1905. Hence, the collapse of the tsarist regime in 1917 cannot be explained with exclusive reference to internal problems; war and military defeats were the main cause and all other factors only accompanying elements.

By employing a minutely descriptive style, the author made this study exclusively for experts. But even this group would have been helped had he realized the advantages of tables, graphs, and even an ethnographic map illustrating the national composition of the Russian empire. The preference and special attention extended some peoples and the neglect to elaborate on the problems of other nationalities create the feeling of uneven treatment and selective objectivity. However, these shortcomings would not suffice to question Levin's scholarship, competency, and the value of the study itself. Students of Russian history will appreciate this contribution that stimulates discussion and further inquiries.

STEPHAN M. HORAK
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C. VAUGHAN JAMES. Soviet Socialist Realism: Origins and Theory. New York: St. Martin's Press. 1973. Pp. xiii, 146. \$9.95.

Mr. James has produced a valuable study of the central governing methodology which dominates the whole of art and literature that is officially acceptable in the USSR. Further, he includes the impressive array of institutions and practices employed to assure the enforcement of socialist realism.

Socialist realism is so foreign to the Western democrat imbued with the values of an open society that there is a strong tendency to reject it as something antithetical to human nature but another of Stalin's artificial inventions in the name of maximizing centralized power. Stalin certainly utilized the concept to this end. Nevertheless, the proponents of socialist realism are surely closer to the mark in arguing that it is Leninist, based upon an evolutionary development rooted in Marxian philosophy.

In further developing Marxism, Lenin claimed to have perceived details of the applicable laws and, in creating the party vanguard, to have discovered an all-knowing head whose primary duty (second only to perpetuating its place in power) was to spare no effort (or blood) in seeing to it that society marches in the direction of the ultimate goal. Thus the essence of socialist realism is "based on a direct relationship between the artist and the

process of building a new society; it is art colored by the experience of the working class in its struggle to achieve socialism" (p. 88). Again, of course, only the party knows what the working class really wants.

Perhaps a good way to understand the vital importance of socialist realism to the Soviet system is to recall that the Marxist-Leninist-Soviet definition of freedom is the "perception of necessity." The known absolute goal is the construction of communism. Marxian-Leninist laws delineate the correct path to communism, and socialist realism provides the proper consciousness of the masses so that they will willingly, indeed joyfully, proceed along the prescribed route. As Stalin observed in 1932, "If the artist is going to depict our life correctly, he cannot fail to observe and point out what is leading it toward socialism. . . . It will be Socialist Realism" (p. 86).

As such, all art worthy of building a classless society can be of only one kind. There can be no painters' painter, no literature intelligible to only a relatively small handful of intellectuals. Only a popular art intelligible to the totality of the masses is acceptable.

James demonstrates that this Marxian-rooted "essentially Leninist" idea is derived from "three basic principles of Soviet aesthetics—naródnost (literally people-ness) . . . hlássovost (class-ness) . . . and Partiinost (party-ness)—the identification of the artist with the [Soviet] Communist Party."

No human can be fully objective. The reader deserves to know the author's own view, if only stated in a paragraph in the preface. I rather seriously fault James for his pretense at neutrality. There are other shortcomings—for example, the kulaks who were destroyed were not just "peasants who employed others" (p. 77). The work is not fully objective; it is not perfect, but it is a very valuable study of a vital aspect of life and thought in the USSR. Anyone who reads James's book will find it hard to dismiss the awesome reality of Soviet socialist realism.

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P. A. ZHILIN et al., editors. Vtoraia mirovaia voina i sovremennost' [The Second World War and the Present]. (Akademiia Nauk SSSR, Institut Voennoi Istorii, Ministerstva Oborony SSSR.) Moscow: Izdatel'stvo "Nauka." 1972. Pp. 854.

The key to the scope of this book is the second part of the title; whereas the reader might an-

ticipate a military treatise, the collection is really about the lessons the World War II experience provides in the official Soviet view. These "lessons" are summarized in the introduction and the first and third articles. The last three articles have an extremely tangential relationship to the war: they treat postwar United States policy, NATO, and Soviet bloc politics. In addition to a general survey of the Soviet military contribution to the defeat of the Axis, there is also a special chapter on the war against Japan, which advances the astonishing claim that the Red Army brought about the defeat of Japan at Guadalcanal. Three articles discuss resistance movements in Europe and its colonies, emphasizing the role of Soviet citizens and Communists. Another article essays to demonstrate that Soviet military help was instrumental in the formation of the Eastern European "people's democracies," but that the decisive element was indigenous popular demand. A less tendentious article summarizes recent Soviet conclusions on the nature of Soviet partisan organization, recognizing some of its shortcom-

The remaining eight articles (of a total of twenty) treat aspects of the diplomatic history of the war and its prologue. Of these, four pieces dealing with the onset of the war and its spread to the USSR are restatements, in an extreme form, of the familiar recent Soviet thesis that the Western powers (including the United States) plotted to turn Hitler against the USSR. Since the articles rely primarily on more detailed Soviet works and on Western revisionist historiography, with only occasional reference to published documents and memoirs, the historian will hardly find this part of the book very useful. The remaining four articles do deal with monographic subjects that have received comparatively little special treatment in Soviet historiography: Japanese plans on the eve of war, Italian preparation for the attack on Greece, Switzerland's precarious position, and the role of Scandinavia. Although no new Soviet sources are cited, bibliographical references to a wide range of Western literature may be helpful. Like the remainder of this book, however, these four monographs must be approached with caution. Thus, while correctly stressing anti-Soviet motives behind the Anglo-French plans for intervention in Scandinavia in late 1939, one author completely omits mentioning the substantial Soviet naval assistance then being rendered to Germany in the adjoining Barents Sea area. More surprising is the assertion that British "provocative actions" (in

seizing the Altmark and mining the Norwegian Leads) brought about Hitler's invasion of Scandinavia, especially considering how other authors in the collection berate Britain for failure to carry on the war vigorously and criticize Switzerland for a neutrality that "objectively" assisted the Axis.

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## NEAR EAST

ARTHUR RUPPIN. Memoirs, Diaries, Letters. Edited with an introduction by ALEX BEIN. Translated from the German by KAREN GERSHON. Afterword by MOSHE DAYAN. New York: Herzl Press. 1971. Pp. xix, 332. \$6.95.

In contrast to other societies in the underdeveloped areas of the world, Israel stands out as an anomaly because its industrial development was firmly rooted in a highly productive agricultural system and in highly motivated, literate, and intensely nationalistic farming communities. As a consequence Israel was able to take off in its industrial development immediately following its liberation from British imperial controls and both absorb and support a vast inflow of immigrants. To the degree that any single individual can be credited with this remarkable achievement, it was Arthur Ruppin, who as early as 1907, when he first came to Palestine to evaluate settlement plans for the Zionist Organization, recognized the crucial role of a productive agricultural sector manned by dedicated Jewish settlers for a viable Jewish homeland in Palestine. Ruppin was then ready to support such highly innovative cooperative ventures as the kevutzah and the moshavah.

Ruppin was no less sensitive to the Arab problem. He struggled to find some modus vivendi that would allow for expanding Jewish settlement without displacing the Arabs, and for many years he was active in the Brit Sholom, which advocated a binational state. At the same time he was realistic enough to recognize that Arab nationalism could not be fended off by the economic benefits that would follow from a modernizing beachhead of Jewish settlement in the Near East, and he had no alternative but to support a viable Jewish homeland, however violent the Arab response. Nonetheless, he always clung to the hope for reconciliation.

All these qualities that made Ruppin so distinguished a Zionist leader come through in Near East 445

this book, a thoroughly absorbing and sensitive self-revelation. The work is divided into two parts. The first is an autobiographical account of his early years until his exile to Constantinople during World War I. The second is excerpts from his diaries and letters selected by Alex Bein. Both sections prove fascinating reading and serve to illuminate for the historian the intriguing process by which a Jewish boy born to well-to-do parents in a small German town, Rawisch, but reared in extreme poverty following on his father's failure in business in Magdeburg became first a successful grain merchant, then by assiduous effort a student at the Universities of Berlin and Halle, and finally a fully qualified lawyer and the first sociologist of the Jewish people ultimately to use his "Western" skills to build a homeland for the Jews in a highly primitive slip of land in the Near East.

Alex Bein is to be commended for his perceptive introduction, the quality of his selections from the diaries and letters for 1920–42, and for his inclusion, as an afterword, of a speech given by Moshe Dayan before the graduates of the Israel Defence Force Command and Staff School in September 1968 that reflected on Ruppin's open-ended approaches to the Arab question. An especial commendation is due to Karen Gershon whose English rendering of Ruppin's German made it difficult for me to put the book down.

ELLIS RIVKIN

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JACQUES THOBIE. Phares ottomans et emprunts turcs, 1904-1961: Un type de règlement financier international dans le cadre des traités. (Publications de la Sorbonne, Université de Paris I—Panthéon-Sorbonne. International Series, 3.) [Paris:] Éditions Richelieu. 1972. Pp. 218.

Despite the catchy title, there is nothing here about lighthouses. The subtitle describes the subject, which is the international financial negotiations of a group of French capitalists to get from Turkey and other Ottoman successor states payments due on three pre-1914 loans made to the old empire. The loans were secured by the income of the Administration Générale des Phares de l'Empire Ottoman, which collected fees from ships using Ottoman ports.

Originally a Sorbonne thesis, this essay in economic history is drawn almost entirely from records of the French lending group headed by Pierre de Vauréal. Thobie shows meticulously how the group tried to get from

the Ottoman successor states the full interest and capital due them in Turkish gold liras under the original contracts. The three loans totaled 855,000 liras, a modest affair but important to the dozen or so lenders. They were successful negotiating with the mandatory governments of Syria, Lebanon, Palestine, Trans-Jordan, Iraq, and Italy (the Ottoman successor in the Dodecanese Islands). Although Greece paid only two-thirds, although Bulgaria and Yugoslavia made only minimal token payments, and although Al-Hijaz, Nejd, Asir, and Yemen paid nothing, the creditors emerged ultimately with a tidy ninety-four per cent of their claims.

The meat of the story is the negotiations with Turkey, which owed by far the largest sum. The republic did not, like Russia, repudiate its predecessor's debts but stubbornly insisted that times had changed since 1913 and that Vauréal's group be treated like the Ottoman Public Debt bondholders, scaling down claims by about half. After sometimes stormy negotiations between 1928 and 1933 the Turks won. Sükrü Saracoğlu and Vauréal reached an agreement whereby the creditors abandoned their contractual claims, accepting a compromise total in fifty-year Turkish bearer bonds at 7.5 per cent.

Much of the account is arid, though usually clear. There are many tables, in which a few columns add incorrectly. Only occasionally do the dramatic aspects of the story appear; this is in part because almost all individuals remain names only, not personalities. Vauréal and Antonin de Mun are exceptions. Important Turkish individuals are hardly made flesh and blood. Thobie is fair about presenting the Turkish nationalist position, but he evidently sought no documents or interviews in Turkey.

Were the whole story of lighthouses, concessions, and loans fully developed, there is probably material for a book as fascinating as David Landes's Bankers and Pashas (1958). Since the Administration des Phares was run by a French firm owned by Vauréal and others of his lending group, and since Vauréal was administrator of the Phares, it appears that Thobie has left out half the story in his singleminded concentration on international financial haggling. What he has done is on the whole well done. He should do more.

RODERIC H. DAVISON

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MICHAEL LLEWELLYN SMITH. Ionian Vision: Greece in Asia Minor, 1919-1922. New York: St. Martin's Press. 1973. Pp. 401. \$14.95.

This book, a successfully popularized doctoral dissertation, comes as the first and very welcome synthesis of widely scattered archival and memoir materials concerning the most crucial turning point of modern Greek history: what the Greeks themselves refer to as the "Asia Minor Catastrophe."

For the specialist the book's chief contributions center on the diplomatic history of Greek relations with the Allies and particularly Great Britain. What we have here is a fascinating model of interaction between a great power and a client state, acted out by charismatic personalities like Venizelos and Lloyd George, in an undertaking of great relevance to our own post-World War II era (i.e., Truman Doctrine, "Vietnamization"): the attempt to establish an Anglo-Greek condominium in the Eastern Mediterranean.

The ensuing rapid rise of Greece to the status of a solid second-class power, capable of replacing Ottoman Turkey in British Middle Eastern policy, is truly breathtaking. The equally rapid fall from this pinnacle adds to the narrative a dramatic quality transcending ordinary diplomatic and military history—something akin to the epic sweep of War and Peace. This is conveyed by the author with a fine sense of perspective, neither exaggerating nor belittling the role of the protagonists in complex, multidimensional events.

Throughout the narrative there is an awareness of social and economic factors, which make it rich in incidental insights into Greek political processes and institutions. The most interesting such insights concern the familiar phenomenon in modern Greek history of the inability of the liberal Center to assume extraparliamentary forms of power at times of crisis, thereby creating the kind of power vacuum that invites military interventions.

To the student of nationalism, this book offers an outstanding illustration of the irresistible dynamics of what we might call "national Bonapartism" resolved in a supreme military adventure.

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## **AFRICA**

DOUGLAS FRASER and HERBERT M. COLE, editors. African Art & Leadership. Madison: University of Wisconsin Press. 1972. Pp. xvii, 332. \$17.50.

For centuries African rulers have used art to symbolize their power and legitimacy. Regalia such as swords, stools, statues, crowns, and staffs are essential to the ceremonial life of African courts and serve as reminders of the unique status of the monarch. Royal art may provide a visual link between the dynasty and important myths and religious beliefs, commemorate past glories, or show the king's wealth. Even in stateless societies art objects may indicate rank and prestige.

Although this pioneering book is not primarily directed toward historians, they will find much of interest in its fourteen essays on art associated with leadership in eleven West African and Congolese societies. As the editors make clear in their introduction, the essays focus on the plastic arts to the virtual exclusion of politically significant performing arts.

Daniel Biebuyck (Lega) and Herbert Cole (Ibo) discuss objects associated with secret societies in acephalous groups. Simon Ottenberg's "Humorous Masks and Serious Politics among the Afikpo Ibo" is an analysis of the social and political functions of satirical plays conducted by young masked dancers. Daniel Crowley's essay on Chokwe political art touches on several important issues, including royal patronage of carvers and trends toward secularization. The ndop statues of Kuba kings are described by Jan Vansina, who argues that even the oldest ndop are originals or faithful copies. Thus the statues illustrate royal dress over three centuries. The changing political uses of sacred masks are discussed in a fascinating article by Leon Siroto on the BaKwele of Congo-Brazzaville.

Many historians will take special interest in René Bravmann's contribution, "The Diffusion of Ashanti Political Art." Bravmann argues that the Ashanti government used presents of regalia as a device to control outer provinces, especially in non-Akan areas. His study of Nafana political art shows that many objects were gifts from Ashanti rulers and can be linked with specific personalities and events. Similar studies elsewhere in the Ashanti hinterland might clarify chronological problems and the extent of Akan cultural influence.

Douglas Fraser's article, "The Fish-Legged Figure in Benin and Yoruba Art," is less satisfactory. He traces representations of men with fish-legs through various periods of European and Asian art and concludes that southern Nigerian carvers were influenced by motifs used in the Eastern Roman Empire before 1000 A.D. Fraser is aware of some of the dangers of diffusionism; he does not postulate wandering Byzantines in Benin. But, despite resemblances

Africa 447

between Yoruba figures and specimens from Afghanistan, I suspect trait-chasing.

Other articles deal with the Cameroons Grasslands (Suzanne Rudy), Baule gold-plated objects (Hans Himmelheber), Yoruba beaded crowns (Robert Thompson), Kwahu terracottas (Roy Sieber), Ashanti regalia (Douglas Fraser), and Ife sculpture (Frank Willet). The editors' overview examines common features of political art and stresses how little is known about African art history.

African Art & Leadership is a commendable attempt to breach disciplinary boundaries and, inevitably, raises more issues than it solves. Why not, for example, something on Dahomey, Ethiopia, or the depiction of Europeans and their artifacts? How can students of history and art collaborate? Numerous black and white pictures make this book as esthetically pleasing as it is intellectually stimulating.

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Charlotte

VIRGINIA THOMPSON. West Africa's Council of the Entente. (Africa in the Modern World.) Ithaca: Cornell University Press. 1972. Pp. xxiii, 313. \$13.50.

This book belongs to the series Africa in the Modern World, edited by the distinguished political scientist, Professor Gwendolen Carter, who points out that English-speaking readers know much less about francophone Africa than about the successor-states of the British Empire, which "present no language barrier" (p. viii). Although English-speaking historians might observe that lack of the conqueror's tongue is not the only linguistic barrier, in practice, despite some distinguished exceptions, they have neglected French-speaking Africa rather more than political scientists and should be grateful that two of the first four volumes of this series focus on these states. (The survivors of the scramble, Liberia and Ethiopia, equally neglected, make up the other two.)

The series supplied Dr. Virginia Thompson with a difficult subject. The Entente's admirers praise its longevity in contrast to more radical regional organizations. But not the least of Dr. Thompson's achievements is to hold the reader's attention while cataloging the many failures to develop joint policies. The price of survival appears to be a lifetime of nonevents.

Dr. Thompson explains the Entente's formation by interweaving episodes from President Houphouët-Boigny's manipulative diplomacy with the development of Upper Volta, Dahomey, Niger, and Togo. These complicated sequences are handled with authority, but, presumably because the Ivory Coast will have a separate volume, Dr. Thompson does not explore the close connections between Houphouë:'s domestic and foreign policy in similar depth. As Houphouët "preferred to arbitrate disputes between . . . members on a personal basis rather than to have them settled by the round-table method" (p. 33) concentration on the Entente entails a limiting perspective on Ivory Coast foreign policy.

The overthrow of President Yameogo of Upper Volta merits closer examination for the light thrown on the problems of the Entente's Solidarity Fund. More important, readers will hardly be equipped to understand contemporary African discussion of these states without explicit acquaintance with the views of radical thinkers like Samir Amin and Jean Pierre N'Diaye. The debate between liberals and radicals on neocolonialism is far from academic in the bad sense of that term. Nevertheless, West Africa's Council of the Entente exhibits the essential property of success: the whole is greater than the sum of its parts. Imperial and postimperial structures do not provide those who write about them, any more than those who live in them, with the ready-made coherence supplied by nation-states. Readers will admire Dr. Thompson's resource in keeping her somewhat amorphous but significant subject in sharp focus.

HENRY S. WILSON
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A. G. HOPKINS. An Economic History of West Africa. (The Columbia Economic History of the Modern World.) New York: Columbia University Press. 1973. Pp. x, 337. \$15.00.

Hopkins presents a masterly synthesis of West African history from early times to independence within an analytic framework derived from economists and economic historians specializing on the "underdeveloped" world. The result is an extremely stimulating and highly readable study, one that incorporates an enormous amount of scholarship and challenges the reader to re-examine his thinking on numerous issues. The introductory chapter, "Approaches to Africa's Economic Past," surveys the conceptual and methodological (and mythological) issues outstanding and introduces "the market" as the key organizing principle, which is conceived in quantitative, spatial, and social-structural terms.

Chapter 2, "The Domestic Economy: Structure and Function," makes admirable utilization of data from anthropologists and geographers combined with comparative economic theory and tools of analysis. Chapter 3, "External Trade: The Sahara and the Atlantic," provides an informed and balanced discussion of both commercial spheres analyzed against a model of international trade, which helps to explain the limited growth of the market in both instances. Chapter 4, "The Economic Basis of Imperialism," examines the changes in import and export structures accompanying the growth of legitimate trade in the nineteenth century and the changes in the terms of trade at the close of the century that intensified European competition for West African markets. Chapter 5, "An Economic Model of Colonialism," delineates the principal structural features of the colonial period and employs quantitative data to chart the development of the colonial economies from 1900 to 1960. Chapters 6 and 7 rework the same time period from different perspectives: "Completing the Open Economy" assesses the respective roles of Africans and expatriates up to around 1930, and "The Open Economy under Strain" covers the years from 1930 to 1960 when extra-African influences increasingly affected economic, social, and political developments in West Africa. Chapter 8 is a four-page coda, a combination summary statement and historian's invocation to his fellows and to policy makers. Each chapter is well organized and argued and marked by fairminded discussion of controversial theses and opposing viewpoints. The twenty-nine-page select bibliography attests to the author's scholarship and dedication to the Sisyphean task of "keeping current." Seventeen maps and six graphs, together with footnotes printed at the bottom of the page and impeccable copyediting, merit the publishers special acknowledgment. An Economic History of West Africa is certain to become recognized as the standard work on the subject, as an indispensable first reader and reference book, and as the point of departure for new scholarship for years to come. It is indeed the pacesetting volume launching the Columbia Economic History of the Modern World series.

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NEHEMIA LEVIZION. Ancient Ghana and Mali. (Studies in African History, 7.) London: Methuen and Company; distrib. by Barnes and

Noble, New York. 1973. Pp. x, 283. Cloth \$10.00, paper \$6.50.

For many years Nehemia Levtzion has been studying the ancient empires of Ghana and Mali, basing his work on Arabic and Portuguese works, the chronicles of black Muslim literati, and surviving oral traditions. He has now provided us with the first full-length account of the two states based on research of a consistently high quality. Ancient Ghana and Mali is divided into two parts, the first of which describes the rise, expansion, and disintegration of Ghana, followed by the expansion of Mali into the Sahel. In the fifteenth century Mali declined and many of its people, the Malinke, migrated south to the fringes of the forest region. Part 2 is a detailed study of the principal themes of trade, government, and Islam. The importance of the gold trade in the expansion of the Sudan and its relations with North Africa and Europe is rightly stressed. Levtzion builds up a fascinating picture of a sophisticated people with strong cultural traditions, effective governmental systems, and a thriving trade.

The major thrust of Levtzion's research has involved Arabic sources. His account of Mansa Musa's famous pilgrimage to Mecca in 1324, for example, is based largely on Islamic sources. But a tremendous amount of work remains, especially in the areas of oral tradition and archeological field research. Large-scale excavations and settlement pattern studies of ancient Ghanian and Malian towns and villages are likely to provide valuable insights into the reasons for the rise and decline of both states. The sophisticated systems approaches to problems of state formation now being applied to early Meso-American and Near Eastern civilizations could yield rich dividends in West Africa. Research into the problems of cultural process and the rise of West African states has hardly begun and requires major archeological investigations into early Ghana before its first appearance in al-Fazārī's chronicle in the late eighth

Ancient Ghana and Mali is essentially a descriptive history compiled by a scholar with a shrewd perception of the uses and limitations of Arab sources. Levtzion's study is an important first step, for it provides a straightforward, closely argued, and well-referenced account of two great African states. But, without question, future research by African scholars is likely to give us a new perception of Ghana and Mali. In the meantime Levtzion has written what is

Africa 449

likely to be the definitive history of Ghana and Mali for some time to come. Fortunately his important study is available in cheap paper-back form, so wide dissemination among students is assured. I only regret the lack of illustrations.

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FRANÇOIS RENAULT. Lavigerie, l'esclavage africain et l'Europe, 1868–1892. Volume 1, Afrique centrale; volume 2, Campagne antiesclavagiste. (Études historiques sur le Cardinal Lavigerie.) Paris: Éditions E. de Boccard. 1971. Pp. 433; 506. 45 fr. each.

There is something strangely anachronistic about Charles Martial Allemand Lavigerie, founder of the White Fathers, archbishop of Algiers, and cardinal of the Church. At about the time when others, including many missionaries, seemed to have a livelier interest in proper drainage than in the Trinity, Lavigerie was dreaming of crusades. There is such a curious time lag in his perceptions of the modern imperial world.

True, he never has received much of a press in the English-speaking regions; their views of him were colored by reports from interested parties in the Scottish churches or with connections to the Imperial British East Africa Company or possibly the consulate in Zanzibar. Whether Renault's massive work provides an antidote is doubtful, for the translation problem remains. Still, this is a colossal two volumes, a compendium nearly nine hundred pages long containing a sixty-eight-page bibliography, some attractive maps, detailed indexes, chapter précis, and the imprimatur of Rome. The title is somewhat misleading for it is not truly a biography, nor is it concerned with those parts of Africa not adjacent to lakes Victoria and Tanganyika, the latter especially. By "slavery" Renault means slavery organized by "Arabs."

Lavigerie unfortunately does not appear in three dimensions. Of course, there may only have been the public man, though this seems unlikely. Still, we are given quite a lot of the official person's life. His aim was clear: the eradication of the East African slave trade. His plan was not complicated, merely impractical, and certainly audacious. Lavigerie proposed to send special religious from Algeria to the lakes region to ransom the likeliest-looking

youths in captivity and send them to some sort of training institute on the Mediterranean (evidently after conversion) where they would be trained in medicine and then returned as missionaries.

At one time he promoted the idea of establishing Christian villages, as had been done in Algeria with some success. The idea was to influence surrounding communities. The cardinal also called for armed volunteers to accompany missionaries to what are parts of today's Uganda, Zaire, and Tanzania. There is no indication that he questioned the value of Europeanization or the benefits of European political aggrandizement. His program, however, called for this to be done under the banner of the Holy See. Throughout he called for European cooperation, but failing that, at least for French and Belgian assistance. He does not seem to have appreciated the extraordinary nationalist sentiments of his age.

English Protestants regarded the cardinal as the late-blooming representative of a heeldragging and compromised institutior. While their specific criticisms usually had to do with the Church's Portuguese connection, they also held that the Church was damaged by its relations with a France insufficiently governed by humane considerations. Lavigerie rather unfairly absorbed a lot of this punishment, and some of his angrier correspondence demonstrates that it hurt. The fact remains that his programs were not new, nor was there any shortage of sensational reporting from the afflicted territories. Most of what he said had been said previously and vigorously by British or British-employed missionaries. Certainly Lavigerie should not be ignored because Livingstone preceded him in the field (literally, as the cardinal was an armchair explorer). But one is left with the impression that until memoranda began circulating through the papal chancery, the cardinal did not accept that anything had been done or that a problem had been defined satisfactorily. Perhaps it was this impression of smug dismissal that put the English off.

Lavigerie first experienced another culture on a trip to Syria in connection with French intervention on behalf of persecuted Christians. This contact with Islam made a great impression and caused him to leave his university post to enter upon a career of active missionizing. With the blessing of French authority he was installed in the archbishopric of Algiers and very soon afterward organized and received papal approval for a new order, the Algerian

Brothers or White Fathers. These received a special mandate for "Central Africa," as the idea of proselytizing in this devastated zone appealed to the Church Militant.

Between 1879 and 1884 Lavigerie dispatched missions to Buganda first and then to various places south of the lake. It cannot be said that they were very successful. In Kampala the situation was impossibly complicated and his men lacked the material backing of their English rivals. Elsewhere they could not dispute the authority exercised by various brigands, some with traditional (or assumed traditional) prestige. Moreover, at least in the English universe, they were accused of surrounding themselves with slaves whose conditions no doubt had improved but whose status had not. Lavigerie indignantly rebutted these charges. There was a vast difference between a ransomed person who showed his gratitude and insecurity by staying in one's service and a bought person who remained of necessity. Besides, the Scottish missionaries in the Shire Highlands did the same thing. The argument was too lawyer-like: your charge is untrue; moreover, it also is your practice.

Those troubles which afflicted the White Fathers also afflicted their competitors. There was a natural tendency to seek the support of sympathetic lay powers, but the peculiar diplomatic position of the Vatican sometimes complicated matters. Thus the ambiguous connection with Leopold, the king of the Belgians, who after the Berlin Conference emerged with a very much strengthened hand. The principal difficulty between the king and the cardinal had to do with the latter's latest call for a united European war against the slavers. Leopold, or more accurately H. M. Stanley, needed the Swahili magnates of the interior. Had the Papal States controlled some force beyond the Swiss Guard, there might not have been quite the same necessity for compromise with secular agencies willing to field a military. As it was, Lavigerie even came up with the idea of a religious armed force, "armed brethren," whose moral code precluded the species of conduct alas too common among lay volunteers, who mostly were rejects from various European armies.

His weird proposal came too late to constitute a chapter in this bizarre period of Central African history. Cardinal and Church alike were overtaken by worldly events, for by 1891 both Germans and Belgians had commenced serious action, though for different reasons. Absurd as it might seem now, the placement of gunboats on the lower tier of lakes irrevocably altered the political situation, and this without benefit of clergy.

The situation became Algerian. The Church would henceforth operate through those European governments that laid effective claim to Central Africa, and men such as Cardinal Lavigerie would become absorbed in the routine of missionizing in colonial circumstances. Renault's judgment may be correct. Though all this comprised a very modest undertaking despite the accompanying rhetoric, it began a dialogue across cultures. Lavigerie aroused Europeans, though very late in the game, somewhat as Livingstone had done earlier in the British Isles. There is, finally, something fascinating about a man with an idée fixe. The main problem with Lavigerie is that he resembled the man who independently invented some useful item after it already had gone into general use. The motive and passion were there but the situation had changed, as had the other players.

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VICTOR T. LE VINE. The Cameroon Federal Republic. (Africa in the Modern World.) Ithaca: Cornell University Press. 1971. Pp. xxiii, 205. Cloth \$8.50, paper \$2.45.

In 1964 Victor T. Le Vine published The Cameroons from Mandate to Independence; writing from the perspective of a political scientist, Le Vine made skillful use of recent anthropological and historical research about Cameroon, and the resultant volume was one of the best studies of a single African state published by an American university press in its decade. The present work is largely an updating of Le Vine's earlier book, which has both advantages and disadvantages. It is a compact, factual, and tightly written survey of the recent political history of Cameroon. All of the important names, dates, and events of the recent past are there in digest form. The narrative sections succinctly and completely identify such topics of continuing importance as the geographic differences between Cameroon's dry, flat north and its rain-forest south, between its English-speaking western section and its larger francophone east. It clearly treats the rise of political parties in Cameroon from more than 125 at one time in the 1940s to their consolidation into the single Union Camerounaise party.

The book traces, in outline form, the rise of Ahmadou Ahidjo, the president of Cameroon, Africa 451

from obscurity in the northern town of Garoua to leadership of one of West Africa's most economically and politically viable countries. Still, in some ways, Ahidjou remains as much an enigma at the book's end as at its beginning. He deserves much fuller biographic treatment. It is regrettable that, although the author devotes major sections to the rise of nationalism and to social questions, the book makes no use of the fictional work of a succession of Cameroon's writers from Mongo Beti to Francois Beybe, whose novels and short stories vividly display the problems of urbanization, economics, and generational differences. It is unfortunate that the book went to press shortly before Cameroon became a unitary state in 1972. This will require some revision, but not much, for Le Vine's assessment of Cameroonian politics is solid, carefully grounded in an easy familiarity with the range of written sources on Cameroon and a personal knowledge of many of the participants in its recent history. What the book sets out to do it does very well. It is a timely, concise history of this complex, stable, and much-ignored African state.

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MARCIA WRIGHT. German Missions in Tanganyika, 1891–1941: Lutherans and Moravians in the Southern Highlands. (Oxford Studies in African Affairs.) New York: Oxford University Press. 1971. Pp. xiv, 249. \$10.50.

Mission history in Africa has experienced something of a revival during the past decade owing primarily to its growing integration with major currents of African and colonial history generally. Professor Wright's book touches on several themes of this scholarly discussion as she focuses on two German mission societies operating in southern Tanganyika. This is in itself an important contribution since little on the German missionary enterprise has been published in English.

One of these themes concerns the politics of mission expansion before and during the era of conquest. It is in chapters 3 and 4 that we are shown most clearly the interaction of African societies with the German missionaries as Wright relates the expansion of the Berlin and Moravian missions to the complex political situation in the southern highlands. A second theme involves the relationship of German missionaries to the colonial government. Unlike neighboring Kenya where early mission-govern-

ment cooperation partially gave way as the implications of a settler economy became clear, Wright argues that German missionaries from the very beginning engaged in a virtual Kulturkampf with local administrators primarily over the government's "reliance on alien elites" for administrative purposes. This early antagonism represents the unique feature of the mission situation in southern Tanganyika. A third emphasis of the current literature involves a new concern for the "orthodox" mission communities and churches as significant social groups in colonial society rather than an exclusive focus on independent churches. Wright contributes to redressing this imbalance by showing how the contrasting styles of the Moravians and Lutherans issued in very different mission communities.

The major weakness of the book grows out of the nature of its sources. Wright's almost exclusive reliance on European documentary sources prevents her from getting really "inside" these emergent communities to the same extent as John Rennie's recent dissertation on American missions among the Ndau of Rhodesia or David Sandgren's forthcoming thesis on the American Inland Mission (AIM) in Kikuyuland, both of which draw heavily on oral material. The sources perhaps also dictated a decreasing emphasis in later chapters on the African side of the story, for as missionaries became enmeshed in their cwn institutions and those of the colonial government, they doubtless became less interested in what was going on around them. Thus the theme of cultural encounter tends to get lost. This is unfortunate since the most recent thrust of mission history involves the interaction of Christianity and African religious systems, a theme only lightly touched upon in this book.

Despite these reservations, Wright's book clearly represents a solid contribution to the political and social history of southern Tanganyika and a most welcome addition to the literature of comparative mission history.

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SAMUEL G. AYANY. A History of Zanzibar: A Study in Constitutional Development, 1934–1964. Nairobi: East African Literature Bureau; distrib. by Rowman and Littlefield, Totowa, N.J. 1970. Pp. vi, 208. \$7.00.

The fertile, clove-growing islands of Zanzibar and Pemba, both part of the British Protec-

torate of Zanzibar since 1890, through their African, Arab, and Indian inhabitants played a vital role during the nineteenth century in opening the East African interior to the outside world. During these years the islands were ruled by an Arab dynasty originating from Oman that continued to preside without executive power over the islands' affairs after the British take-over of 1890. Buttressed by British policies that favored the Arabs over the Africans (both indigenous inhabitants and immigrants from the nearby African continent), the Zanzibar Protectorate remained an Arab-dominated state until a bloody revolution of 1964 firmly secured power in African hands. Ayany purports to study Zanzibar's constitutional development from 1934 to 1964 in this volume, which grew from an M.A. thesis he completed in 1963. When the thesis was undertaken there existed no adequate account of the events of this thirty-year period, and Ayany's research might have been of some real value. Even then, however, there were serious flaws. The historical introduction, for example, includes many errors of detail and interpretation for the early period of Zanzibar's history, including the long-rejected assumption that there was a "Zenj Empire" in East Africa in the period before 1500.

When Ayany reaches the 1930s the quality of his analysis improves. He uses a good range of sources, including many of the published government reports, to portray events in Zanzibar, but the details of this section no longer require detailed criticism. It is unfortunate for the author that a far more critical account of events leading to Zanzibar's independence and the 1964 revolution appeared in 1965 when Michael Lofchie's Zanzibar: Background to Revolution was published by the Princeton University Press. Lofchie, utilizing a broad range of written and oral sources, provided a very critical and highly readable account of the intricate political events that culminated in 1964. The publication of Lofchie's book made the appearance of Ayany's volume of little value, especially since Ayany apparently decided to let his materials for the years up to 1963—the date of his thesis-stand without any significant inclusion of Lofchie's analysis. It is only in recounting the events since 1963 that Lofchie's materials are given any real use, and even in this section they are not handled with any critical sophistication. Thus readers interested in the recent past of Zanzibar and Pemba are best advised to forget about Ayany and instead turn to the volume and several articles of Loschie.

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PETER M. GUKIINA. Uganda: A Case Study in African Political Development. Notre Dame, Ind.: University of Notre Dame Press. 1972. Pp. xii, 190. Cloth \$7.95, paper \$3.25.

A historian's reaction to Mr. Gukiina's study of political evolution in Uganda from precolonial times to the downfall of the Obote government in 1971 can only be mixed. On the one hand Gukiina has produced an adequate and readable survey of the most significant political events that marked Uganda's transition from colonial status through the first decade of independence. On the other hand the work is marred by some important factual errors and by some serious historical misinterpretations.

In addition to errors already noted in an earlier review by Dean E. McHenry, Jr. (African Studies Review, 16 [1973]: 146-48), Gukiina provides a good bit of additional misinformation. Concerning the peoples of Karamoja (the Ugandans whom I know best), for example, he states that they are "Hamites" (a linguistic classification that never existed), that they do not farm (in fact, their economy is a balanced one in which agriculture and pastoralism are of about equal importance), and that their age system includes circumcision (a ritual that in fact they abhor).

Of even greater concern to the historian is Gukiina's thesis concerning the exclusiveness and the implied sociopolitical stagnation of precolonial Uganda societies (see, for example, pp. 14, 39, and 65–66). In developing this thesis he completely ignores the very clear indications, meticulously gathered by oral and archival historians over the past two decades, that some of the key features of traditional East African societies were their dynamic political, social, and economic evolutions, based in large part on close interaction between neighboring peoples.

Moreover, while in some place Gukiina, himself a Ugandan, with full justification attacks and tears down some of the stereotypes and misconceptions built up about Uganda by non-African observers, he nevertheless in other places commits precisely the same type of errors. Thus, he stereotypes the Karimojong as taciturn and silent (p. 28), in effect perpetuates the myth of "ancient tribal rivalries" (pp. 55 and 93), and brands the Uganda peasants of the late 1960s

Africa 453

as not "capable of understanding the economic world" (the italics are mine).

His treatment of the Amin coup of 1971 seems at best naive and misinformed. For instance, as a resident of northern Uganda at the time of the coup, I can assure Mr. Gukiina that the "instantaneous public jubilation" he claims was "nationwide" with the military takeover was most certainly not found throughout the north

Perhaps some of the most successful parts of the book were those in which the author drew on his personal experiences. If he had drawn more on such personal information (and augmented it perhaps with oral evidence gathered from acquaintances and kinsmen), and somewhat less on certain of the published sources upon which he so heavily relied, the book might have been much more successful.

Nevertheless, the book as it stands could be used judiciously in secondary and undergraduate classrooms, as it provides a rather different and in some ways admirable approach to Uganda political development.

J. E. LAMPHEAR

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ROBERT C. GOOD. U.D.I.: The International Politics of the Rhodesian Rebellion. [Princeton:] Princeton University Press. 1973. Pp. 368. \$12.50.

Robert C. Good, an American scholar, served as United States ambassador to Zambia during the Rhodesian crisis. His study reflects his official experience. It is also a book with a purpose. The writer asserts that Americans cannot, and indeed ought not, stand aloof from the Rhodesian tragedy. He argues that "it is inconceivable that two great racial revolutions (in America and South Africa) can climax at roughly the same point in history without finally each affecting, exciting, and probably aggravating the other." In order to reach this conclusion most directly the author, in his own words, has "reduced to a minimum the conceptual baggage usually associated with scholarly ventures." He has provided a "scholarly memoir or analytical journalism" to provide guidance for his countrymen on a critical issue.

Good's version of modern Rhodesian history fits into a wider framework of ideas. His study closely follows the interpretation of contemporary events in Central Africa put forward by British prestige journals such the Guardian or the Economist. These organs have always disliked the Rhodesian Front and its supporters, not merely for their politics but also for social

reasons. The Rhodesian Front was created, in the author's words, by an "amalgamation of conservatives, dissidents and eccentrics." It represents a coalition of white workers, managers, farmers, and the like, the kind of people whom many British academics and journalists dislike on cultural and esthetic, as well as political, grounds. When the Rhodesian Front issued its Unilateral Declaration of Independence (UDI), the British prestige press, and also many academicians concerned with the area, assumed that UDI would be a nine-day wonder. The Front repudiated the older white upper-class establishment in Rhodesia, together with its British connections. The new rulers rejected the timehonored symbolism of Union Tack and royalty. They ran up a flag of their own, incorporating the Rhodesian rugby colors instead of the accustomed red, white, and blue. They established a dissident republic, fully convinced that success would be theirs. To British critics, however, it seemed inconceivable that 3mith's "Cowboy Cabinet," backed supposedly by hardhats and hayseeds, should successfully defy-even if only for a short space of time—both the inevitable tide of history and the good opinion of their betters.

Good dislikes the Rhodesian Front for reasons similar to those advanced by the British prestige press. His book also follows what might be called the abolitionist tradition in American historiography. As he sees it. the Rhodesian imbroglio has some aspects of a morality play. It starts as a "madcap adventure, yet with frightening overtones," which are provoked by the overweening ambitions of a small European minority. This community is reactionary to the core, devoid of historical perspective, and even of common sense. White Rhodesians, for the time being, manage to cling to a luxurious existence, supposedly characterized by easy living, a high level of conspicuous consumption, and an unnaturally low cost of living. The future of this "three-servant, two-car, one-swimming pool society" depends on the availability of "cheap labor and liquid assets within the country." But, Good argues, the day of reckoning will come. Given time, black revolutionaries will crush the white oppressors.

The African rebels' task will at least be made easier by a policy of continuing international economic sanctions against Rhodesia. Even at their present level of relative ineffectiveness "the policy of making Rhodesia an international pariah curtails the flow of white immigration into the country, encourages the drain of white young people from the country, and denies

Rhodesia access to major money markets, thus thwarting economic growth, hastening the rundown of the infrastructure, while increasing the cost to South Africa of sustaining its awkward and embarrassing northern neighbour."

This analysis essentially conforms also to the United States Department of State thinking, expressed in such pamphlets as Southern Rhodesia: The Question of Economic Sanctions (Department of State Publication 8744, December, 1973). It is effectively presented, and the writer is experienced in setting out alternative options. But Good's account suffers from a distinct weakness: like so many other investigators, he does not make a sufficiently dispassionate analysis of the Rhodesian power structure and its dynamics. Despite what Good and many others would like to see, too much remains to be explained. The writer thus fails to show why a small white community, so insignificant in numbers and so contemptible in attainments, should have been able to defy the prophets for so long and cling to power against such widespread opposition. The Rhodesian Front, in fact, differs considerably from the stereotype presented by Good. It has now run Rhodesia for about twelve years. "White Rhodesia" remains beset by serious internal weaknesses. But Good does not really bring these out, any more than its strengths. Despite the forecasts of so many experts, the Front, according to its own terms of reference, has after all been surprisingly successful up to the present. The Rhodesian economy first recovered from the depression associated with the breakup of the Federation of Rhodesia and Nyasaland. From then onward, the economy has continued to grow, although at a much slower pace than during the halcyon days of federation. Rhodesia's rate of inflation has been kept to a minimum. Rhodesian factories have both expanded and diversified their production. Despite the loss of many foreign markets for Rhodesian tobacco, its former staple crop, the white farming industry has not collapsed. Rhodesian farmers instead have managed to readjust to a changing situation and considerably to increase their food production. Social services have continued to operate. The incumbent regime, for the time being, has maintained effective political and military control over the territory. Up to the present, guerrilla incursions have as yet failed in their declared objective of disrupting the state machinery, breaking white morale, and bringing about an economic collapse. Salisbury remains physically safer than an American city of similar size. And, despite almost apocalyptic prophecies of an impending bloodbath

south of the Zambezi, the amount of bloodshed in Rhodesia has, up to now, been much less than the civil violence experienced in countries such as Nigeria, the Sudan, or Burundi, all of which, according to the experts of the late colonial era, had embarked upon independence under much more favorable auspices than Rhodesia's.

Economists, political scientists, and historians alike will of course differ widely in their respective analysis of the Rhodesian phenomenon. But whatever their political or moral assumptions may be, they will have to improve on Good's analysis by taking "white Rhodesia" seriously as a legitimate subject of academic concern.

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RICHARD GIBSON. African Liberation Movements: Contemporary Struggles against White Minority Rule. New York: Oxford University Press, for the Institute of Race Relations, London. 1972. Pp. x, 350. \$8.50.

Like other contemporary analyses of Southern Africa, Richard Gibson's wide-ranging study of the African revolutionary groups that have been fighting against colonial and white minority regimes has been overtaken by events. Guinea-Bissau is already independent under PAIGC control, and FRELIMO will form the government when Mozambique becomes independent next June. Events in huge and potentially wealthy Angola are less easy to prophesy since the three separate liberation movements— FNLA, MPLA, and UNITA—are still hostile to each other, but the Portuguese remain determined to provide that country also with independence. Thus the strategic configuration existing at the time Gibson's book was published has changed radically. There are now new factors and forces impinging on Rhodesia, Namibia, and the powerhouse of white domination, South Africa.

Gibson's book remains a handy reference for the liberation groups that have achieved political power, are about to, or are still seeking to do so. The author, a black American journalist for Negro Press International, has had long experience in Africa, and he does not hesitate to point out reasons for earlier failures as well as achievements. He is clearly biased, however, in favor of those groups favored by the Chinese Communists. He also underplays the influence of the Sino-Soviet split in intensifying the rivalries between the liberation groups from every Africa 455

Southern African country, except Mozambique, that vie with each other for both internal and external support.

While at the time that Gibson prepared his book it was understandable that the struggles of liberation movements were the focus of attention, present circumstances indicate that liberation for Southern Africa must be evaluated in new terms. For some groups the immediate objective remains to unseat existing minority regimes. Increasingly, however, the primary need is to build popular support among settled African or multiracial populations for the African leaders and movements that are assuming, or are about to assume, control. Associated with this effort is the need to build viable economies. Independence for Mozambique, for example, is only the first step toward freeing that country from the pervasive economic dependence on South Africa that affects all the territories of Southern Africa except Zambia and Angola. Henceforth, the more subtle features of Southern African relations will'increasingly determine the character of that area.

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ERIC AXELSON. Portuguese in South-East Africa, 1488–1600. [Cape Town:] C. Struik, for the Ernest Oppenheimer Institute of Portuguese Studies of the University of the Witwatersrand, Johannesburg. 1973. Pp. xii, 276. \$11.60.

The King George V Professor of History at the University of Cape Town began his detailed archival research in Europe in 1935 and is thoroughly familiar with the archives and libraries of Continental Portugal and the archives of Portuguese India. Among his carefully documented studies are South-East Africa 1488-1530 (1940), Portuguese in South-East Africa 1600-1700 (1960), Portugal and the Scramble for Africa 1875-1891 (1967), and Congo to Cape: Early Portuguese Explorers (1973). The book under review represents a rewriting, updating, and extension of the 1940 volume in the light of more recent researches conducted under the auspices of the Ernest Oppenheimer Institute of Portuguese Studies.

This study, which includes excellent maps, narrates in minute detail the story of Portuguese activity from the Cape of Good Hope to Mozambique Island and beyond, including Kilwa, Mombasa, Malindi, and even Cape Guardafui. It begins with Bartolomeu Dias's arrival in southeast Africa and ends with the

establishment of the English East India Company and the concomitant influx of foreign shipping to the Indian Ocean. It concentrates on Sofala and Mozambique Island. Two chapters treat southeast Africa in its relationship to the sea route to India, for, as the author is frank to admit (p. 198), the Portuguese added to Europe's knowledge of southeast Africa incidentally to increasing their knowledge of the route to India, or, as he puts it, "the importance of south-east Africa to the Portuguese lay not so much in trade as in the mere fact that it flanked a vital and often critical sector of the sea-route to India" (p. 105). Of particular interest to students of literary history are the documented analyses of the famous shipwrecks that figure in the História Trágico-Maritima: of São João (1552), São Bento (1554), Santiago (1585), São Tomé (1589), and Santo Alberto (1593).

The Portuguese genre known as the relação de naufrágio presented these and other wrecks with moralizing overtones. Not so Professor Axelson, whose book is straightforward and factual. Reading it in a year of crisis for the Portuguese colonies in Africa, one begins by looking for "relevant" comments, philosophical and political asides that might explain events of the 1960s and 1970s. Suddenly one realizes that no such editorializing is needed, for the verdade nua e pura explains everything. The author adopts none of the rhetoric of Portuguese historiography, no glorification, no praise, just the plain unvarnished truth. Thus he bluntly points out (p. 131) that certain regulations "confirm that Portugal held Sofala for one purpose and one purpose only: for trade and material profit" and in a footnote at that point contrasts his own affirmation with brief discussion of the distinguished Portuguese historian Alexandre Lobato on whose researches he draws very heavily: "Dr Lobato, however, quotes the regimento as proof of the constancy through the centuries of Portuguese moral principles, based on humanity and justice . . . and he concludes . . . that the fundamental economic purpose did not militate against establishing a policy for the indigenous inhabitants that was truly humane and Christian, based on social justice, with the final aim of integrating those negroes who accepted religious conversion into Portuguese citizens."

Professor Axelson writes most competently of winds, currents, position finding, and appropriate sailing seasons. He would have favored landlubber readers had he outlined an important aspect of the monsoons between the west coast of India and southeast Africa. In those days there were no protected ports along the Indian coast, only open roadsteads. During the period of the southwest monsoon, which blows from Africa to India between May and October, sailing vessels may cross to India but once there they are unable to discharge passengers or cargo, as I can attest from visits to Cannanore, Calicut, Cranganore, Cochin, and Quilon in the months of June, July, and August.

It was in order to be able to land in India that the Portuguese departed Africa at the tail end of the otherwise favorable southwest monsoon, that is, in September or early October. To use an example given by Axelson (p. 202), Diogo Botelho Pereira departed Lisbon March 23, 1550, arrived at Mozambique Island August 18, and reached India October 20. He departed Cochin January 18, 1551, rounded the Cape of Good Hope April 7, and reached the Azores July 14.

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EDWARD C. TABLER. Pioneers of South West Africa and Ngamiland, 1738–1880. (South African Biographical and Historical Studies, 19.) Cape Town: A. A. Balkema. 1973. Pp. ix, 142. \$10.25.

As in his *Pioneers of Rhodesia* (Cape Town, 1966), Edward C. Tabler has attempted in this book to provide a comprehensive biographical dictionary of mainly Europeans who served as explorers, traders, hunters, and missionaries in the regions north of the republic of South Africa. Concentrating upon what is today Namibia and western Botswana, Tabler places only one significant limitation upon his study—he does not describe, even in a collective form, the African migrations into the region before 1738, and he stops his work in the 1880s and therefore does not include the Germans and others who were to claim and rule much of the area.

The lack of any exclusive standard of selectivity for entries in the volume means the devotion of space to short and inconsequential information on many insignificant persons in his "A to Z" presentation. These listings detract from his more substantial commentary and fresh information on persons who played an active role in the region's history. Perhaps, had the author grouped his entries by occupation or interests, he would have been able to provide an introductory text, develop more fully his treatment of the important but lesser-known persons, and relegate the insignificant to an anno-

tated list at the end of each section. The volume would have benefited, too, from the inclusion of at least some detailed maps or charts of the region in the nineteenth century that cannot be easily found elsewhere.

However, as with his work on Rhodesia, Tabler's references to both primary and secondary sources containing additional information on persons included in his study and his cross-references and glossary give the volume importance as a valuable if limited tool for the researcher.

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PETER WALSHE. The Rise of African Nationalism in South Africa: The African National Congress 1912-1952. Berkeley and Los Angeles: University of California Press. 1971. Pp. xvi, 480. \$13.50.

MONICA WILSON and LEONARD THOMPSON, editors. The Oxford History of South Africa. Volume 2, South Africa, 1870–1966. New York: Oxford University Press. 1971. Pp. ix, 584. \$8.00.

In the past decade historians of South Africa have, however belatedly, begun to emphasize the role of Africans in South African history. This movement is still in a transitional phase. These two early examples were undertaken in the mid-1960s and are characteristic of liberal South African interests and attitudes at that time.

Walshe's appropriately subtitled book presents an analytical and descriptive study of the development of the African National Congress (ANC). It is divided into three chronological sections: from the beginnings of the ANC to 1924, from 1924 to 1939, and from 1939 to 1952. Within these time periods there are topical chapters that deal with the political activities of the ANC, with ideological and socioeconomic influences on its development, and perhaps most original and most valuable—with its organization and administration. Walshe takes the conventional mid-1960s position that African nationalism in South Africa was primarily a response to such Western influences as Christianity, European education, economic interdependence, and black American religious and political movements—and that these influences explain in large part its early moderate and nonracialist stance. He restores the significance of A. B. Xuma (president, 1940-49) to the rebuilding of the ANC after 1936. Throughout the author examines the basic ANC dilemma: whether to work for practical but minor gains within a system that was in principle unacceptable to it, or whether to stand or fall on the issue of the principle itself. The entire work is buttressed by an extensive bibliography, particularly enterprising in its use of ephemeral political documents and manuscript sources.

The Oxford History of South Africa, volume 2, like its predecessor (1969), is an interdisciplinary history deliberately emphasizing "interaction" between different races and groups in South Africa. Its ten chapters are written by eight liberal, white authors. Four survey chapters at the beginning are devoted to separate themes for the entire period, 1870-1966: the South African economy (by the economist, D. Hobart Houghton), African "peasant" society (by the anthropologist, Monica Wilson), agriculture (by the economist, Francis Wilson), and urban development (by the political scientist, David Welsh). The last three of these constitute the most original chapters of the book, both in conception and in execution—solid attempts at interdisciplinary history. Following these chapters are three by Leonard Thompson (the sole historian) on the acquisition after 1870 of the remaining unoccupied African territories, the British subjugation of the Afrikaners by 1900, and the formation of the Union. The final three chapters deal with the post-1910 period in a selective way: the development of Afrikaner nationalism (by the former journalist, René de Villiers); the development of African nationalism (by the sociologist, Leo Kuper); and South Africa's international relations (by the political scientist, Jack Spence).

Although they contain enormous amounts of detail, both books suffer from a topical structure that tends to chop up chronological continuity, scatter the discussion of some events through several chapters, and omit significant material without a compensatory analytical coherence. Both books rely almost entirely on written sources, although Walshe in particular has an ideal subject for the extensive use of oral evidence. Both books are, as well, totally committed to the liberal, integrationist view that political separation is ultimately incompatible with economic interdependence. This commitment creates difficulties for the Oxford History. It conflicts with a contrary thematic emphasis on African and Afrikaner nationalism: the result is that while there is little material on moderate, English-speaking South Africans, African and Afrikaner movements that do not express an explicitly integrationist ethic (such as the Pan-Africanist Congress) are didactically criticized. At the same time the authors do not directly confront the more radical contention that segregated, migratory labor may not only be compatible with economic interdependence but may in fact be a necessary component for a particular kind of capitalist machine. Nonetheless, despite their perhaps inevitable limitations, both of these works are significant historical landmarks. Despite the liberal orthodoxy of their positions, the interpretive debate they are part of is a complex, and in some respects an old one that is by no means over. And the new subject matter they consider, the new sources they have uncovered, and the new materials they present are indispensable to anyone concerned with modern South African history.

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## ASIA AND THE EAST

KENNETH K. S. CH'EN. The Chinese Transformation of Buddhism. Princeton: Princeton University Press. 1973. Pp. ix, 345. Cloth \$15.00, paper \$9.50.

While Europe was being conquered by Christianity and, repudiating the worldly materialism of the Roman Empire, was entering the great Age of Faith that we know as the Middle Ages, China was undergoing a strikingly parallel religious experience. The internal collapse of China was not as complete as that of Rome nor was the success of the foreign religion in China as sweeping as that of Christianity in Europe. Nevertheless the similarities and differences between the two phenomena, occurring simultaneously at opposite ends of Eurasia, offer a highly interesting field for comparative historical study.

Kenneth Ch'en's book provides a good introduction to the Chinese side of the question. After a short chapter that sets forth in more detail the problem already announced in the title—how the foreign religion was adapted to and eventually absorbed into the already fully formed civilization of China—come five further chapters dealing with the Buddhist impact in the fields of ethics, politics, economics, literature, and, finally, education and social life.

It was on the ethical plane that Buddhism offered the greatest challenge to traditional Chinese ways and met the fiercest resistance. The claim of the Buddhist monk to have abandoned the world and to be no longer bound by the ties of family relationship or the duties

of a subject to his ruler was deeply shocking to the Confucian world view, with its emphasis on filial piety and the cosmic role of the emperor as Son of Heaven. The early Buddhists in China found themselves in a running battle to justify their pretensions and were constrained to emphasize points where Buddhist ethics were closest to Chinese traditions. At certain moments pious emperors might concede almost all of the Buddhists' claims but, as Ch'en shows, in the long run the power of the Chinese state reasserted itself and the Buddhist church, instead of existing, as it claimed, as a separate body outside secular society, became a closely controlled and regulated institution within it.

The political role of Buddhism was comparatively small since Confucianism remained at all times the basic ideology of the Chinese state and Confucian education provided the chief road to public office. On the other hand the Buddhist church had profound economic effects at all levels of Chinese society. Buddhist monasteries became large and privileged land owners, they managed industrial enterprises such as water mills and oil presses, they engaged in moneylending and pawnbroking, thus contributing to the development of banking and credit institutions.

To illustrate the deep influence of Buddhism on literature Dr. Ch'en takes the poet Po Chü-i, already fairly well known to the English reading public through the work of Arthur Waley, as a representative figure. Po's lay Buddhism, which he combined, as a government official, with an allegiance to Confucianism and with a strong personal interest in Taoist practices, provides an excellent example of the tolerant, eclectic spirit that was typical among Chinese intellectuals.

Finally, in the last chapter, we get some glimpses of how Buddhism penetrated into the life of ordinary people through its festivals, its public preaching, its charitable activities, and in many other ways.

There are particular points on which one could question the author's interpretation and places at which one could wish for a sharper analysis, but, on the whole, this is a well-written and scholarly treatment of ar important subject that one hopes will be read by general historians as well as specialists on China.

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ARTHUR F. WRIGHT and DENIS TWITCHETT, editors. Perspectives on the T'ang. New Haven: Yale

University Press. 1973. Pp. viii, 458, 1 map. \$15.00.

This impressive volume contains eleven papers presented at the first research conference on T'ang studies ever held in the West. The authors are specialists in this celebrated period (618-906) of Chinese history: Ikeda On of Japan writes on "T'ang Household Registers and Related Documents" and Wang Gung-wu of Malaysia and China (teaching in Australia) on "The Middle Yangtse in T'ang Politics." The other contributors are from England or America: Denis Twitchett writes on "The Composition of the T'ang Ruling Class: New Evidence from Tunhuang," Howard J. Wechsler on "Factionalism in Early T'ang Government," Charles A. Peterson on "The Restoration Completed: Emperor Hsien-tsung and the Provinces," Arthur F. Wright on "T'ang T'ai-tsung and Buddhism," Stanley Weinstein on "Imperial Patronage in the Formation of T'ang Buddhism," David McMullen on "Historical and Literary Theory in the Mid-Eighth Century," Hans H. Frankel on "The Contemplation of the Past in T'ang Poetry," Elling O. Eide "On Li Po," and David Lattimore on "Allusion and T'ang Poetry." As might be expected, these studies illuminate this glorious era of imperial China.

The lengthy introduction (pp. 1-43), prepared by the two veteran editors, Professors Wright and Twitchett, is scholarly, balanced, lucid, and incisive. Their sections on the political and cultural background, the perspective of thought and religion, and the survey of institutions are outstanding and should be read by all students of T'ang history. The reign-by-reign survey in the first section (including also the preceding Sui dynasty, 581-618) may appear old-fashioned. Such treatment is nevertheless justifiable and proves helpful for an understanding of the papers. After all, a weak or strong emperor makes a considerable difference. Even powerful Buddhism relied on imperial patronage and suffered badly from persecution in the mid-ninth century.

The introductory section on poetry is less satisfactory. Interaction between poetry and society is discussed only inadequately. No mention is made of the relationship between poetry and the examination system. Traditional Chinese scholars divide T'ang poetry into four periods instead of five as given on page 41. The four periods are Early T'ang (618-712), Flourishing T'ang (713-65), Mid-T'ang (766-

835), and Late T'ang (836-906). This scheme, although criticized by Hu Shih and Lu K'angju, who consider the An Lu-shan uprising in the mid-eighth century as a landmark, is still followed by many modern specialists, including Liu Ta-chieh, Liu Wu-chi, and Yu Kuo-en. The same scheme is also used by art historians in China and Japan in their discussions of T'ang ceramics and T'ang painting. As for poetry as historical material, the poet Li Po wrote that he would pay a thousand pieces of gold for his favorite drink. He probably would protest, however, the injustice done to him by the editors' expression "for a cup of rare wine" (p. 39), unless the cup were constantly refilled. Economic historians have found more realistic prices of wine elsewhere in T'ang

The papers in general are distinguished by substantial documentation and sophisticated interpretation, although some unfortunate slips in philology may be noted. In the translation of a poem on page 349, allusions to two eminent Chinese of the Han period have been overlooked. As a result the official title Kuang-lu or Kuang-lu Hsün becomes "splendid," and a particular general, "generals." The name of the Nan-T'ang ruler was Li Pien, not Li Sheng as given on page 231. This name has acquired new significance because the royal tombs of Li Pien and his successor Li Ching were excavated in 1950-51 and the remarkable findings from the site published in 1957.

Altogether, this valuable volume marks a long stride made in T'ang studies in the West. It is gratifying to note that younger American scholars are making their share of the contribution, as illustrated by the excellent papers by Eide, Peterson, and Weinstein. Students in the field will appreciate not only the papers and the introduction but also the bibliographical notes, maps, and glossary-index.

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JOSEPH NEEDHAM, with the collaboration of WANG LING and LU GWEI-DJEN. Science and Civilisation in China. Volume 4, Physics and Physical Technology. Part 3, Civil Engineering and Nautics. New York: Cambridge University Press. 1971. Pp. Ivii, 931. \$55.00.

This book by Professor Joseph Needham is surely an outstanding work, one that will bring the author an eminent reputation in the field of the history of sciences. In it the author gives much important information and discusses civil engineering and nautical technology.

The first part of this volume is a description of the roads, walls, buildings, bridges, and canals, together with the systems of irrigation, hydraulic engineering, and water conservation. The second part deals with sailing craft, the technical development of the ship both in time of peace and war, and some information on sea routes of ancient Chinese sailors. The author not only depicts Chinese techniques in the greatest detail ever presented in the Western world, but he also gives more exact identification of Chinese civil engineering with English technical terms, especially for Chinese buildings constructed under systems extremely different from those of the West. The viewpoint of this book goes a long way toward helping Westerners, as well as Chinese students with modern training, to better understand ancient Chinese architecture.

Among the valuable information in this book are several points that impressed me. Since some problems might be solved with further discussion, they are pointed out separately. The design of a house, with its many steps of development, should be traced to the single tent. The reconstruction of the ruin of Hsiao-tun before the tenth century B.C., discussed by Mr. Shih Chang-ju, should be considered as the basic pattern of the Chinese house, which is related to both the Book of Rites (I-li) and the later development, from the design of the imperial palaces (and Buddhist or Taoist temples) to the typical ground plans of the house of the common people. The steps of the development of the ground plan are: first, a hall with two main front pillars and a left wing; second, a hall with left and right wings; third, for a palace, the wings developed independently into three halls in one line (compare Liu Tun-chen, Tung-hsi-tang K'ao), and for a common house, both of the wings led into the side rooms (socalled Hsiang-fang); fourth, opposite the hall there might be the main gate with two side rooms, but for a common house it becomes a row of three rooms, and the main gate moves to the right side by the row. This is the typical pattern of Ssu-ho Yuan or the compound.

Needham is very enthusiastic in tracing the tradition of the Chinese house system to the classics of rites. It is a rather hard task, however, because very few scientific studies could be found in this field. San-li-tu by Nieh Chung-I of the Sung dynasty is simply a collection of constructions. During the Ch'ing dynasty all of the scholars without exception followed

Nieh's assumption, including Jen Chi-yun, whose work is mentioned by the author.

The only article with an entirely different method is Kan Lao's "Li-ching Chih-tu Yu Hantai Kung-shih" ("The System in Classics of Rites and the Relation to the Buildings of the Han Period," in the special issue of Kuo-hsueh Chi K'an, Peking University, 1939), which is based on the actual relics and which has a ground plan that is totally different from the traditional view of N'ieh Chung-I. It is unfortunate that it was not seen by Needham because of the limits on its circulation during the Sino-Japanese War.

Another point that impressed me, which was noted by Needham, is that a town or a city, other than a Chinese village growing from unofficial administration, was established for political purposes. In Chinese history there was no distinction between a castle and a town because the towns or cities were not creations of burghers and never achieved any degree of autonomy with regard to the state. Furthermore, the author adopted a typical city plan by E. A. Gutkind, which is a square inside the city wall with crossed streets and a drum tower at the center. All of these are true, but I would like to suggest two points for discussion. Since the development of the Chinese city is very complex, the best way to manage the study is by tracing and analyzing its history.

The historical development before unification, which came during the Ch'in Empire, was a sequence of progress from tribal administration, city-states, and kingdoms to empire. In ancient China the Chou dynasty was not truly feudal, and in the later Chin dynasty (300-420 A.D.), feudalism was incomplete. In the former there were no burghers, and in the latter there were castles (so-called wu or pao) with burghers, but they were all wiped out by imperial power very soon. In Han Kuan I the author Ying Shao of the Later Han period indicated that the assistants of a magistrate might be compared with ministers of a ruler in the ancient state. It means that the hsien (district) was transferred from the city-state. Of course the city of a hsien was adopted from the mode of a citystate. Ya-men was certainly derived from the palace of a former lord. According to Chou-li the palace was located at the southern part of the capital. It may be compared with the Yamen, which was always situated at the south of a city. The drum tower could only be traced back to the main beacon tower of the Han dynasty. (According to Han wooden slips, the drum was used in the beacon tower.)

Note A on page 73 is based on the late E. A. Gutkind's work in which he considered that Chinese cities were square or rectangular rather than circular, round, or irregular because in former times Chinese cities were designed with the belief of the ancient "squared earth" theory. I do not think it is a uniform regulation in China. Evidently it is found that the cities to the north of the Tsinling Mountains and the Huai River are always square or rectangular, while to the south of this line the cities are always round or irregular. In my previous article, "The Cities of Southern Style and Northern Style," I mentioned that northern cities were built planned, but that southern cities were transformed from villages which arose naturally. I think it should be an important supplementary idea to this book.

Needham indicates that a curving roof is a very characteristic and beautiful feature of Chinese buildings. He gives more important information from Ying-tsao Fa-shih and research and pictures from Ying-tsao Hsueh-sheh. He wonders, however, about the reasons why ancient Chinese buildings were always made of wood instead of stone, which was used in other countries like Greece, India, and Egypt and which allowed many monuments to endure. It cannot be surmised that China had no stone suitable for great buildings similar to those of Europe and Western Asia; in China stone was used only for tomb-construction styles and monuments and for pavements of roads, courts, and paths. Needham points out that perhaps further knowledge of social and economic conditions might illumine the matter of the form of slavery, known in China in different ages but never equivalent to slavery in the Occident, which could dispatch thousands at a time to hard labor in the quarries.

In my opinion this explanation is good, but there are some other elements besides it. On page 262 of this book (in figure 876) Needham explained the illustration showing that the Chinese always had men of genius for the efficient organization of mass manpower. On careful study of Chinese civilization, the dolesmen are found in many parts of China. In the records of oracle bones it was shown that several ancestors of the Shang royal family, such as Shang-chia, Pao-I, Pao-ping, and Pao-ting, were worshiped in the stony shrine. But in later ages the ancestor temples were built of wood. Later a very famous story was recorded about the ancestor temple of Wang Mang that was built from lumber taken from the luxurious palace of Han Wu-ti. In the Book of Poetry we find a description of the new palace built in the beginning of the Chou dynasty—"It is as beautiful as a pheasant flying." Thus the invention of the wood curving roof as the trend in the development of Chinese building is due to its magnificent beauty. Since beautiful wooden palaces and temples could be well protected within the longevous power of the rulers, they neglected to consider the importance of monuments.

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MICHEL CARTIER. Une réforme locale en Chine au XVI° siècle: Hai Rui à Chun'an, 1558–1562. (École Pratique des Hautes Études—Sorbonne. VI° Section: Sciences économiques et sociales. Le monde d'outremer passé et présent. First Series, Études, 39.) Paris: Mouton & Co. 1973. Pp. 169. 32 fr.

The title of Michel Cartier's work is somewhat misleading because the central section is concerned only indirectly either with the midsixteenth-century taxation reforms or with Hai Jui, a reform-minded official. The reforms and Hai Jui's background and role as a reformer in Ch'un-an, a county in western Chekiang, are treated relatively briefly in the introduction (pp. 11-17) and in chapter 4 (pp. 85-97). Also, a translation of selected portions of a work by Hai Jui, the Hsing-ke t'iao-li, is included as an appendix (pp. 103-50). As has been stated, however, Cartier's work does not really focus on Hai Jui and the mid-sixteenth-century reforms. The main body of the book is essentially a study of the administrative and fiscal systems of midsixteenth-century Ch'un-an County, preceded by an introduction that considers the county's geography, demography, and recent socioeconomic changes.

Cartier's work is one of the best of a modest, but growing, number dealing with the institutions and socioeconomic situation of mid-sixteenth-century China and with the "single-whip" taxation method, a reform measure, which gradually and in varying degrees replaced the older taxation systems in many areas from the Yangtze Valley to Kwangtung. The "single-whip" tax reforms had as main objectives the combining of the diverse land, labor service, and miscellaneous taxes into a few tax packages and the commutation of most of the tax packages from payments in kind and in labor into payments in silver. The reforms were prompted by various changes that were occur-

ring in the socioeconomic sphere: the growth of a money economy, the expansion of handicrafts, the appearance of abuses under the old tax systems, which had not been kept up to date after their enactment at the beginning of the Ming dynasty, and the need for more revenue and greater efficiency in the face of growing demands.

Cartier's work is solidly based on an impressive array of Chinese primary materials and of secondary sources in Western languages, Chinese, and, particularly, Japanese. Obviously, however, in a work of a mere 150 pages, Cartier can treat many aspects of his highly complex subject in only a preliminary fashion. Thus, it is to be hoped that he will supplement his successful initial work with a fuller treatment of the institutional and socioeconomic changes of the mid-sixteenth century. Such a fuller treatment will prepare us to answer more adequately what is undoubtedly the most fundamental question about the sixteenth-century changes and reforms: were they truly significant developments or merely minor alterations in tradition? On the basis of the information available at present, including this work, the conservative and restricted nature of the changes and reforms, plus the fact that they were supported by such a Confucian purist and moralizer as Hai Jui, lead one to believe them to have been minor alterations. But a definitive answer still eludes us.

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EDWARD V. GULICK. Peter Parker and the Opening of China. (Harvard Studies in American-East Asian Relations, 3.) Cambridge, Mass.: Harvard University Press. 1973. Pp. xi, 282. \$12.00.

Students of the missionary movement in China and of early Sino-American relations will find this first scholarly and full-length biography of Dr. Peter Parker rewarding reading. Parker's early work in Canton prior to and immediately after the Opium War constituted the most significant part of his career. Thousands of Chinese suffering from illness, particularly diseases of the eye, benefited from treatment in Parker's hospital. The success he enjoyed, given his limited training in medicine and the state of medical knowledge at the time was remarkable. The most refreshing aspect of this account is the author's careful attention to the kinds of cases Parker encountered and the

methods he employed, plus the description and analysis of Chinese medical practice at the time. There is recognition of the fact that Chinese practice, in spite of absence of scientific studies, could lay claim to significant achievements.

The author's admiration for Parker's work as a medical missionary is summed up in the list of Parker's achievements: "the founding of a key hospital, treatment of 50,000 patients, performance of the most spectacular surgery of the day, significant cultural transmission . . . , the inspiration of a very remarkable pictorial documentation of gross pathology, the establishment in 1838 of a bi-national institution—the Medical Missionary Society—to publicize a new profession and to assist in securing new recruits for it."

Gulick does not exaggerate Parker's abilities as a diplomat. Parker's most useful contribution was in assisting Caleb Cushing in the negotiation of the first treaty. Thereafter Parker served with only moderate success as chargé, secretary of legation, and finally as commissioner. In the latter position he sought to negotiate a new treaty but failed, not because of his own shortcomings, but because the Manchu-Chinese at the time were determined to negate proposed treaty revision. In the course of his efforts Parker fell victim to his own imperialistic orientation and foolishly advocated the occupation of Formosa as a means of forcing China to yield.

Professor Gulick, himself a former missionaryeducator in Nanking, writes with understanding about the China of the 1840s and 1850s. He likewise refrains from stereotyped indictments of both missionaries and the Western governments' imperialism, but he does not hesitate to call attention to highhanded measures born of ignorance and a revolting sense of superiority. Parker does not emerge as a hero. He was arrogant, stubborn, far from tactful in personal relations, and committed to a truculent and narrow orthodoxy, but he was also courageous, an indefatigable worker, and skilled in the art of medicine.

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RICHARD C. THORNTON. China, the Struggle for Power 1917-1972. Bloomington: Indiana University Press. 1973. Pp. x, 403. \$12.50.

Western scholars have called the Cultural Revolution of 1966-69 everything from a demonstration of Mao's unique approach to modernization to a power struggle. As his title

suggests, to Thornton it was the latter, along with all other passages in Mao's tumultuous public life. As for the "correctness" or aptness of Mao's policies, Thornton says little. To hagiographers Mao is the Revolution; here he is the political infighter who has outlasted all other "comrades in arms." The revolution—certainly the one with the reifying capital R—is pretty well crowded out in this teeming and salient work.

Thornton holds that Mao's originality as a Marxist-Leninist has been oversold. Mao, he writes, was usually a faithful follower of the Comintern, whose line was usually correct for China, being skewed mainly by the Chinese Communists in their intramural scrapping. Russian influence has been critical, early and late, being the most important of external vectors, including those of Japan and the United States, which have had "tremendous impact." The Cultural Revolution was not Mao's reasoned and autonomous confrontation with modernization; rather it was thrust on him by Khrushchev's ouster and the change in Vietnam.

Another revisionist interpretation for some will be Thornton's rather sympathetic coverage of Chiang Kai-shek, who was, it seems, kept from winning Manchuria largely by American rigidities and evasions. To Thornton the problem was military, there being only passing reference to the deterioration of the Kuomintang. Here, as elsewhere, the author does not erect the larger historical setting.

Nothing prevents Thornton's meticulously assembled data from being fitted into some wider frame: Mao may indeed have been animated by thoughts higher than political survival, but in the absence of any device for orderly succession, he had to use the means here so ably chronicled. For those who insist on the deeper view, this work will provide an invaluable guide into the day-to-day maneuvers, few of them edifying, albeit effective enough.

The book suffers from having no bibliography, and occasionally one wonders if one is reading Thornton's intelligent speculation or a documented particular. But the notes are full and very useful, and the work is an important contribution.

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JOHN HUNTER BOYLE. China and Japan at War, 1937–1945: The Politics of Collaboration. Stanford: Stanford University Press. 1972. Pp. ix, 430. \$16.50.

GERALD E. BUNKER. The Peace Conspiracy: Wang Ching-wei and the China War, 1937–1941. (Harvard East Asia Series 67.) Cambridge, Mass.: Harvard University Press. 1972. Pp. 327. \$13.50.

One of the not infrequent "accidents" of history is here repeated: two first-rate Sino-Japanese scholars, both Americans, almost simultaneously publish, in this case, outstanding works on essentially the same subject matter. Their focus is Wang Ching-wei (1883–1944), an extraordinarily attractive Chinese figure, a leading associate of Sun Yat-sen, friend and foe of Chiang Kai-shek, a leader in the Kuomintang who never had a firm political or military base in that nationalist party-amalgam that sought and fought unsuccessfully to unite China under its banner.

Though Wang is their focus, both Boyle and Bunker examine his role in the broader canvass of the Sino-Japanese War immediately before and after the Marco Polo Bridge Incident on July 7, 1937. Despite the cutoff date in Bunker's title, both authors carry their historical analysis past the death of Wang to the end of the war in the Pacific. Both authors utilize their voluminous data (archival and published sources in Japanese, Chinese, and Western languages and interviews with leading figures of the period) to illuminate Japanese policies vis-à-vis China, the Kuomintang, and the actual and potential Chinese collaborators, among whom Wang is unequivocally placed.

The Marco Polo Bridge Incident, highlighted ever since 1937, was just that. The Japanese had already conquered the Manchu territory and set up as their "puppet," as both authors agree, "Emperor K'ang-te" (Henry Pu-yi). At the end of 1937 the political/military leaders in Japan faced a series of policy issues: to expand the war and thus establish Japanese paramountcy over all of China or to continue some military action while offering "peace" terms to Chiang Kai-shek and the Kuomintang.

Both Boyle and Bunker point out that there were Japanese military and civilian leadership factions ranged on each side of the debate. When it became clear that Chiang Kai-shek would not accept their current peace terms—recognition of Manchukuo and acceptance of Japanese economic primacy, especially in the Northern Chinese and Inner Mongolian provinces controlled by their military with the aid of local Chinese "puppets" or "collaborators"—Prime Minister Konoye Fumimaro announced in January 1938 that Japan "would cease all

contact with the National Government," would not negotiate with or accept Chiang, and "would look forward to the establishment of a new Chinese regime with which she would adjust her relations" (Boyle, p. 4, Bunker, p. 2).

There then emerged—in China and Japan leading individuals and groups, some genuinely, some opportunistically, seeking to end the war with acceptable peace terms. Wang Ching-wei, always "an alternative to Chiang as heir of Sun in China's search for a viable political system to replace the old imperial system" (Bunker, p. 12), then serving as chairman of Kuomintang's Executive Yuan and as foreign minister, undertook to negotiate with some of the Japanese for attainable peace terms. Initially, it seems clear that Wang's effort was fully acceptable to Chiang. Wang sought to maintain the unity of China proper under a Kuomintang, anticommunist regime but would concede to Japan's "puppet" in Manchuria. The Japanese not only insisted on retaining Manchuria but also maneuvered for what came to be called its "bunji gassaku" policy, that is keeping China as a "federation of local regimes," a politically fractured China. Eventually there might be a new central government for all China.

It is at this point, late 1938 and early 1939, that Wang, having failed in this effort, departs Chungking and breaks with Chiang Kaishek. Wang decided to pursue his "peace conspiracy," hoping, perhaps, to succeed as an independent leader. The story of the next four to five years, as both Boyle and Bunker recount it, is made up of a tangled web of conspiracies, plots, counterplots, attempted and actual assassinations—all in the name of saving the unity of China and re-establishing peaceful relations with Japan. Wang eventually submitted to Japan's terms, and a "reorganized 'Kuomintang' Government" was established by the "basic treaty" of November 30, 1940, with Nanking as its capital seat. He was given the hopeless task of winning over to his leadership the "Provisional" and "Restored" local governments in North China and Inner Mongolia and also of detaching from Chungking dissident factions, such as warlord Lung Yun of Yunnan and others.

Bunker begins and concludes his analysis with an assessment that describes Wang in classical Greek tragedy terms: Wang's character determined his fate. "His courage caused him to dare, his blindness to persevere, to stumble, and to fall." Both authors reject the view that he was yet another Japanese "puppet." Boyle

suggests that we look at other Asian actors during this period, and he correctly cites the fact that certain ones among the collaborators with Japan—Roxas of the Philippines, Ba Maw and Aung San of Burma, Sukarno of Indonesia, and Subhas Chandra Bose of India—were and are accepted as heroes or patriots or both in their respective countries. Wang, however, became a "traitor" to his contemporary Kuomintang comrades though he had been considered a towering patriot for most of his life. At the end he failed because what he wanted to achieve was really unachievable at the time.

If the reader can afford to add both books to his library he should by all means do so. If he has to choose between them—a most difficult choice—then Boyle offers a somewhat larger canvass than Bunker. And the Harvard University Press should be chastised for the miserable way it reproduced Bunker's twenty-six photos, especially in comparison to the general excellence of those twenty-one reproduced by the Stanford University Press. Let the reader compare photo number 10 in Boyle with the sole duplicate on page 134 in Bunker.

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HELEN FOSTER SNOW (NYM WALES). The Chinese Communists: Shetches and Autobiographies of the Old Guard. Book 1, Red Dust; book 2, Autobiographical Profiles and Biographical Shetches. Introduction to Book 1 by ROBERT CARVER NORTH. Westport, Conn.: Greenwood Publishing Company. 1972. Pp. xxi, 398. \$15.00.

JACQUES GUILLERMAZ. A History of the Chinese Communist Party, 1921-1949. Translated by ANNE DESTENAY. New York: Random House. 1972. Pp. xviii, 477. \$12.95.

PETER R. MOODY, JR. The Politics of the Eighth Central Committee of the Communist Party of China. [Hamden, Conn.:] Shoe String Press. 1973. Pp. xi, 346. \$14.00.

These three books span well over half a century of Chinese history, from the last years of the Manchu dynasty to the Cultural Revolution of a few years ago. Some of the autobiographies appearing in Helen Foster Snow's The Chinese Communists take us back to the final decade of imperial China. And many of the figures mentioned in the autobiographies and again in Jacques Guillermaz's History of the Chinese Communist Party became the protagonists in the power struggles of the first two decades of the Chinese People's Republic, described in Peter Moody's The Politics of the Eighth Cen-

tral Committee, reminding us once more of the unusual longevity of China's revolutionary elite. But, while the actors provide a common denominator, the three books are very different from each other. Snow presents primary historical materials; Guillermaz gives straightforward descriptive history; Moody, although dealing with a later time period, provides interesting political analysis and interpretation.

The past two decades have seen the appearance of a number of valuable studies dealing with the Chinese communist revolution, but the field has lacked a history of the period that is comprehensive in scope or in time. Guillermaz's History of the Chinese Communist Party, 1921-1949 is among the first to fill this gap. It is a well-organized, generally reliable, and detailed account of the political and military events (with emphasis on the latter) surrounding the growth of the Chinese communist party from its foundation to its accession to power. This book should have its greatest appeal to the reader who has some basic familiarity with the subject and would therefore not be overwhelmed by a welter of often lifeless detail. It includes several dozen interesting photographs and many maps (especially of military movements) usefully scattered throughout. To the more specialized reader, however, it provides little more than a basic reference work. As a "comprehensive outline and synthesis of available knowledge" drawn primarily from Western and Chinese secondary sources, the book does not add significantly to our present understanding of the period. And while it claims to illuminate the causes of the communist success, the book reflects uncritical acceptance of earlier interpretations, such as the overwhelming significance attributed to the anti-Japanese war. The material presented is of course too general to support definitively any hypotheses about the successes of the revolution, but in an overview of this type one might have expected the author to have raised at least some points of possible interpretive debate and to have suggested future avenues of investigation.

Helen Snow's The Chinese Communists is essentially a welcome new edition of one of the classic English-language works about the Chinese revolution, Red Dust (published under the pseudonym Nym Wales). Supplemented by scattered additional materials, the new edition consists of twenty-nine transcribed interviews with Chinese communist leaders and partisans (five of them women) conducted by Helen Snow, mostly during a clandestine visit to the Yenan revolutionary base area in 1937. Although the

interviews were unsystematic and are of uneven length and content, they constitute (along with occasional photographs) vital and exciting raw material of modern Chinese history. To fully appreciate the autobiographies, the reader should have some knowledge of their historical context, and such a background is provided by Robert C. North's introduction to Red Dust, also included in this edition, but clearly dated by its 1950s cold-war tone. The interviews provide a rich panoply of anecdotal life histories reflecting the diversity of the people attracted to the revolutionary cause. The respondents range from the cosmopolitan children of scholarofficial families to illiterate or self-taught sons and daughters of peasants, factory workers, and coal miners for many of whom the Long March marked the greatest extent of their travels. But virtually all have served the communist movement with ever-increasing commitment as soldiers and leaders, as teachers, doctors, and actors. In addition to having experienced the common physical and psychological hardships of early twentieth-century China, as enemies of the political establishment they have shared a life of armed clashes, narrow escapes, and the loss of comrades and relatives.

Not all of the stories read smoothly, but we are thereby reminded that we are dealing with the transcription of rare bits of oral history. The anecdotes cannot, of course, answer all of the questions of social scientists, but many of the random details ("It was [in Tibet] that [General] Chu Tê got his big Sikang dog which barks at anybody who does not wear a redstarred cap.") present to all readers some very real human beings.

While the five biographies added to the original edition of *Red Dust* are among the most stimulating (in particular, one dealing with Mao's relationships with women in Yenan), the same cannot be said of the last section of the book ("Biographical Sketches"). This short "who's who" certainly does not make good general reading, and the incorporation of miscellaneous personal notes does not substantially supplement the more complete biographical reference books available to students of communist China (and from which a lot of this material was drawn). This unsuccessful attempt to make *The Chinese Communists* into a new book should not, however, obscure its basic value.

Peter Moody's The Politics of the Eighth Central Committee of the Communist Party of China is intended for the specialist in contemporary Chinese politics, although its conclusions

may be of interest to all students of comparative politics, and of communist systems in particular. It has the unpolished flavor of a barely revised dissertation, but its documentation from Chinese sources is extensive, careful, and thorough. The aim of the study is to suggest that a decade of internal struggles within China's political elite led to the upheavals of the Cultural Revolution rather than to a waning into a phase of revolutionary Thermidor. The "power struggle" approach to the Cultural Revolution is not a new one, although it has generally been used to describe the events of 1966-69 rather than to explain their occurrence. Moody delineates a number of political groups within the Central Committee membership and discusses their positions on various policy issues that were dominant from 1956 to 1966. He suggests that the major underlying conflict is between the party apparatus seeking to institutionalize its power and the charismatic leader, Mao, in coalition with other political groups among which he wishes to have power diffused. As the conflict developed, the Central Committee became increasingly "a federation of fairly autonomous hierarchies," each pursuing its political interests, leaving the party center unable to assume Thermidorian leadership. The interpretation requires a number of assumptions with which the reader may not always agree, but Moody appears conscious of the tentative nature of his analysis (a common problem in the field), and his arguments remain challeng-

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JOYCE C. LEBRA. Ōkuma Shigenobu: Statesman of Meiji Japan. Canberra: Australiar. National University Press; distrib. by International Scholarly Book Services, Portland, Ore. 1973. Pp. 195. \$10.45.

While ordinarily numbered among those former samurai who directed the course of Japan's imperial government from 1868 into the second decade of the twentieth century, Okuma Shigenobu was of a different order from most of his colleagues. They were from the great fiefs of Satsuma, Choshu, and Tosa; he was from the less important Hizen. And even in Hizen he had only tenuous connections, having rebelled against Confucian tradition to engage in "Dutch studies" in Nagasaki. There he learned not only Dutch and English but also a good deal about Western mercantile and financial methods, making him, by contemporary stand-

ards, unusually well prepared to deal with problems attendant upon Japan's emergence from feudalism into the modern world.

It was this expertise, rather than cabalistic credentials or personal clout, that brought Okuma into the inner circles of government, and Joyce Lebra emphasizes the paradoxical significance: an outsider, with no power base, Okuma was unencumbered with the parochial concerns of most of his peers; his horizons were less limited, his attitudes more flexible. Too flexible, perhaps, and he finally resigned out of frustration over what he regarded as the shortsighted conservatism of the dominant Satsuma-Choshu clique. Demanding a constitutional monarchy, Okuma then launched a career as a party politician committed to parliamentary democracy, the national welfare, and the rights and duties of the common man.

The author does not hesitate to criticize Okuma's occasional opportunism as he helped shape Japan's policies (twice as premier) through a long career that lasted until 1916, but she sees him on the whole as a man of honor, courage, and foresight, a man with "unswerving faith in the concept of progress in general and for Japan specifically" who became the archetypal national, as opposed to regional, statesman of Japan.

This well-documented, well-indexed book is a useful addition to the biographical studies of other leaders of Meiji and Taisho Japan. Lebra's stress is on Okuma the politician-statesman, and her explication of his place in the dreary genealogy of Japanese party politics is fairly painless. But the general dryness could have been leavened a bit if more attention had been paid Okuma's role in education (as founder of Waseda University) and as an ordinary man. Lebra's style is not very warm, and Okuma the private person seldom emerges; Mrs. Okuma is mentioned but twice, and their family life remains a secret. Joyce Lebra has told us much that is valuable about Okuma's politics but too little of the humane qualities with which a man of his vision must have been endowed.

EDWIN B. LEE
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HENRY DEWITT SMITH, II. Japan's First Student Radicals. (Harvard East Asian Series 70.) Cambridge, Mass.: Harvard University Press. 1972. Pp. xv, 341. \$16.00.

This is an impressive, scholarly book. The author, an assistant professor of history at Princeton University, has made an exhaustive study

of the precursors of the modern student movement in Japan. He begins by providing the reader with the necessary background on the university system and the roots of student unrest, such as simple rowdiness, objection to single matters, and political protests. The main body of the book details the founding of the Shinjinkai (New Man Association) and a number of other similar groups in late 1918 and early 1919 until the rise of militarism and the attendant suppression of the student movement in the 1930s. In the last part of the book the reader is treated to the author's analysis of the origin and later careers of the Shinjinkai members. These are in turn scrutinized, finally, to gain some perspective for understanding the revival of student movements after 1945.

The sources of information include an exhaustive bibliography, especially in Japanese, and many current interviews with surviving members of the Shinjinkai, the most representative of the radical groups. Smith pays meticulous attention to details and is very careful in making his readers aware of the limitations of his samples. This book is indispensable for anyone who wishes to learn something about the social ferment behind the modernization of Japan.

I am of the opinion, however, that Smith has not seriously taken into consideration the social and cultural factors in evaluating his data. Because of the fact that "42 out of 100 Shinjinkai members whose sibling rank is known were first sons," which seems to negate the common observation that "first sons in Japan tend to be conservative, responsible and inhibited" (p. 237), Smith dismisses such considerations as irrelevant. But there are other indications of the cultural factor, even from the data the author gives. For example, much of Japanese student life that preceded and accompanied the radical movement was characterized by brawls, pranks, and riots for such causes as bad food in the dining hall (p. 18). That such activities were not accidental was shown by the fact that in 1965, when I visited Doshisha and Kyoto Universities, the students who struck for political reasons also put on big character wall posters (as the Chinese did during the Cultural Revolution) bearing complaints ranging from insufficient heat in the dorms to a poor quality of management of dining halls.

Once this kind of cultural continuity is understood (and the author sees continuity in the Japanese student movement [p. 262]), one

must ask, can the fact that a very substantial number of "jailed communists began gradually to recant" and "the apostasy of hundreds of other imprisoned leftists" be due simply to the "sophisticated use of techniques of persuasion by the government authorities" and the tenho (turn coat) of two communist leaders (p. 222)?

Similarly, Smith seems to offer no more than the "widespread public concern over Japan's security" (my italics) following the controversy over the London Naval Conference in 1930 as the explanation for "the sudden resurgence of student nationalism" and for the fact that "where only twenty-two right-wing campus groups existed prior to September 1931, four times that many were active by early 1933" (p. 220). How did such a widespread leftist movement ("In geographical origins, the Shinjinkai members came from every corner of Japan, representing all the forty-seven pre-war prefectures." [p. 235]) turn in the opposite direction within such a short time? Could it be as simple as the author would have us believe? Had he looked at the neighboring Chinese society, might Smith not have noted that the Chinese disappointment at the Versailles Peace Conference of 1919 was far greater, and the threat to China's security in the ensuing decades far more severe than Japan's in 1930? The May 4 movement of 1919, led by students of National Peking University, had all-China repercussions. But except for a tiny minority who mouthed fascism or Nazism, it was not followed by any proliferation of right-wing campus movements. On the other hand, many of the same Japanese radical students themselves, whose forebears were brought up on J. A. Hobson, G. D. H. Cole, Bertrand Russell, as well as Karl Marx, P. J. Proudhon, and Lenin (as were their Chinese counterparts), were later the Japanese soldiers and officers who became zealots in the conquest of China and who committed against innocent Chinese men, women, and children atrocities worse than those of the Nazis against the Jews.

Western readers today tend to be ignorant of the magnitude of the Japanese atrocities in China for two simple reasons. First, the Chinese themselves, unlike the Jews, have tended to forgive and forget them rather than to amplify and revivify them. That has something to do with Chinese national character and culture. Second, and more important, Americans have minimized even atrocities by whites against nonwhites, not to speak of those of nonwhites against themselves. After all, would the average American have tolerated the daily body counts on TV if the bcdies were not those of Vietnamese but of Europeans?

The security of the United States has been exploited to justify all sorts of American military ventures abroad. Is Smith as a historian falling into the same psychological trap from which his fine scholarship cannot rescue him?

FRANCIS L. K. HSU
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BENJAMIN C. DUKE. Japan's Militant Teachers: A History of the Left-Wing Teachers' Movement. (An East-West Center Book.) Honolulu: University Press of Hawaii. 1973. Pp. xvi, 236. \$9.50.

DONALD R. THURSTON. Teachers and Politics in Japan. (Studies of the East Asian Institute, Columbia University.) Princeton: Princeton University Press. 1973. Pp xiv, 337. \$14.50.

Benjamin Duke's study is not quite what it says it is. It is thorough, detailed, and well-documented, with an excellent set of translated writings appended to the body of the book. However, Duke has studied the Nikkyōso (the Japan Teachers' Union) and its leadership, not the teachers themselves. In this sense it is a less satisfactory book than Thurston's, which focuses on the issue of teachers and politics and pursues it with vigor and crisp analysis.

Duke has done a commendable but not an inspired job. He has identified accurately the pre-World War II roots of the teachers' movement and traced with great care the evolution of the leadership of Nikkyoso. His final chapter, "Militancy, An Analysis of the Czuses," is particularly good in relating the careers of some of the top leaders (Iwama Masao, Kobaya-Takeshi, and Miyanohara Sadamitsu) to institutional developments. Perhaps the most significant of his conclusions, the basis for which he develops carefully and soundly, is that "Communist influence within Nikkyoso has been dominant only when the Communist bloc on the Central Executive Committee was capable of leading the union for or against an issue about which the average union member felt strongly" (pp. 201-02). Anyone wishing to follow and understand the development of this very important institution will find the book an excellent guide, if not definitive then close to it. There remains, however, the very important work of studying the teachers themselves. How did they relate to and view the Nikkyoso and its leadership? What effect did the union's activity have on their daily work and on children themselves? We get little sense of this aspect of the topic.

Thurston's work is much more satisfactory in concept and design. He states his purpose clearly in his introduction. It is "to evaluate the influence the JTU has had as a renovationist interest group on its own members and on the formulation and implementation of educational policies. At a higher level the purpose is to evaluate the significance of the JTU in postwar Japanese politics and society." He has done a superb job of carrying this purpose to a successful conclusion. As a political scientist he has structured his analysis of Nikkyōso in the typology of interest groups, certainly an accurate and helpful approach.

The first quarter of the book is devoted to background. One has to wade through a plethora of political science jargon, but the analysis is accurate and incisive. Thurston's presentation of union ideology and objectives is sound and sets the stage for his discussion of union organization, relationships with the ministry of education, and the teacher's role in politics. One of the most important sections of the book is the author's discussion of the "union consciousness" of teachers (ch. 5). Here we discover the individual teachers, their self-perceptions, and their views of the union, its role, and their relationship to it. We get a sense of how people live and act professionally. Another major contribution of Thurston's work is the clarification he provides of the distinction between the relative lack of influence of Nikkyöso at the national level and its great influence at the local level. He points out (pp. 212-13) that the union has never been successful at influencing the policies of the ministry of education as they are formulated, but since its members are the people responsible for implementation of these policies, teachers have significant if not decisive educational influence at the prefectural and local level.

This combination of careful analysis and meaningful examples highlights the book throughout. Thurston's brief concluding chapter brings the material together in helpful summary form. The documents in the appendixes and the extensive bibliography enhance the value of what the author has done.

These two books fill a major gap in material in English on an important aspect of Japan's postwar experience. Specialists and generalists alike will find them useful.

JACKSON H. BAILEY Earlham College

S. R. RAO. Lothal and the Indus Civilization. With a foreword by SIR MORTIMER WHEELER. New York: Asia Publishing House. 1973. Pp. xix, 215, 43 plates. \$35.00.

With the political partitioning of South Asia in 1947, Indian archeologists initiated a search for Bronze Age sites within their new boundaries that would equal those of Mohenjo-daro and Harappa in the Indus Valley, which had gone to Pakistan. In 1954 S. R. Rao discovered Lothal in Gujarat, a poor cousin to the ancient cities to the north, but certainly within their cultural penumbra. In this volume the discoverer and principal excavator of Lothal offers a survey of archeological research carried out over the past fifty years. Along with a description of Indus civilization, economy and trade, the arts and social life, there is a discussion of the Indus script, which Rao tells us he has deciphered. Preceding the index are over forty plates, some in color. Line drawings appear with the chapters. Scales do not appear with any photographs and are present in only a few of the drawings. The foreword by Sir Mortimer Wheeler is a historical overview of dating analysis.

I myself, a visitor to Lothal, find it difficult to appreciate how a site only two kms. in circuit could support a population of ten to fifteen thousand inhabitants, as Rao claims. He explains the large artificial body of water at Lothal as a dockyard capable of containing at one time thirty ships of fifty tons each, yet does not cite his archeological colleagues holding alternative interpretations about the function of the tank. Surely our author has another word in mind when he writes, "the seafaring merchants of the Indus Valley cities . . . established a small colony at Lothal circa 2450 B.C. with a view to refuel their south-bound ships" (p. 55).

Turning to the broader issues of Lothal's place in the sphere of the Indus civilization, Rao disclaims any significant cultural influence from the Near East. Indeed, even the idea of civilization is disassociated from the rise of high cultures in Mesopotamia since Sumerian and Indus customs are not identical! But Rao is quick to itemize benefits that early civilization, originating independently in South Asia, has bestowed upon peoples beyond the Indus. Among some of the inventions he attributes to the Indus civilization are the compass, the auger drill, and circular saw, "English bonding" techniques in masonry, cultivation of rice, standardization of goods and services (but without

threat to individual initiative, he assures us), the origins of Yoga, compassion for animals, and adoration of the fire god, plus a number of spiritual innovations. The foundation of marine engineering in the world is enshrined at the dockyard at Lothal. Western archeologists may have difficulty in accepting these arguments for one-way cultural diffusion from the Indus Valley.

As a biological anthropologist familiar with the skeletal remains from Lothal and related sites. I admit acute discomfort from our author's application of venerable anthropometric studies, some of limited value, to the very real problems about the biological history and affinities of the people of this high culture. The cranial index is misapplied as a label for racial type. It is jarring to read that "if the linguistic term 'Aryan' is applied to the dolichocephals of Lothal . . . it follows that the majority of the Lothal population of Period A was Aryan" (p. 157) or to note Rao's failure to attack the notion that "some anthropologists hold the view that the Mediterranean (racial type) has contributed most physically, while the Armenoid (racial type) may have contributed more (p. 158). The important questions culturally" of biological adaptability and change of the Indus civilization skeletal series are not raised.

We are indebted to Rao for his reporting of dates from recent radiocarbon analysis, the announcement of new sites, and his imaginative interpretations of the Bronze Age peoples of India and Pakistan, a report that will stimulate discussion among his co-workers and afford a provocative introduction for the nonspecialist interested in the rise of high cultures in South Asia.

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MAURICE HENNESSY. The Rajah from Tipperary. New York: St. Martin's Press. 1972. Pp. xx, 183. \$5.95.

His role was never that of a Wellesley or a Clive, but for at least thirteen years George Thomas was a significant figure in northwest India, and from 1797 until his final defeat in 1801 he did indeed make himself rajah of the sizable area of Haryana. He was one of a number of colorful European mercenaries playing important roles in the political and military struggles that characterized India in the last decades of the eighteenth century. Thomas is therefore a worthy subject for a popular biography, illuminating a significant segment of

Indian history and of the European experience therein.

Unfortunately Maurice Hennessy's effort falls disappointingly short of the mark. It neither illuminates Indian history nor gives a balanced account of Thomas's life and impact. The bibliography itself is sparse. It consists of secondary sources and contemporary accounts. But it is the uncritical use of sources that leads to serious flaws in the book. Hennessy relies heavily upon Thomas's own account of his Indian adventures-indeed several chapters are based on nothing else-without apparent awareness that history is more than the recollections of one or a few participants. He fails to recognize the limitations of Thomas's memoirs, as dictated to another officer shortly after his final defeat. This and two other contemporary accounts, which he calls his three primary sources, provide enough information, the author believes, to "satisfy the most demanding of historians."

Hennessy's understanding of the Indian scene is dubious at best and is marred by factual errors. He repeatedly refers to British India and the East India Company as if they were competitive sovereigns, ignoring parliamentary controls over the company's political activities. Twice he identifies Haidar Ali of Mysore as a "Hindu" rather than a "Muslim" ruler. Elsewhere he mentions Haidar "and his son, Tippu of the Carnatic," as if the two successive Mysore rulers were sovereigns of separate principalities. Inexcusably careless errors also occur, as when the author refers to Thomas's operating area of Haryana, Punjab, and Rajasthan as "eastern India"

Readers might be well advised to look to another recent popular study, Shelford Bidwell, Swords for Hire, European Mercenaries in Eighteenth-Gentury India (1971). Bidwell devotes approximately half of his work to Thomas and portrays him more clearly than Hennessy does. Bidwell also serves history better, for he provides a balanced analysis of the time and setting in which Thomas operated.

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B. G. GAFUROV. Tadzhiki: Drevneishaia, arevniaia i srednevekovaia istoriia [Tadjiks: Prehistoric, Ancient and Medieval History]. (Akademiia Nauk SSSR, Institut Vostokovedeniia.) Moscow: Izdatel'stvo "Nauka." 1972. Pp. 657.

This work is not just a history of the Tadjiks from the earliest times to the eighteenth century, but a cultural and social, as well as a

political, history of the area of Soviet Central Asia. The author, academician Gafurov, is not only himself a Tadjik, hence with special insights into Central Asian matters, but he is also the director and chief architect of the flourishing Institute of Oriental Studies of the Academy of Sciences of the Soviet Union. If any Russian book dealing with this period of the history of Central Asia ought to be translated into English the present publication is surely that one. The author had written previous works on the Tadjiks, and the present volume is the culmination of much past research. A number of specialists advised the author on minute points of prehistory, archeology, numismatics, and the like, making the book a veritable handbook of all the facets of the history of Central Asia.

After reading this book one has the impression that Soviet scholars have completely transformed our view of Central Asia compared to sixty years ago when it was a vast terra incognita. Never has archeology anywhere been so rewarding. The sketches in the book of reconstructed costumes of peoples in Central Asia in various periods of history, in my opinion, are a welcome and valuable addition to the volume. Extensive bibliographies are given and many photographs are of new and unknown materials. For example, the huge reclining Buddha from a monastery in Adjinra Tepe has opened a new field in the study of Buddhist remains from Soviet Central Asia.

It is impossible in a short space to discuss the many new materials presented in this book, which is systematically arranged by periods, in each of which social and economic questions are discussed as well as cultural and political history. With full indexes this publication will undoubtedly become a handbook for the subject and can be well recommended.

RICHARD N. FRYE
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HAFEEZ MALIK, editor. Iqbal: Poet-Philosopher of Pahistan. (Studies in Oriental Culture, number 7.) New York: Columbia University Press. 1971. Pp. xviii, 441. \$12.50.

It is difficult to capture in a brief review the mind and life of a man so complex as Iqbal. However, some of the essential points are made in this book, indeed even in its title. Iqbal has had a profound influence on the life of Pakistan, not only in its conception but increasingly in the 1970s as a kind of legitimizing force as Pakistan seeks to define its Islamic nationhood

in contemporary terms. That Iqbal should provide this kind of philosophical reference point to Pakistan thirty-six years after his death is a reflection of the depth and range of his vision and his importance as a political thinker among the Muslims of South Asia. The poet-philosopher's son, Javid Iqbal, a prominent jurist of Lahore, makes this characterization at the conclusion of his brief essay in this volume: "Today [Iqbal] is a living force in the minds of sensitive Pakistanis—inspiring, directing, and sustaining us in our struggle to reconstruct our national life" (p. 63).

The strength of this collection of seventeen essays by an illustrious international panel of Igbal scholars is that it brings understanding, and through the many views of its authors it achieves balance. There is little of the hagiography that appears increasingly in the public press and in the statements of public men in Pakistan. Malik and his colleagues have done an enormous service in providing an American audience and the world of scholarship with this detailed and balanced assessment of Iqbal so shortly before the poet-philosopher's birth centenary in 1977. Muhammad lqbal was and is many things to many people in personal, philosophical, and ideological terms. The essays in this volume represent many of these perceptions. They are organized under five broad rubrics—biography, politics, philosophy, Islamic mysticism, and poetry-and while they vary in quality, taken as a whole they illuminate abundantly the career of a major figure in the twentieth-century history of South Asia. Iqbal emerges as a man of pragmatic action and thought as he seeks to interpret Islam in very human and humane terms. He has a very "this worldly" quality about him, and I suspect it is this dynamic sense of his view of man in the world that has made him such an intellectual force in the life of Pakistan, Islam in South Asia, and beyond. Professor Hafeez Malik has provided a useful bibliography and two important letters from Iqbal to Jinnah in an appendix. The book is effectively organized making the ideas of Iqbal and the essayists easily accessible. It is a good book and a good job of bookmaking.

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GERD LINDE. Burma 1943 und 1944: Die Expeditionen Orde C. Wingates. (Einzelschriften zur militärischen Geschichte des Zweiten Weltkrieges, 10.) Freiburg: Verlag Rombach. 1972. Pp. 207.

On March 22, 1942, Major Orde Wingate arrived in Maymyo, Burma, to begin implementing a "long-range penetration" tactic of guerrilla warfare behind the Japanese lines that would, it was hoped, significantly affect the Asian land war but that, in any case, would prove to be the crown of Wingate's own short, intense, and often controversial military career. The present volume is a brief military history of Wingate in Burma. After sketchy introductory sections on the Burma theater of war and on the development of the "long-range penetration" concept, Linde focuses almost entirely on the 1943 and 1944 operations of the Chindits, which went on uninterrupted by Wingate's own death in an air crash in the Bishanpur mountain range in March 1944.

Written with a careful eye for logistical detail, tightly narrated, and well provided with sketches and reproductions of operational documents, the book is indispensable for any study of the Burma campaign and constitutes a useful supplement to the studies on Wingate's exploits by Christopher Sykes, Bernard Fergusson, and others. In his conclusion Linde sees an even longer perspective: touching on the persistence of guerrilla insurgency in Upper Burma today and on the proximity of Chinese military concentrations, he raises the possibility that the embattled strongholds of the 1943–44 Chindit campaigns may "also in our time" become new centers of tactical significance.

Though not primarily a study of command, the book inevitably raises again the question of Wingate's leadership and tactical perception, now perhaps of greater interest than ever in this era of preoccupation with guerrilla and "people's war." The record seems cloudy. Wingate rejected the tactics of Lawrence of Arabia, which he, rather unfairly, stigmatized as "letting people fight for one" for "a sack full of gold" (the Royal Central Asian Society's subsequent award to Wingate of its Lawrence of Arabia Memorial Medal must have moved Wingate "strangely," as Linde observes). Instead Wingate, on the basis of his Ethiopian experience, urged that the population be made to realize the existence of a common enemy, that others should not be fighting "our battles for us," and that if there was a popular readiness to help, an offer to supply arms could and should then be made. In the event, though an anti-Japanese guerrilla movement grew in Burma in the course of the war, the Chindits' relationship with it remained quite limited. Neither British nor United States operations were really part of a Burmese "people's war," and, as Linde

observes, the Chindits were neither "partisans" nor ideological fanatics.

Then, too, according to Wingate's loyal subordinate. Brigadier Bernard Fergusson, the development by Wingate of direct air assistance and supply of ground forces were "wholly new contributions to military thought," while his ideas on "long-range penetration" were not merely dimensions of localized guerrilla action but were viewed by him as "integral parts" of a battle to bring the access rcute to China into Allied hands. There is no question of the significance of the Chindits' campaigns in terms of the latter objective. Yet it remains questionable whether Wingate, despite his extensive guerrilla experience in Palestine and Ethiopia, was necessarily the best man for the job, given his difficult personality and the operational limitations imposed upon him. One sidelight is revealing. Linde notes that after the Quebec Conference the well-known American fighter pilot, Colonel Philip Cochran, became Wingate's air force commander. But Wingate and Cochran were "at first sight thoroughly unsympathetic" to each other, and Cochran's unit remained under American command. Little love also appears to have been lost between Wingate and other American commanders, for example Lieutenant General Joseph Stilwell. Linde describes Stilwell as a "typical American" who was unable to put himself in the place of "other peoples" and who allegedly reflected his "national inferiority complex" with respect to military planning. Such lapses, fortunately, are few in these pages, and the concluding section of the book gives a fair, tactical synopsis of Stilwell's "special force" in May 1944.

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PETER BOLGER. Hobart Town. Canberra: Australian National University Press. 1973. Pp. xvii, 237. \$A6.95.

The jacket in which Peter Bolger's Hobart Town is wrapped suggests that "this book gives a lively account of the growth of the City of Hobart from its earliest days as a convict settlement to a metropolis with wide streets and fine buildings." Such an account would be welcome, but this work does not contain any such complete story. Rather, it deals in detail with the local history of Hobart, now the capital city of Tasmania, between about 1844 and the end of the 1880s. Only a short twenty pages are given to the two decades from settlement in

1803 and relocation in 1804 until the mid-1840s when the old town began to take definite shape. Likewise the reader is inclined to feel shortchanged by the fact that the story ends a decade before the end of the nineteenth century. Except for a short epilogue of less than six pages devoted to but four incidents from among events of the 1800s, the decade prior to federation receives no attention. While the author calls this one of the "most dramatic decades of the century," it rates almost no attention, regardless of the fact that during these years Hobart cast off its earlier simplicity and assumed a new role as the capital of one of the states of Australia. The limitations that Bolger has placed on the story will be regretted by scholars searching for something like the account provided in A. Morley's Vancouver-From Milltown to Metropolis (1961) where the story of a provincial city of the British Empire is told with real appreciation for the way in which, regardless of the disadvantages of location, a new city served as the core from which life radiated out to the nearby beaches, bush, and hills.

Many historians of Australia—especially those dealing with the colonies where early settlement was largely a result of transportation of convicts-tend to draw, as George Johnston says, a discreet curtain across "raw and hard beginnings." Bolger does not fail to tell of convict lawlessness-which he says has made "Tasmanian history a two-headed monster"-but he tends to overlook many of the valuable contributions that the convicts and their descendants made to "comfortable little Hobart." He tells the story of a Victorian community dedicated to seeking respectability and gaining acceptance and provides a varied account of merchants, clergymen, lawyers, prostitutes—and citizens in general-during the fifty years from the 1840s through the 1880s. The story is interesting and valuable. While based on little-known local records, which give it worth, the account is too anecdotal and lacking in interpretations to be fully satisfactory. The author cites in his bibliography, which is only partial and refers the reader to a more complete listing in an earlier doctoral dissertation deposited in the distant libraries of the Australian National University and the University of Tasmania, a number of books about the history of other cities. No references are made to these in the text, but it would have been helpful if comparisons had been drawn between events and developments in Hobart and those in other South Pacific cities like Sydney, Melbourne, Auckland, and Wellington. In fact other growing cities of the nineteenth-century British Empire, like Toronto, Halifax, Winnipeg, Port Elizabeth, and Durban, faced many of the same problems as Hobart, and the story could have been enriched by demonstrating how developments in Tasmania were similar, or dissimilar, to those of widely separate regions of British migration and settlement on other continents. All were shaped to a greater or lesser degree by their isolation, or what Geoffrey Blainey has so aptly called "the tyranny of distance." The benefits of such comparative study are real and would have added to the value of Bolger's work for those other than readers with a purely local interest in the subject matter.

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BEDE NAIRN. Civilising Capitalism: The Labor Movement in New South Wales, 1870–1900. Canberra: Australian National University Press; distrib. by International Scholarly Book Services, Portland, Ore. 1973. Pp. xii, 260. \$13.45.

In his valuable and compressed introduction Dr. Nairn states that his book is attempting three objectives: first, to describe and analyze the key role of the trade unions, particularly that of the Trades and Labor Council in New South Wales between 1870 and 1891, when they formed the parliamentary Labor party; second, to argue that the Labor movement was "an integral social institution" that kept capitalism under control throughout the eighties and, in 1891, formed the Labor party to civilize a capitalism that was becoming uncontrolled; and third, to prove that by 1900 the Labor party was "a mass democratic radical party." It is unfortunate that these three objectives are overwhelmed at times by archival overkill-too much detail, too many quotes, not enough generalizations, and no proper concluding chapter bringing the whole work into focus by summing up all the evidence and encapsulating what has been proved. Often the details are so bountiful that the year of the events is omitted for pages, causing the reader to hunt at the back of the book for footnotes giving the date. More serious, however, is Nairn's pro-Labor bias to the point that famous non-Labor statesmen, such as Sir Edmund Barton, are not given fair treatment. Furthermore, the author fails to provide a periodic synthesis of the general events of the years 1870 to 1900 and how the Labor party related to these events. Three examples will suffice: first, the role of the Roman Catholic Church in the trade-unions movement and in the Labor party is glossed over; second, Nairn does not clarify the role of the upper house, the Legislative Council, though he brings out its seeming inurement to change; and third, the part played by the immortal Billy Hughes is not made clear enough, nor is his demagogic touch properly illuminated.

But the book has many plusses. Nairn shows clearly how the power of the Trades and Labor Council handled such issues as the eight-hour day and the payment of M.P.'s. He pinpoints the parliamentary confusion after 1885 and builds on the book by Peter Loveday and A. W. Martin, Parliament Factions and Parties (1966; AHR, 73 [1968]: 893-94), whose thesis was that there were no real parties before 1891, only groups with leaders such as Sir John Robertson or Sir Henry Parkes. Nairn praises Parkes as a man possessing "extraordinary political instinct and perception," and his final resignation in 1891 is clearly attributed by Nairn to Parkes's desire to weaken the controls of the new Labor party. Other political figures of importance are well identified, and the role of the Labor party's George Reid emerges with clarity. The irony is not lost on Nairn that Reid's reforms in New South Wales and his tireless and courageous work for federation cost him the honor of becoming Australia's first prime minister in 1901. Nairn produces evidence to prove that Reid "was incontestably the outstanding man of the colony" in the midand late 1890s. He also stresses, quite correctly, the "entrenched legislation paralysis" of the New South Wales parliament before 1891. The intricacies of politics, the seamy double-crossing, and the slick maneuvering are nicely brought out. He introduces new evidence to show how the 1895 election marks "a further great and complex step towards the modernization of New South Wales politics" (p. 129). He shows how the Labor party's position in opposing non-European migrants was not "racist" but purely economic, because Chinese and other alien workers were a threat to reasonable conditions of work and living. Nairn documents very well the disunity and confusion of the socialists. Finally, the last part of the book improves as it shows how the federation movement dominated politics, how the Labor party made federation possible in New South Wales, and how the party ended the century by asserting its autonomy. Nairn's archival digging is truly impressive.

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## UNITED STATES

EDWARD A. PURCELL, JR. The Crisis of Democratic Theory: Scientific Naturalism & the Problem of Value. [Lexington:] University Press of Kentucky, for the Organization of American Historians. 1973. Pp. xi, 330. \$11.50.

If an American's doing American intellectual history is to have any extraordinary excitement at all, then that excitement invariably comes from the reflexive act of making problematic the knowledge and belief systems of the historian's own community. This act demands that the historian be self-conscious as he works through the task before him. Purcell is not self-conscious and his book is not exciting.

Nonetheless, it is solid intellectual history of the traditional sort, solid enough in fact to win the Frederick Jackson Turner Award for 1972. Purcell builds a case for the growth of a midtwentieth-century relativist theory of democracy out of nineteenth-century scientific naturalism. Science provided American intellectuals with metaphors and methods that, when pressed to their logical conclusion, posed a multiple threat to traditional democratic theory (a theory that Purcell never quite articulates). The Darwinian society-nature analogy from biology and the non-Euclidean metaphor from geometry, logic, and physics undermined absolutist ethics, while the methodological imperative of scientific naturalism—that is, the commitment to empiricism and experiment-challenged the possibility of government by a priori law, the rationality of human behavior, and the practical possibility of popular rather than elite government. Purcell identifies the relativist inheritors of scientific naturalism in several intellectual communities, including the legal realists, the Boasian anthropologists, the logical positivists, the Chicago school of sociology, the psychologists of the irrational and subconscious, and political scientists in the manner of Lippmann and Arnold.

According to Purcell's scenario, relativist theory faced its maturing crisis when it was forced to deal with the reality of European dictatorships. By the late 1930s a full counterattack was underway, restoring Aristotelian deduction, fundamental law, and a Thomistic rationalism critical of pragmatism. From this conflict emerged a new relativist theory of democracy (largely the work of Dewey) that viewed American experience as the empirical and moral norm for democracy. Thus, what was meant to be only a scientifically descriptive theory became prescriptive, carrying neo-relativists like Daniel Boorstin and Reinhold Niebuhr

into the postwar decades armed with an essentially conservative ideology.

Despite the fact that this book traces the intellectual genealogy and hard times of relativist theory. Purcell is not at all self-conscious about his model of change in belief systems. His chapter titles abound in the words of conflict, yet Purcell lacks the sort of language that Gene Wise in his American Historical Explanations: A Strategy for Grounded Inquiry (1973) or Robert W. Friedrichs in his A Sociology of Sociology (1972) use so well to talk about paradigmatic changes in intellectual communities. Both of these books overlap with Purcell's on certain issues and persons, but they are ultimately more impressive than Purcell's in that they make explicit their model of change—in both cases, Thomas Kuhn's.

Purcell also lacks a self-conscious sense of the relationship of scientific naturalism and relativist democratic theory to the community of American historians. Boorstin and Niebuhr are really the only historians to whom he pays any serious attention, and there is little or no mention of several other important, contemporaneous intellectual historians (Parrington, Miller, Gabriel, etc.). Surely those intellectuals deserve some attention in any analysis of the paradigm revolution in twentieth-century democratic theory.

Which leads finally to a third way in which Purcell lacks a self-conscious voice in his task. For a book that is about the tenuous and sometimes dilemma-laden relationship between ideas and human values, The Crisis of Democratic Theory is strangely value-neutral in tone. In an early chapter Purcell writes that at the center of the debate over the methodology of scientific naturalism was "the crucial problem of the nature of scientific knowledge and its relationship to human value judgments" (p. 40). I would have Purcell substitute "historical knowledge" for "scientific knowledge" and ask the question of himself.

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WILLIAM H. CARTWRIGHT and RICHARD L. WATSON, JR., editors. The Reinterpretation of American History and Culture. Washington: National Council for the Social Studies. 1973. Pp. xix, 554. \$8.50.

TIMOTHY PAUL DONOVAN. Historical Thought in America: Postwar Patterns. Norman: University of Oklahoma Press. 1973. Pp. ix, 182. \$7.95.

GENE WISE. American Historical Explanations: A Strategy for Grounded Inquiry. (The Dorsey Series in American History.) Homewood, Ill.: Dorsey Press. 1973. Pp. xx, 370. \$5.95.

The Reinterpretation of American History and Culture consists of twenty-five essays commissioned by the National Council for the Social Studies. The aim is to make available, especially to teachers, "as authoritative and up-to-date an account of the state of scholarship in American history" as could be reasonably put into a single volume (p. 15). Hence the essays are of the same genre as those published in the American Historical Association's pamphlets; and one of the latter (Jack P. Greene's Reappraisal of the American Revolution in Recent Historical Literature) actually reappears here in slightly modified form (pp. 259-95). Although some readers of this Review are bound to find that contemporary scholarship in their own specialty has not been summarized and interpreted exactly as they might wish, the volume undoubtedly succeeds in pulling together the conventional wisdom on most standard questions. The contributors are accomplished scholars in the fields about which they have been asked to write (for example, Ann Firor Scott on women, Paul K. Conkin on intellectual history, W. W. Abbot on colonial America, Robert Wiebe on the Progressive Era), and they speak always for a substantial professional constituency. The opening selection by Edward N. Saveth (pp. 17-34) will probably gain the widest readership, since it discusses the scholarship of the 1960s as a whole. Saveth sometimes strays from his mission (for example, he gratuitously tells the radical Stoughton Lynd how short a life span the latter would have "in the Marxist paradises of Brezhnev and Mao"), but manages to focus helpfully on the decade's interests in ethnic and class relations, cliometrics, and microanalysis.

Saveth's essay affords an instructive contrast to Timothy Paul Donovan's Historical Thought in America: Postwar Patterns. Donovan addresses not only the 1960s, but the entire period since 1945, and is more concerned than Saveth with the basic philosophical ideas of historians. Still, what distinguishes Saveth's approach from Donovan's is less the questions pursued than what sort of evidence is used to answer them. Donovan depends almost entirely on programmatic or aphoristic statements by historians about what history is or ought to be, while Saveth bases his generalizations on the actual characteristics of monographs. Saveth's analysis would be richer had it given more attention to

the moral and social values implicit in the monographs of the 1960s, and Donovan's would be more interesting if the latter's sense of what counts as historical thought were expanded beyond prefaces and presidential addresses to include the philosophical outlook implied by actual works of scholarship. Within its own terms, Donovan's book is comprehensive: it scrutinizes the views of not only the leaders of the guild, but also historians who have contributed to the Georgia Review, the Maryland Historical Magazine, and Catholic World. Unfortunately, his careful research is not focused as clearly as it might be; the reader must strain to find cogency in, and support for, Donovan's argument that World War II is somehow responsible for what he sees as "a revitalized humanism" in history, according to which "the significance of personality and human values in the drama of history" (p. vii) have been re-emphasized.

A much more bold approach to recent American historical thought is found in Gene Wise's American Historical Explanations, a sprawling, informal, yet intensely ambitious book with three salient purposes. It seeks to describe the metaphysical and moral outlook implicit in the writings of twentieth-century historians of the United States, and to account for the periodic changes in this outlook. It aims, secondly, to justify and to practice an ostensibly new mode of critical historiography, the study of "explanation in American historical studies." Finally, the book criticizes what its author believes to be conventional views of the nature of historical knowledge, and seeks to articulate and to defend an alternative view with which Wise's plans for historiography are supposedly compatible.

Wise's pursuit of the first aim is the most successful, for his analysis of Frederick Jackson Turner and Vernon Louis Parrington sharpens our understanding of how the progressive historians dealt with their own inchoate recognition of nonprogressive aspects of American experience. His discussion of R. W. B. Lewis and Perry Miller illustrates how historians of the 1940s and 1950s focused on complexities and ironies slighted by their predecessors. Wise's close study of Miller's New England Mind: From Colony to Province is his most valuable contribution, partly because it establishes the truth of Miller's claim to have depicted inherited ideas as a culture's functioning instruments for perceiving and contending with contingent experience. Wise is at his best doing textual exegesis, and is much less compelling when attempting to deal with general patterns in his-

torical writing. Wise seems to assume that there is a single torch to be passed, and that the ambiguity-eschewing New Left grabbed it in the 1960s from the faltering hands of the ironyloving "counter-Progressives," just as the latter had once left straightforward old Charles Beard to die in the dark. It is the "counter-Progressives" about whom Wise has potentially the most to teach us; unfortunately, he has squandered his energies on impressionistic accounts of "paradigm-change" instead of looking more closely at John Higham's Strangers in the Land (1963), Henry F. May's End of American Innocence (1959), William R. Taylor's Cavalier and Yankee (1961), Bernard Bailyn's Ideological Origins of the American Revolution (1967), Robert Wiebe's Search for Order: 1877-1920 (1968), and Winthrop D. Jordan's White Over Black (1969). Scrutiny of these proverbially influential works might have enlarged Wise's characterization of the ironic vision, and saved him elsewhere from the incongruous claim that Miller, whom Wise correctly sees as the pre-eminent "counter-Progressive," is also the neglected hero of a "revolution that never happened' (p. xiii) in the methodology of intellectual history. Miller is decidedly not the only historian of the last twenty years to treat ideas as devices for coming to grips with concrete social situations.

Wise is surely right in asking that we treat the writings of historians not simply as accurate or inaccurate interpretations of primary sources, but as intellectual artifacts in themselves, with their own conventions and symbolic structure. This idea certainly deserves the wider acceptance Wise hopes to win for it, but he overshoots the mark by insisting that his own advocacy of it constitutes the opening up of a "new field" (pp. ix, xii, xvii, 51, 153) for scholarly inquiry. Wise understands that his own approach is not so distinct from that of a dozen or so historiographers whose work he acknowledges (esp. p. xvii), but he is needlessly offended by the fact that many of these scholars make history, and not historiography, the major focus of their careers. The actual innovation Wise seeks to introduce into our historiographical tradition, moreover, consists largely in the acceptance of a very dubious presupposition about the discourse of working historians. This discourse, we are to believe, has an endemic structure so fixed and determinate that we can fairly represent it by a set of interlocking jargonbound concepts of Wise's invention (explanation-form, situation-strategy, fault-line, pivotalmoment). Indeed, what weaknesses there are in Wise's account of Miller derive from his overly

literal attachment to this pretentious and stifling analytical apparatus.

What Wise has to offer as a theorist of historiography and as an interpreter of twentiethcentury scholarship may go unread, however, for American Historical Explanations opens with a disastrous, three-chapter discussion of "objectivity" and "relativity" that is bound to create much distance between Wise and any but the most charitable of readers. In the most striking of several cannards against historians, Wise denies to us the understanding that our claims about the past are, in themselves, episodes in intellectual history and therefore, like any human cognition, "grounded in time and place and circumstances and social mileu" (p. 57). It seems not to have occurred to Wise that this truism about historical knowledge could be consistent with the belief that some knowledge claims are better founded than others, for he continually equates allegations about "truth" with a naive belief that there exists an absolute reality with which a "true" interpretation must fully correspond. Had Wise been less preoccupied with straw men, he might have more quickly taken up the task he admits, in an uncharacteristic understatement, he has left for his future work: "we'll need to learn more about the logic of explanation [and] about what comprises a good explanation and what is a poor one" (p. 53).

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EDWIN T. LAYTON, JR. The Revolt of the Engineers: Social Responsibility and the American Engineering Profession. Cleveland: Press of Case Western Reserve University. 1971. Pp. xiv, 286. \$9.95.

If one were to ask that mythical figure, the typical Ph.D. in American history, what he knows about the social attitudes of American engineers, such a person would probably cite Veblen's The Engineers and the Price System and might conceivably refer to that curious phenomenon of the Great Depression, technocracy. Indeed, Professor Layton tells us that as a student he became interested in the subject of this book by reading Veblen. But his researches have revealed a story more extensive and considerably more complex than Veblen or Howard Scott imagined.

In part this book is in the tradition of recent analyses of civil engineers by Calhoun and Merritt, but it goes considerably beyond them. While Calhoun and Merritt were chiefly interested in the development of professionalism among civil engineers, Layton is more interested in the connections of these engineering organizations with more general American social thought and movements. He begins by analyzing the internal history of the major engineering societies, pointing out the conflicts between those who conceived engineers as independent professionals and those who saw them as an integral part of burgeoning business bureaucracies. Yet most engineers, he argues, shared a common ideology that held that engineers had absorbed the rationality and objectivity of their discipline and had the ability to apply it to other areas of life. In a sense F. W. Taylor and the scientific management movement were the logical culmination of this engineering ideology. Progressive engineers took up these ideas and argued that engineers could, by applying rationality and objectivity to social problems, solve most of the ailments that afflicted America in the Progressive era. For more than a decade the progressives fought to unite American engineers behind their credo and to use united engineering societies as an instrument for transforming American society. While the progressives had some first-rate leaders, notably Morris L. Cooke, and appeared to have converted Herbert Hoover to their cause, their larger hopes were dashed. Engineering societies proved to be inadequate reform instruments, and in the twenties these societies returned to "normalcy" under conservative auspices.

This is a first-rate study. Based largely on professional engineering journals it suggests some of the riches hitherto hidden in such sources. Layton has also used some manuscripts, notably those of Taylor, Cooke, and Hoover, and his study offers new insights into their respective careers. The analysis is always sound and often shrewd; the writing is clear and craftsmanlike. All in all the book richly deserved the Dexter Prize it received from the Society for the History of Technology. There is one additional bonus for readers of this journal: a patient reading of the experience of engineers with their societies may offer some helpful clues to historians brooding over future directions for the American Historical Association.

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MARY C. HENDERSON. The City and the Theatre: New York Playhouses from Bowling Green to Times Square. Clifton, N.J.: James T. White and Company. 1973. Pp. xiv, 323. \$14.95. This book is the first attempt to tell the history of theater building in New York City. Or more particularly of the commercial or Broadway theater, as Mrs. Henderson omits reference to buildings elsewhere in the city. She provides information on construction date, the architect's name when known, a brief survey of productions and something on the building's demise. Between the brief descriptions we are given potted history of the city.

Unfortunately she does not tell us more about the theaters, and the potted history intrudes. Somehow she got off on the wrong foot by not confining her story to the theater. The occasional fact or date as background would have proven sufficient, thus leaving space to discuss the buildings and their associations.

We long for more information. For example, George Washington enjoyed going to the John Street Theater in his brief stay in the city in 1789 and 1790, but we are told nothing of his interest. In describing the Park Theater, the city's most important in the first decades of the last century, she fails to mention that it left notice of its passage in the name of Theater Alley, a rare New York alley running between Ann and Beekman streets. Minor notes these associations may be but they round out the picture. Just as it would be nice to be told that G. Albert Lansburgh, architect of the Martin Beck, was associated with Arthur Brown, Jr., in the design of the San Francisco Opera House.

The large curiosity is absent, essential in a work of this kind. It explains why she has made no use of the diary of George Templeton Strong. Nor is there any mention of the redoubtable congressman Sol Bloom, the builder of several 42nd Street theaters that are now movie-grind houses. Nor did Mrs. Henderson make use of the records of the city's building department that might have turned up a few odd facts such as that the Billy Rose Theater is in a structure built for an indoor tennis club. Even a page or two of description of the "Rialto," as 14th Street south of Union Square was known from 1870 to 1890, would have been entertaining.

More serious is her failure to dwell on theater design. We would like to know the chief characteristics of the nineteenth-century theater, such as its shallow stage, so different from eighteenth-century theaters. Also we should be told to what degree the commercial playhouse served as ornament to the city. To a certain degree, despite the limitation of the New York grid plan, they contributed to the urban panoply. Sculpture and mural decora-

tion were often present in quantity. For example, the New Amsterdam, the great Ziegfield's theater, now a movie-grind house, still has a foyer decorated by R. Hinton Ferry and a proscenium arch mural by Robert Elum and A. B. Wenzel. The sculpture on the front, by George Gray Barnard, has unfortunately long since gone. The Broadway theater did adorn the city if not in quite the spectacular terms of the movie palace of the 1920s.

Mrs. Henderson has no doubt pioneered in the subject. But those who come after need not worry. There is plenty left to explore in this neglected aspect of urban history.

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Museum of the City of New York

WILLIAM T. GENEROUS, JR. Swords and Scales: The Development of the Uniform Codε of Military Justice. (National University Publications, Series in American Studies.) Port Washington, N.Y.: Kennikat Press. 1973. Pp. x, 25c. \$12.50.

During the last few years the American military has fallen on hard times. In popular esteem, in congressional support, and, alas, even on the gridiron the hapless Army team, at least, has suffered a diminution in prestige.

William T. Generous deals with one cause for the loss of respect for the armed forces—their failure to keep the administration of criminal justice up to date with the corresponding changes in civilian criminal law. He tells us that the outmoded British Articles of War were still operative in 1917 as they had been since the American Revolution. The author traces the efforts of General Samuel T. Ansell for change during World War I, details the creation of the Uniform Code of Military Justice in 1950, and completes his discussion with a description of the current efforts to amend the system.

Though the problem is not stated so succinctly, it becomes readily apparent to the reader that the chief obstacle to reform has been a failure on both sides of the struggle to realize that the main weakness of the military justice system has been one of definition—or, rather, of distinction between military discipline and criminal justice. The inability of those responsible for the creation of the code and for its implementation to recognize this distinction has resulted in the perpetuation of a system of men rather than of laws. The good of the service is placed above the ends of justice. As the author points out on page 122, "in order to overcome . . . liabilities [of the Uniform

Code], some proposed legislative overhaul, others simply disregarded the more onerous parts of the law, and most began increasingly to rely on non- and extra-judicial means of handling service delinquency problems. . . . As late as 1963, the Navy JAG told his lawyers that the laws were not the source of [non-judicial punishment], but that the authority was 'inherent in the disciplinary powers of the CO.'" Lest the story seem too bleak, it should be pointed out that there are participants like Homer Ferguson and Sam Ervin who, when joined with Samuel Ansell, prevent the tale from becoming a total victory of "black hats" over "white hats."

From the detailed footnotes and the extensive bibliography it is evident that Professor Generous has researched his topic well. His interesting narrative of the history of the Uniform Code of Military Justice and of the Army, Navy, and Air Force Manuals, which implement it, should be valuable both to those whose interest is academic and to those whose contact with a court-martial has been first hand.

It is perhaps the author's absorption in his subject that has caused, at least in my opinion, a formal weakness in the work. Generous presumes at times that his readers are as familiar with technical and legal terms as he is. If those of us who have more than a passing acquaintance with law need our memories prodded with an explanatory adjective or phrase, how much more the general reader.

CHARLES A. LEONARD
Western Illinois University

C. ROBERT KEMBLE. The Image of the Army Officer in America: Background for Current Views. (Contributions in Military History, number 5.) Westport, Conn.: Greenwood Press. 1973. Pp. xi, 289. \$10.75.

This volume, by a retired colonel who was formerly the director of American studies at West Point, is a significant contribution to social and intellectual history. Covering the period from the end of the Revolution to the early twentieth century, it is the first of a proposed two-part "cultural-historical study of America's conceptions of, and attitudes toward, the military professional." While describing the changing ways in which the nation has viewed the profession, the author attempts to identify "the basic, though often contrary impulses" that lay behind the successive shifts in perception. Here is no simplistic juxtaposition of militarism with antimilitarism but a careful

study of the subtle transformations by which the eighteenth-century officer-and-gentleman patrician image of the European tradition was Americanized only to be attacked by egalitarian Jacksonians. But even while under assault as privileged aristocrats, officers as scientist-technicians serving society constructively won respect and support. Significantly, the military academy established a department of engineering in 1812 but no department of military tactics until 1858.

After the Civil War, as civilian universities took over the scientific-technical role, Army officers turned increasingly toward their military calling, becoming by the end of the century true professionals. As the author puts it, they were thereafter marked more by their calling and less by their class. But the values of the Gilded Age and the coming of Darwinian evolution challenged this new professionalism. Today's critics who scent abuse in a military-industrial complex might be surprised at the vehemence of the attack by big business spokesmen on Army officers. The "fittest" were seen not as the strongest militarily but as the productively efficient businessmen. It is ironic that the use of Army troops to protect the property of the business interests in periods of industrial unrest only served to alienate the ranks of labor and reinforce the image of "Prussian" officers whose very professionalism was held against them. Despite these critics, the literary image of the American officer continued to be largely favorable, at least until the late 1880s, depicting a class of gentleman leaders of the highest ideals, notable for their strict honor, bravery, and patience in poverty and exile.

In short, this study traces a remarkably complex pattern of changing attitudes toward Army officers over more than a century. The author has culled both primary sources and secondary literature with a discrimination and fair-mindedness reflected in meticulous citations and a twenty-five-page bibliography. His next volume will be welcome.

I. B. HOLLEY, JR. Duke University

ROBERT C. NESBIT. Wisconsin: A History. (State Historical Society of Wisconsin.) [Madison:] University of Wisconsin Press. 1973. Pp. xiv, 573, 88 plates. \$12.50.

The University of Wisconsin Press evidently intended this book primarily for text use in college and university courses on Wisconsin history. It can also serve the advanced second-

ary school student and will appeal to the adult Wisconsinite seeking a scholarly survey of his state's experience. The author, a native of the state of Washington, is a professor of history at the University of Wisconsin, Madison.

Nesbit's volume should immediately supersede William F. Raney's standard history of Wisconsin (published in 1940 and slightly modified for reissue in 1965). Raney's book, whose systematic presentation of the best modern scholarship and splendid bibliographical notes offset a forbidding format and lackluster style, has long outlived its usefulness. Incorporation of a new generation's research into Larry Gara's A Short History of Wisconsin (1962) failed to break Raney's hold on the textbook market, for Gara's work was cursory and in appearance and content less suited to a mature audience. Some general readers may continue to prefer the extensive 1965 revision of H. Russell Austin's The Wisconsin Story, The Building of a Vanguard State (1948, 1957) for its sprightly, if journalistic, style and its colorful human interest.

Nesbit has special talents for state history. He skillfully synthesizes monographic and periodical literature and relates it to regional and national perspectives; he rarely confuses antiquarian lore with significant historical data; and he perceives and explains the broader meaning of state and local events. He has not changed the traditional outline of Badger State history, however, nor has he made up through his own research for deficiencies in existing scholarship on such topics as the industrialization and urbanization of the last hundred years. On the other hand, his portrayal of late nineteenth-century reform stirrings, the elder Robert M. La Follette, and the Progressive era is informed by recent revisionist studies, published and unpublished, of his Madison campus colleagues and their students; and his judgments on La Follette and the La Follette tradition, blending skepticism and admiration, are the freshest part of this volume.

One is impressed by the author's balanced and urbane judgments, his wide-ranging knowledge, his crisp style. One is grateful for the succinct, sometimes acid or puckish, annotations to his chapter bibliographies; but the latter suffer from incomplete and inaccurate citations, inconsistent practice, and neglect of some sources (almost no consultation of graduate theses at Milwaukee's two universities). Four appendixes provide convenient election and population data. The thirty-two-page pictorial essay at the end of the book, drawn by George Talbot from

the iconographic collections of the State Historical Society of Wisconsin, is a model of its kind, though some items lose impact from reduction in size.

Generally satisfactory though Nesbit's book is, I wonder whether it is not premature. Completion of a scholarly six-volume history of the state later in this decade should make possible a superior one-volume synthesis of Visconsin history.

FREDERICK I. OLSON
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ROBERT F. HEIZER and ALAN J. ALMOUIST. The Other Californians: Prejudice and Discrimination under Spain, Mexico, and the United States to 1920. Berkeley and Los Angeles: University of California Press. 1971. Pp. viii, 278. \$7.95.

RICHARD B. CRAIG. The Bracero Program: Interest Groups and Foreign Policy. Austin: University of Texas Press. 1971. Pp. xvii, 283. \$7.50.

RODOLFO ACUÑA. Occupied America: The Chicano's Struggle toward Liberation. San Francisco: Canfield Press. 1972. Pp. vi, 282. \$4.50.

From bumper stickers proclaiming "Polish Power" to the lettuce counters of supermarkets, from political caucuses to newscolumns to campuses (according to a recent report in the New York Times, 135 colleges and universities are giving 315 courses in white ethnic studies alone), indications are that ethnic consciousness and assertiveness exert as strong an influence in America today as at any period of our history. Research and teaching in the history of immigation and ethnic relations once again seem to be relevant, popular and, one hopes, intellectually stimulating, creative, and useful to our troubled society. The field is again potentially "profitable" in many ways; one effect is that it has elicited from publishers—as was the case earlier with black studies—an outpouring of books and nonbooks, good, bad, and indifferent. The three volumes I have been asked to review illustrate the disparity of results achieved.

The easiest of the books to dispose of is The Other Californians. Although written, or put together, by two Berkeley anthropologists, there is nothing particularly anthropological in their method, content, or conclusions. Their approach is historical, but we are served up poor, superficial history, indeed. The 203 pages of text (followed by 55 pages that reproduce 19 oddly-assorted documents that somehow capti-

vated the authors' attention) are allocated heavily in favor of the Indians of California one suspects that Heizer and Almquist have done more research, or reading, on themwhile the state's Mexican immigrants, Asians, and blacks receive scanty treatment. Occasionally the reader stumbles across a relatively little-known and interesting nugget of WASPish perfidy, but obviously such a book generally can recount only the best-known incidents of Californian and American discrimination, repression, and racism, for all of which the authors are thoroughly ashamed. This mea culpa adds virtually nothing to our knowledge, and although it may serve to salve two anthropologists' consciences, one feels that certainly somewhere amidst the sprawling complex of research endeavors that it serves, the University of California Press should have been able to come up with a more deserving entry in the "ethnic books derby."

Replete with the jargon of political science research technique is Richard B. Craig's The Bracero Program. Early on, the author announces that "the theoretical framework around which this study is structured is an amalgam of two approaches to the study of the political process: interest-group theory and systems analysis." Despite this, however, he does succeed in presenting a comprehensive and generally comprehensible study of how international and domestic circumstances combined and were manipulated so as to preserve in operation, for eighteen years beyond the end of the Second World War which engendered it, a program that imported a total of four million two hundred thousand Mexican farm laborers into the United States under contracts that benefited agribusiness in the Southwest at the expense of both native and resident-immigrant farmworkers. With some knowledge of the political history of the period, one can rather easily fathom the main storyline of how this feat was accomplished, and also why it came to an end during the green years on the New Frontier of the Great Society. Reverting to considerations of political theory in the concluding parts of his book, Professor Craig, who teaches at Kent State University, concedes the fact that "less than 2 percent of the nation's farmers in five states could for so long dictate policy detrimental to the interests of resident workers, and perhaps to the majority of all United States citizens, was cause for serious concern" to subscribers to the "countervailing powers" theory. Yet "in a comparatively short time," he concludes, "the seemingly unbalanced group milieu

of imported Mexican labor" righted itself, providing vindication for J. K. Galbraith, Seymour M. Lipset, and the other "realists" who championed and popularized the notions of interest-group democracy.

Among those not likely to agree that eighteen years constitutes "a comparatively short time," especially for those on the short end of the stick, is Rodolfo Acuña, a Chicano who heads the Chicano studies department at California State University, Northridge. His book, Occupied America: The Chicano's Struggle Toward Liberation, is the most rewarding of the three reviewed here in that it affords us a textbook with a difference-an excellent introductory survey of the history of a particular minority group that conveys not only scholarship and information, but sincerity, concern, and commitment as well. Despite a few blemishes probably attributable to hasty copyediting, the book should open the eyes of most of its non-Chicano readers to a compellingly different perspective on an important aspect of American history and present-day society that includes such matters as the My Lais of the Mexican War and the occupation that followed the conquest; the growth of self-help mutualistas among captive and immigrant Mexican-Americans; the emergence of labor movements that waged and lost bloody strikes against copper barons in the early 1900s, cantaloupe and pecan growers in the 1920s, agribusiness in general during the Depression decade (violent encounters doggedly reported at the time by Carey McWilliams, whose chronicles of outrage are still labeled "communistic journalism" in some academic circles), and the Di Giorgio Fruit Corporation in the later 1940s (when Congressman Richard M. Nixon lent his name to a phoney House "committee report" that hastened the demise of the strike); the trials of Cesar Chavez and the United Farm Workers union; the changing role of the Church and other traditional institutions, and the increasing influence of factors like urbanization in Chicano history; the decline of "integrationist" sentiment and the rise of separatist groups during the catalytic but frustrating 1960s; to the point where today, according to Acuña, "most activists were disillusioned with the existing system, and many were bidding goodbye to America." Yet, ironically, it may be that Chicano malcontents and spokesmen for the many other Americans slighted by the theory or practice of the "countervailing powers" approach to democracy up to this point in time are performing a highly patriotic service. "The Chicano people seek self-determination," Professor Acuña writes, and "self-awareness in a . . . community that is inner-directed instead of being directed from without . . . the retention of culture; collective liberation instead of individual cooptation . . . control of their political, economic, and social institutions." In this post-Watergate era, millions of victims of our present system of "internal colonialism" might well be impressed with the insights and conclusions thus derived from the study of just one phase of immigration and ethnic relations in American history.

J. JOSEPH HUTHMACHER University of Delaware

ALDEN T. VAUGHAN and GEORGE ATHAN BILLIAS, editors. Perspectives on Early American History: Essays in Honor of Richard B. Morris. New York: Harper and Row. 1973. Pp. ix, 405. \$10.00

It is appropriate that this Festschrift for Richard Brandon Morris reflects in several ways the career of the master. Its contributors deal with a wide range of issues, often suggest new avenues of approach to important subjects, include strong elements of biography, and in general have carefully researched and judiciously stated their theses.

Alden T. Vaughan and Harry M. Ward analyze, respectively, the colonial perspectives of early Virginia and New England historians. Vaughan makes several perceptive comments on the differing purposes and viewpoints of these early writers. The New Englanders, he summarizes, "wrote ecclesiastical history, the Virginians natural history; the Puritans wrote of Canaan, the Anglicans of Eden" (p. 37).

Patricia U. Bonomi departs from the historiographical format of Vaughan and Ward to argue that the middle colonies were an "Embryo of the New Political Order." She contends that ethnic and religious diversity, sectional fragmentation, and the growth of practices we associate with the emergence of modern political parties were part of the middle colonies' scene and contributed significantly "to the formation of our political habits" (p. 65). H. James Henderson, in writing on "The First Party System," examines John Marshall's, Richard Hildreth's, and Charles Beard's views of the first national parties but concentrates on more recent scholarly efforts to analyze early national party growth. Henderson argues briefly that the regional and ideological tensions apparent in the first national congresses had surfaced earlier in the Continental Congresses. He also suggests that historians have assumed an articulation between state and federal party battles that often did not exist.

In essays on twentieth-century perspectives, Philip L. White laments Herbert L. Osgood's careless writing, poor organization, lack of careful definitions, and his commitment to exposition without explicit analysis. In a provocative and critical essay on Carl Becker, Milton M. Klein explores the paradoxes in Becker's intellectual commitments and in his professional career. Richard Morris's article on "The Spacious Empire of Lawrence Henry Gipson," which first appeared in 1967, is reprinted here because it is still stimulating reading.

In the last and miscellaneous section of essays Emil Oberholzer examines selected aspects of Puritan historiography with his point of departure being the ideas and influence of Perry Miller. Recent historiography on the New England town is analyzed in an intelligent and useful way by John J. Waters. Herbert Alan Johnson does much more than present a historiographical interpretation of American legal history. Johnson urges legal historians to follow the approaches suggested by Daniel J. Boorstin and Stanley N. Katz, "to strike out for a broadly conceived legal history that seeks to explain colonial law . . . as it was conceived in the seventeenth and eighteenth centuries" (p. 277). In what may be the most useful and insightful essay in this collection, George Athan Billias analyzes the loyalists in American historiography. Scholars interested in pursuing loyalists and loyalism will find here several excellent suggestions for approaching these topics in imaginative ways. Mary-Jo Kline closes with a brief review of Morris's writings to which is appended Richard Morris's bibliography.

MARVIN R. ZAHNISER
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GEORGE L. SMITH. Religion and Trade in New Netherland: Dutch Origins and American Development. Ithaca: Cornell University Press. 1973. Pp. xiii, 266. \$12.50.

Mr. Smith demonstrates at length and convincingly that the Dutch Reformed Church never had a great deal of influence in the founding of New Netherlands. When the fanatic governor Peter Stuyvesant tried to drive Lutherans, Jews, and Quakers out of New

Netherlands, the West India Company rebuked him and ordered "every one to have his own belief, as long as he behaves quietly and legally. . . . As the government of this city [Amsterdam] has always practised this maxim of moderation . . . we do not doubt, that your Province too would be benefitted by it" (p. 230).

So religious pluralism was established in the Middle Colonies both in theory and practice before New Netherlands became New York. Multinational, multilingual, tolerant in religion, oriented toward trade, New Amsterdam reflected the social patterns of its Old World namesake. These patterns anticipated the mood of England's post-Restoration imperialists and were reinforced by Richard Nicholls after 1665. The historical significance of the Dutch contribution to the religious pluralism in the Middle Colonies, however, has been largely ignored. Within the last ten years general books by Sydney Ahlstrom, W. F. Craven, and Edwin S. Gaustad have all failed even to mention it. It is therefore fortunate that Smith has so carefully and thoroughly made the point. This book, along with those by Simon Hart (1959), Thomas J. Condon (1968), and Van Cleaf Bachman (1969), provides a welcome stimulous to the study of the Dutch period of early American history.

Smith devotes more than half of his book to the rise of religious pluralism in Amsterdam. He perceives, too simply, that story to have been a struggle between gold-oriented merchants and God-oriented Calvinists. The argument is familiar and superficial. Smith's own feelings are evident throughout, although he tries to be fair: "It is far too easy to wax cynical and fall back upon epithets such as geldwolven to describe the great heren" (p. 187). In his second hundred pages (it is not a large book), Smith turns to events in America. His hero is Governor Stuyvesant, champion of predikanten and the Dutch Reformed Church. Under Stuyvesant's rule Smith writes admiringly that "the 'trading-post mentality' of earlier days was gone and had been replaced by an eagerness to sink deep and permanent roots for a true colonial society" (p. 189).

Whatever one thinks of Smith's eager partisanship, the central thrust of his account is well documented and its meaning for the history of the Middle Colonies seems uncontestable.

MICHAEL G. HALL University of Texas, Austin HUGH T. LEFLER and WILLIAM S. POWELL. Colonial North Carolina: A History. (A History of the American Colonies.) New York: Charles Scribner's Sons. 1973. Pp. xvi, 318. \$10.00.

There has long been a need for a good, solid, interpretative history of colonial North Carolina. Anyone wanting a comprehensive knowledge of the state's early history has had to consult the multivolume works of Herbert L. Osgood, Lawrence H. Gipson, or Charles M. Andrews and read the scattered chapters before putting it together for themselves. In an attempt to fulfill the need of a comprehensive history, Hugh T. Lefler and William S. Powell have written the volume on North Carolina for the series, A History of the American Colonies. This book covers the period from early settlement down to the outbreak of the American Revolution.

For the serious reader, the book suffers from a number of omissions. There is no apparent theme around which the authors have organized their thoughts. There is the suggestion of a theme but not until the epilogue. The book is not very interpretive with the exception of a sweeping general concluding sentence for each chapter. For example, the chapter on the social makeup of the colony is concluded by the sweeping and undocumented statement that "the revolutionary struggle, led in large measure by the upper class, was a leveling influence that began to wipe out class differences, which had become rather deeply entrenched during the Colonial period." Culpepper's rebellion is viewed as a part of a colonial-wide movement, ignoring any connection to the local problems of internal social and political instability resulting from the colony's early government-or lack of it.

While there is a well-written selective annotated bibliography, there are no footnotes. Direct quotations are not identified. This definitely destroys the value of the book for the serious reader.

Several chapters are extremely well written, but there is a definite unevenness in the writing style. The best chapter is the one concerning immigration into the colony. The reader immediately grasps the frontier nature and diversity of North Carolina's early society.

The book serves as a good introduction to the study of North Carolina's colonial history, but it adds nothing new to the knowledge of the period. It does pull together the myriad of events that made up the period. The authors would have done a greater service, however, if they had untangled the complex evolution of North Carolina's government.

I doubt if those with knowledge of North Carolina's history have any need to consult this work except for the convenience of refreshing their memories.

EDWARD S. PERZEL
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Charlotte

ERNEST TRICE THOMPSON. Presbyterians in the South. Volume 1, 1607–1861; volume 2, 1861–1890; volume 3, 1890–1972. Richmond, Va.: John Knox Press. 1963; 1973; 1973. Pp. 629; 528; 636. \$15.00 each.

Treatments of religion in the South have often been narrowly promotional or have concentrated too exclusively on selected faults distinctive of the region. Here is a history—based throughout on primary sources—from earliest days to the present of one of the three major Protestant denominations in the South that is well rounded and fair. While never disloyal to its sectional heritage, it is distinctly a voice of the New South, which quietly alters the whole perspective in which Southern Presbyterians have traditionally viewed their church. The author was himself a prominent leader in bringing about this ecclesiastical change.

The story portrays the gradual development of a distinctively "Southern" Presbyterian viewpoint and ethos, the heightening of this in the Civil War and its long aftermath, and, then, in recent decades, the steady movement back into the mainstream of American life—a kind of systole and diastole—paralleling somewhat tardily similar developments in Southern history as a whole.

Though the study attempts encyclopedic allinclusiveness, certain distinctive themes stand out with ever-recurring emphasis. Among these themes is the ultimate modification of a "rigid Calvinism," which unfortunately is not here analyzed in depth. This type of hyper-Calvinism is seen as the chief cause of the extensive Presbyterian losses in the Kentucky revival and as defensively forcing the more serious Old School-New School Presbyterian disruption of 1837. In the case of the latter, the author skillfully interrelates important differences over slavery and ecclesiastical structure as factors also contributing to division. When Southern Presbyterians created a separate "Presbyterian Church in the Confederate States of America" at the outbreak of the Civil War, their inherited Old School strict Calvinism became congealed for nearly a century, but recent decades have seen extensive modification of this along lines similar to those followed somewhat earlier by the parent national church.

Slavery and race relations are treated frankly and constructively. Religious instruction of the slaves, early started and long advocated, was increased in response to abolitionism. The author views gradual emancipation much more favorably than radical abolitionism. In spite of more favorable interpretation by some recent scholars, the author finds no merits in postwar Reconstruction, which is seen essentially as exploitation by carpetbaggers and scalawags with the help of untutored Negroes. But the author is entirely on the side of those in recent times who demand greater justice for the Negro and the ending of segregation and discrimination.

One of the most important expressions of the author's revisionism is his repeated criticism of the traditional Southern Presbyterian theory of the "spirituality" of the church. This doctrine, developed shortly before the Civil War to exclude antislavery agitation from the church, relegated all social issues to the civil sphere, leaving the church purely "spiritual." After emancipation the doctrine was retained to exclude from church discussion agitation for Negro rights. But the church's more progressive elements have, with increasing success, been demanding that the church speak out directly on justice for the black and on growing problems of industrialization and urbanization.

In spite of vigorous but numerically decreasing opposition from within, the church in recent decades has moved toward closer cooperation with national and world Christian bodies and is currently engaged in merger negotiations with its parent body, the United Presbyterian Church.

One might perhaps wish that the author had made less of an effort to "tell all" and had concentrated in even greater depth on the crucial central forces that he sets forth so clearly and knowledgeably. But it is a magnum opus—large in outlook, broad in sympathy, full of human interest—for which author and denomination are to be congratulated.

LEFFERTS A. LOETSCHER
Princeton Theological Seminary

MELVIN B. ENDY, JR. William Penn and Early Quakerism. [Princeton:] Princeton University Press. 1973. Pp. viii, 410. \$17.50.

This book was conceived, Professor Endy tells us, as an inquiry into the effect of Quaker convictions in the forming of Pennsylvania. Finding, however, that neither the more than forty biographers of Pennsylvania's founder nor recent monographs on his social ideas and political activities have done justice to their subject's religious thought, Endy developed his study into an intellectual, specifically theological, biography of William Penn. The approach is thematic and contextual: Endy establishes the dominant motifs of Penn's thought, relates them to the ideas and impulses of the Quaker movement, and sets the whole in the context of the English Reformation. The result is far more than an addition to Pennsylvania history—finally reduced to a mere tailpiece; it is a valuable account, long needed and skillfully executed, not only of Penn's mind but of the mind—the many minds—of radical Protestantism in the England of the later Stuarts.

By the time of Penn's conversion to Quakerism the Commonwealth had failed, the nation had been officially confirmed in Anglicanism, and the nonconforming left had splintered. In a superb chapter Endy surveys the tangled bank of sectarian radicalism, not only showing how to tell a Ranter from a Seeker from a Quaker-each in several varieties-but locating for these and other fringe groups a common ancestry in the high-keyed religiosity that had powered the "spiritual brotherhood" (Endy draws here on William Haller) early in the century. Endy calls attention to the radical duality of nature and grace in the thought of the spiritualists; in this respect he emphasizes the differences between Quakerism-with its dynamic evangelicalism, its inner-light individualism, its universalism, and its rejection of the formulas of conditional covenant and preparation for salvation—and the more rationalistic forms of puritanism, thus proposing to revise the views of such scholars as Geoffrey Nuttall and Hugh Barbour in whose writings the puritan origins and relations of the Quakers are stressed.

Limits of space preclude extended comment on Endy's interpretation, although it may be worth remarking that his definition of "puritanism" leaves something to be desired in point of clarity and that in several respects Penn seems rather more "puritan" than Endy is prepared to admit. Such reservations aside, it is enough to observe that this learned and penetrating study fills a hole in our knowledge of Penn and both extends and amends our understanding of the Radical Reformation. In addition, it amply fulfills the author's hope that the work "can serve as a guide to Penn sources for students of colonial history, English religious thought, and Quakerism" (p. 383).

MICHAEL MCGIFFERT
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and Culture

The Diary of Samuel Sewall, 1674–1729. Volume 1, 1674–1708; volume 2, 1709–1729. Newly edited from the manuscript at the Massachusetts Historical Society by M. HALSEY THOMAS. New York: Farrar, Straus and Giroux. 1973. Pp. xxxvii, 612; xii, 614–1254. \$30.00 the set.

The extensive journal of Samuel Sewall, first published by the Massachusetts Historical Society almost a century ago, has long provided an invaluable source of data for colonial scholars. Although Sewall has been compared with the English diarist Samuel Pepys, the literary quality of Sewall's writing never reaches that of Pepys. Nevertheless, Sewall's journal offers an extremely entertaining, informative, and intimate description of the man and his times. As a Harvard graduate, wealthy merchant-landowner, influential Massachusetts magistrate, and a member of Boston's prestigious South Church, Sewall was a part of the Bay Colony's Puritan aristocracy, yet his entries from 1674 to 1729 describe both momentous and commonplace events. Thus, he recorded such prominent episodes as King Philip's War, the Salem Witch Trials, the political squabbles between the Mathers and Governor Joseph Dudley, and at the same time such mundane matters as planting trees, the arrival of spring swallows, funerals, and church services. The entries also reveal much of Sewall's personality. He was a pious, selfcritical, opinionated individual who was constantly aware of the transitory nature of life and the consequent urgency of seeking God's protection. He was theologically conservative and intolerant of dissenters, yet humanist qualities were reflected in his opposition to slavery and concern for dispossessed Indians. (Pride in his American environment was apparent in his rebuke to one woman who audaciously called New England "filthy.") Overall, the journal presents a fascinating portrait of New England society during the transitional period from King Philip's War to the eve of the Great Awakening.

The present two-volume edition of the Sewall diary is a first-rate work. After many years of careful research, Mr. Thomas has succeeded

in eclipsing the Massachusetts Historical Society's expurgated and circumscribed version. Besides the completeness of this transcription, Thomas's work also presents well-documented and comprehensive annotations that amplify significant items from the text. Thomas elaborates upon many of Sewall's hundreds of identifications of individuals and descriptions of events; he offers a provocative analysis of the scanty notations of Sewall's judicial role in the witchcraft trials, and he uses Sewall's reactions to the losses of most of his fourteen children and his beloved wife, Hannah, to exemplify the spiritual manner in which Puritans accepted death. Supplementing the text and annotations, the volumes contain an explanatory preface, several relevant illustrations, and appendixes that include an updated genealogical section, a record of Sewall's published writings, a listing of imprints made during his management of the Boston printing press, and a reprinting of Sewall's notable antislave tract, The Selling of Joseph. Individuals examining colonial New England should be deeply indebted to Mr. Thomas for his superb reproduction of this venerable manuscript.

SHELDON S. COHEN Loyola University, Chicago

JACOB JUDD and IRWIN H. POLISHOOK, editors. Aspects of Early New York Society and Politics. Tarrytown, N.Y.: Sleepy Hollow Restorations. 1974. Pp. viii, 150. \$7.50.

In this slim volume are four formal papers, four brief comments about them (introductions by the editors and critiques by Lawrence Leder and Jackson Turner Main), and a sparkling address by Richard Morris, "The Revolution Comes to John Jay." Together they constitute the proceedings, amplified by footnotes, portraits, maps, and even a brief further readings list, of a conference sponsored by Sleepy Hollow Restorations and the Institute of Early American History and Culture at Tarrytown, New York, in October 1971. The book's editors organized the program on that occasion.

Chronologically the papers range from Thomas Archdeacon's study, "The Age of Leisler—New York City, 1689–1710," to Edwin Burrows's, "Military Experience and the Origins of Federalism and Antifederalism," with Patricia Bonomi's essay, "Local Government in Colonial New York," analyzing the board of trustees of Kingston from 1711 to 1776. More historiographical in content is Milton Klein's

"New York in the American Colonies: A New Look." Klein complains that historians have neglected early New York despite the relevance of its heterogeneous character to modern America. He blames fondness for the "homogeneous, rural Arcadia" of colonial Virginia and Massachusetts for this "historical amnesia."

Archdeacon identifies the polyglot character of New York City as the key to Leisler's movement. His analysis of ethnic background, age, scale of wealth, and occupation of the electorate of 1701 suggests ethnicity as the most important factor in voting behavior. Leislerianism, bearing "an unmistakable Dutch aura," revealed the resentments of those unable to adjust to "the Anglicization of the city."

Bonomi's paper bears even more directly upon Klein's theme by challenging charges that the "aristocratic" politics of colonial New York were not relevant to the mainstream of American development. Of Kingston's 148 freeholders in 1728, sixty-one served on the township board of trustees at some time. In a brief comparison with local government in England, Kingston is judged superior in vigor and good repute among its citizens as well as broad-based participation. In New York, then, as well as in the New England town and the Virginia parish, a "base for republicanism" was in formation.

The briefer Burrows study offers another statistical analysis. The 103 candidates for New York State's 1788 convention on the Federal Constitution are the subjects, and Burrows finds a "pronounced correlation between Antifederalism and militia experience . . . [and] between Federalism and service in the Continental Army."

If Klein's complaint has merit, these proceedings constitute an interesting if tentative promise of amelioration.

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RICHARD WARCH. School of the Prophets: Yale College, 1701–1740. (The Yale Scene. University Series, 2.) New Haven: Yale University Press. 1973. Pp. xii, 339. \$15.00.

The eighteenth-century history of Yale College has been served well in recent years by biographies of Thomas Clap and Ezra Stiles. The present volume provides an excellent, even-handed interpretation of the years before Clap's turbulent presidency and the Great Awakening, which was responsible for so many of his and Yale's difficulties. Even more important, as much of the story has been told before, Richard

Warch places Yale's development within the social and ecclesiastical context of western New England.

Warch acknowledges his debts to earlier historians of the college, and, when he disagrees with them, he does so with gentle firmness. Clap's politically inspired tale of the collegiate school having been created by Connecticut ministers before the charter is demolished. The influence of the Mathers and other Massachusetts men on Yale is minimized without being ignored. The college was founded in a larger reformed tradition and did not derive from a single source.

The central point in the study is, as might be expected, the apostasy of Rector Timothy Cutler in 1722, only shortly after the school had finally settled in New Haven. The appearance of Anglicanism in Connecticut placed Yale and the whole presbyterial-Congregational establishment on the defensive, the more so as Samuel Johnson, a Yale man and apostate, remained nearby. The result was a drawing back into ancient orthodoxy so that the rectorship of Elisha Williams, which finally brought stability to the college, was one that emphasized adherence to the strictest standards of the Westminster Confession.

While Warch does not find early development of true Arminianism in Connecticut (except for a growing number of Anglicans), he argues that Yale unwittingly contributed to an emphasis on works and man's role in his own salvation. The ancient covenant theology of William Ames and Johan Wollebius existed in tension in the curriculum from the twenties onward with philosophical ethics and rational metaphysics. In comparison with Harvard's growing latitudinarianism, however, Yale remained orthodox. Her clerical graduates divided almost evenly between Old and New Lights during the Great Awakening but provided much of the leadership for the New.

Warch is no academic filiopietist and makes no undue claims for his subject. Education at Yale was English in spirit but not in quality. The college in New Haven was not a match for Edinburgh, the English universities, or the Dissenting academies, and her students learned Locke and Newton not from reading the originals but at second hand from tutors.

Warch's study fills a major gap in the history of colonial education and contributes significantly to the history of latter-day Puritanism in the critical years immediately preceding the Great Awakening. Even more significantly his analysis of the internal strains in the educational process give promise of a larger work on eighteenth-century higher education.

GEORGE F. FRICK University of Delaware

STEPHEN E. PATTERSON. Political Parties in Revolutionary Massachusetts. [Madison:] University of Wisconsin Press. 1973. Pp. ix, 299. \$12.50.

Political dissension, to the point of taking shape as political parties, persisted in Revolutionary Massachusetts, despite the rhetoric of the political leaders in appealing to unity. The internal revolution was a more pressing reality than the Revolution itself.

Much has been written on the emergent party system in Massachusetts, perhaps the most politically oriented of the thirteen states; and the theory of democracy versus the Revolution is, of course, old hat. But Professor Patterson superbly delineates the powerful ground swell for democratic revolution in Massachusetts and yet the inability of the people en masse to succeed in party formation and in bloc domination of the legislature and the constitutional conventions. The radical tone of the resolutions of early county conventions reflected a dissipated energy. The author has digested the interpretations of numerous other studies on Massachusetts politics and republicanism during the Revolutionary era. He suggests, however, that republicanism and appeals for principles of the Revolution were not very sincere but rather a means of settling who should rule at home. Coastal conservatives saw in the Revolutionary ideology the opportunity to thwart the demands for reform from a backcountry and rural constituency.

Patterson is more concerned with the external responses and protests of partisan towns to key issues than with probing in depth their voting behavioral patterns, which several other writers have tried to do and which is difficult at best on the legislative level because of the lack of records of roll call votes during the provincial congresses. He has constructed, however, the legislative voting records of the towns for the period of 1757-64, denoting the stand on selected issues according to "Court" and "Country" party towns. Other tabulation is useful in identifying personnel in the legislature and constitutional conventions, their other office holding, representation by towns, petitions, classification of towns according to their reaction to the Constitution of 1780, and the reaction of town responses to leading issues.

The treatment in the early chapters of the

transition from the corporatism of early eighteenth-century political life in the towns to an intertown political awareness is brilliant. The colonial historian will particularly find of interest the discussion on the intellectual and constitutional sources of partisanship in eighteenth-century Massachusetts.

Although Massachusetts politics during the Revolution retained to a large degree the character of "ins" versus "outs," definite political issues appeared that contributed toward a party alignment in the modern sense. The growth of a propertied working class, evolution of the commercial classes, and political corruption all aided in changing the "Country" party into a party of internal revolution. Patterson traces the rise and decline of a popular party that seemed to take hold at the beginning of the Revolution in the county conventions and provincial congresses, only to become divided by the time of the constitutional conventions of 1778-80 because of sectional, economic, social, and ideological differences. The movement in over one hundred Massachusetts towns for bringing government closer to the people failed because of insufficient organization and ineffectual leadership. The state's ruling groups, the Adamses and the Hancockites, for different reasons, and the eastern conservatives had their way in turning the clock back to a corporatist society. But animosity engendered over the issues-including paper money, the courts, representation, militia organization, and the kind of constitution-left scars. If the counterrevolutionaries seemed to prevail in establishing a new unity and consensus there was a deepseated political discord; an antipartisan theory belied a partisan reality, which paved the way for a more definite political structure during the late 1780s and the Federal era.

This study will be very useful to students and teachers alike in the understanding of Revolutionary politics in Massachusetts. It is well written and researched substantially in the legislative and convention records and in the correspondence of leading figures. But since the book deals primarily with local versus central control, agrarian versus mercantile interests, east versus west, democracy versus the Revolution, or simply liberty versus authority, there could have been wider use of town records and a greater relation to the war effort itself in its effect on politics. Only rarely in Patterson's work does the Revolution intrude on the lives of the people of Massachusetts. Such undoubtedly was the case as Patterson would have it. But it does raise the question of a neglected area in the internal history of the Revolution.

HARRY M. WARD
University of Kichmond

ROBERT MCCLUER CALHOON. The Loyalists in Revolutionary America, 1760-1781. (The Founding of the American Republic.) New York: Harcourt Brace Jovanovich. 1973. Pp. xviii, 580. \$17.50.

Loyalism was for a long time a dreary subject. Aside from a few biographies and Moses Coit Tyler's fine chapters on loyalist writing, the histories were no more than biographical scraps strung together with apologetics. In the past decade or so, historians began to see that to dismiss or condemn the Tories was to limit understanding of the Revolution. The change is marked by William H. Nelson's American Tory, published in 1962. Soon after, a second and livelier impetus was given to the field, albeit indirectly, by the major re-examination of the Revolution that began in the mid-1960s. If, as Bernard Bailyn and others argued, the origins of the Revolution are located in a body of libertarian beliefs radically reshaped by political experience, the immunity of large numbers of men to that ideological transformation becomes a significant problem. While loyalism as a central subject has not yet attracted so many or so gifted scholars as has the movement toward revolution, numbers of monographs have now been written and the field is well established.

Robert M. Calhoon has undertaken to summarize and to synthesize this new body of loyalist scholarship. His summary is thorough and industrious; his failure to achieve a satisfactory synthesis can be ascribed in part to the field itself. Two sorts of problems have interested scholars, and they bear no logical relation to each other. One, which reflects the preoccupations of the ideological school, is the study of loyalist assumptions, attitudes, and political roles. The other, which picks up the newer theme of the history of the inarticulate, is an effort to find out who the loyalist ranks were and what happened to them. It would be difficult to put these two kinds of research together, but Calhoon's attempt suggests that what is impeding even preliminary cohesion is a lingering residue of patriotic bias against those who opposed independence.

In his preface Calhoon says that he has sought "to make the loyalists intelligible and comprehensible." Such a statement shows that like almost every historian of loyalism before him, Calhoon assumes that it was normal to be for the Revolution and therefore abnormal-unintelligible and incomprehensible-to be against it. Thus the loyalists must be polar opposites of the revolutionaries. Following the now-dominant argument about the causative force of radical ideas, Calhoon sees loyalist ideology as its mirror image. The model is a dynamic one, and thus Calhoon posits three developmental stages: beginning in 1765, the enunciation of principle; in 1767-69 and again in 1774-75, the search for accommodation; finally, late in 1774 through early 1776, an appeal to doctrine. A series of biographical sketches illustrates each stage, but the stages are not as clear-cut as Calhoon would have them. The first and third-principle and doctrine-blur together; the men of the second stage—the accommodators—were primarily concerned not with ideas but with political arrangements. Many of the sketches are admirable but change does not emerge from them. What they do reveal, rather, is how firmly the men described held in 1776 to the political beliefs that prevailed throughout the colonies in 1765. The developmental model is clearly inapplicable to loyalist ideology.

In much the same mechanical way, Calhoon searched for an ideological point where the loyalists directly opposed the revolutionaries. He found it in what he describes as the loyalists' "total antipathy to popular political activity and expressions of discontent." If this is truly the distinguishing mark of a Tory, then the patriots would have had to smile upon something resembling participatory democracy. The political public, to be sure, expanded during the Revolutionary era as it had through the century, though now in crisis at a more rapid rate. But patriot leaders were not modern populists, and the Revolutionary expansion of the political public did not reverse eighteenth-century elitism. The United States was founded as a republic and did not begin to become a democracy until at least two generations later.

This insistence upon an antithetical pattern confuses Calhoon's discussion of loyalist ideology and cannot be maintained at all in the second half of his book, whose subject is the "opponents or victims of the Revolution." Here all effort at synthesis breaks down. There is a chapter on Barnstable, Massachusetts, for example, a town where half the inhabitants voted against independence. Calhoon explains this split by saying there were "many in the town who resented the [Otis] family's wealth and success, distrusted James, Jr.'s past identification with

the Boston radicals, and in a typical eighteenthcentury manner coalesced instinctively against those in power." If such behavior is typical and instinctive, why did it only evidence itself in Barnstable?

In making the recent scholarship easily accessible, Calhoon has served the history of the loyalists well, but until historians move beyond the polarization that sets conservatism and orthodoxy against enlightenment and progress, the accumulation of new materials will not deepen our understanding of why so many men, almost indistinguishable from their fellows before 1776, argued and fought against independence.

BEATRICE K. HOFSTADTER New York City

K. G. DAVIES, editor. Documents of the American Revolution, 1770–1783 (Colonial Office Series). Volume 1, Calendar, 1770–1771; volume 2, Transcripts, 1770; volume 3, Transcripts, 1771. New York: Irish University Press. 1973. Pp. 523; 331; 302. \$39.00; \$32.50; \$32.50.

These are the first three volumes to appear in a projected twenty-volume series designed to draw together the surviving Colonial Office correspondence respecting North America during the American Revolution. The subject is imperial administration-from the correspondence, broadly considered, of the secretary of state for America, the editor attempts to recreate "the situation in which the administrator in Whitehall was placed." For the years 1770 and 1771, 2,984 documents (including 1,229 enclosures) are calendared in volume 1; and of these, 143 and 138 are fully transcribed in volumes 2 and 3 respectively. The editorial work, of selecting and providing physical descriptions of documents in the calendar and accurate transcripts for volumes 2 and 3, is well done.

The core of the work is the American secretary's letters to and from the colonial governors and the commander in chief, plus the Indian superintendents, surveyors, and other lesser officials in America; but it also embraces letters between the American department and other departments in London: Admiralty, Treasury, Ordnance, War Office, and other secretaries of state. In volume 1 the editor has also provided useful summaries of the distribution of this material in the Public Record Office, tables of the various Colonial Office series and volumes covered, lists of principal royal officers in North America, and a discussion of editorial methods employed in the undertaking. And each

of the transcript volumes contains brief narratives of the chief developments affecting the empire that are covered in the correspondence selected for publication.

Although the task of choosing documents meriting transcription is inevitably a subjective one, most users will have few quarrels with the selections in volumes 2 and 3. The editor has an excellent grasp of the significant events of 1770-71, and his introductions adequately explain the context of the correspondence. Many will be disappointed that more transcriptions have not been published, and that consideration was not given to the availability of these letters in other publications, especially since limitation of space was obviously an important factor in the production of these slim volumes. Nevertheless, the editor and publishers of this work deserve our applause for bringing a vast treasure of research materials within the reach of every library that aspires to promote the serious study of the American Revolution.

PAUL H. SMITH
Library of Congress

NORTH CALLAHAN. George Washington: Soldier and Man. New York: William Morrow and Company. 1972. Pp. xiii, 296. \$7.95.

RICHARD B. MORRIS. Seven Who Shaped Our Destiny: The Founding Fathers as Revolutionaries. New York: Harper and Row. 1973. Pp. 334. \$8.95.

In their own way these two volumes represent extremes in historical scholarship. Mr. Callahan's study of Washington is a mistake. It offers little to the general reader, who would benefit more from Curtis Nettels's George Washington and American Independence (1951), and it affronts the specialist who must continue to rely on Douglas Southall Freeman's seven-volume George Washington (1948–57) or James T. Flexner's multivolume study in progress.

So little has Mr. Callahan understood Washington or his times that he makes such statements as: "The ancestors of George Washington had sired a progeny which was to extend into the New World and lessen to a considerable degree the extent of the British Empire" (p. 2); Washington "encountered some Pennsylvania-German settlers who impressed him as being quite ignorant and unable to express themselves in understandable language," without suggesting that Washington did not speak German (p. 4); Washington "usually felt awkward among them [ladies], perhaps because of his height, his size and especially his big hands and

feet," (pp. 5-6), but "his imposing physical characteristics doubtless gave him tremendous confidence in himself" (p. 6), even though "he never seemed to have great confidence in his own ability as a soldier" (p. 18).

Mr. Morris's volume differs markedly. A combination of deep research, sharp wit, and a touch of iconoclasm marks this study of the psychological features of Franklin, Washington, John Adams, Jefferson, Jay Madison, and Hamilton. Morris is drawn to these figures as human beings, and in treating them he makes their humanity apparent without detracting from their accomplishments.

One problem to which Mr. Morris is seeking answers is why our age has not spawned the same quality of leadership as the Revolution, and seemingly cannot. His evaluation is a pessimistic one: "It would be unrealistic to expect that an age of conformity such as our own would be likely to spawn that creative individualism that marked the leadership of the American Revolutionary era" (p. 3). Among other reasons, Mr. Morris attacks our "age of materialism," our uncongenial climate for intellectuals, and the perversions of technological communications media. America once had a "fortuitous conjunction of character and destiny"; now it needs honest figures, not charismatic ones.

Regardless of the merits of this basic premise Mr. Morris has presented an entertaining and instructive examination of the Founding Fathers, one that elevates their humanity without demeaning their accomplishments. As a tool for enticing students into a study of the Revolutionary era, Mr. Morris's volume has real value.

LAWRENCE H. LEDER Lehigh University

HUGH F. RANKIN. Francis Marion: The Swamp Fox. (Leaders of the American Revolution Series.) New York: Thomas Y. Crowell Company. 1973. Pp. xv, 346. \$10.00.

This, the first full-scale modern biography of Francis Marion, separates fact from fiction concerning the career of one of the best-known guerrilla fighters of the American Revolution. Dubbed the "Swamp Fox" by the British themselves, Marion emerged as a folk hero during the ebb tide of the Revolution in the South. The fall of Charleston to the British on May 12, 1780, was followed in August by the disastrous defeat of an American army at Camden. All semblance of state government in South Carolina disappeared and all the ugly aspects of

civil war began to show themselves. The British resorted to terror to subdue the remnants of resistance; there were patriots against Tories, Tories divided and informing on one another, and just plain criminals roaming the countryside and plundering the populace at will. Acting as a leader of small bands of militia, Marion harried the British supply lines, threatened their outposts, and, according to his own views, righted wrongs.

Until the appearance of this reliable account, it was difficult to make an assessment of the significance of Marion's exploits. The first biography did not appear until the early part of the nineteenth century. Peter Horry, who served under Marion, produced a life that made some use of documents but that relied mainly on the memories of those who knew Marion. Unable to find a publisher, Horry somehow made the acquaintance of "Parson" Weems whose recently published life of Washington was rapidly becoming a best seller. Since he was casting about for yet another Revolutionary hero on whom to exercise his vivid imagination and neoclassical literary style, Weems undertook to rewrite Horry's account. What emerged was a knight in shining armor, a "Washington of the South." Totally disregarding the chasm that separated the relative importance of the two leaders, Weems concluded that it was "difficult to determine whether Marion or Washington most deserves our admiration." The legend thus created would not die. Nearly half a century later the South Carolina poet-novelist, William Gilmore Simms, found Weems's biography a fable for babes and undertook to write a book more suitable for adults of his time. Although Simms added some substance, the portrait that he presented was hardly less idealized than that of Weems. In recent years the nineteenthcentury romances have been corrected—at least obliquely-by some good local history and by reliable military studies of various facets of the war in the South. Rankin, however, is the first professionally trained historian to examine in detail the entire military career of Marion. Since there are few personal manuscripts, the story had to be pieced together from relatedbut widely scattered-documents and collections. Rankin has spared no effort to search out all the available evidence on both sides of the Atlantic.

The product of this painstaking research is a realistic but not a debunking biography. Marion served continuously from the beginning to the end of the war. His military career consisted of both normal or conventional service as an

officer in the Continental Army and service in irregular warfare as an officer of the South Carolina militia. As a Continental officer he took part in the defense of Charleston in 1776 and served capably, but not outstandingly, until 1780 when, owing to an accident in which he broke his ankle, he was convalescing in the back country when Charleston fell to the British. With only general directions from the governor, Marion acted alone for a few months as a militia officer. Rankin concludes that Marion "was a natural partisan leader and was able to utilize a relatively small striking force to its greatest potential—his primary weapon was surprise. His greatest strength was in keeping his men well-mounted thereby frustrating the designs of a superior force to bring him to a decisive action and destroy him" (p. 298). The author makes it clear, however, that Marion's operations were small scale and scattered, that his efforts were ably supplemented by Thomas Sumter, Andrew Pickens, and other partisan leaders in the South, and that battles such as King's Mountain and the Cowpens had far greater effect on the larger picture. As for the legend that he dispensed even-handed justice to friend and foe alike, Rankin concludes "Francis Marion was no knight in shining armor for his operations often have a stamp of ruthlessness when he considered such a course necessary" (p. 299).

HARRY L. COLES
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I. H. BUTTERFIELD and MARC FRIEDLAENDER, editors. Adams Family Correspondence. Volume 3, April 1778—September 1780; volume 4, October 1780—September 1782; Index. (The Adams Papers, Second Series.) Cambridge, Mass.: Belknap Press of Harvard University Press. 1973. Pp. lvii, 426; xvii, 472. \$32.50 the set.

The richness of the Adams Papers is again apparent in this installment. Since John Adams was in Europe and young John Quincy Adams was with him during most of the timespan covered, while Abigail remained in Braintree, there was plenty of occasion for writing. Consequently we have here an exceptionally profuse exchange of letters among members of the family. Taken together, the four volumes now in print are as full a domestic correspondence as exists for eighteenth-century America, and the wisdom of bringing them out as a separate series becomes apparent. Although the letters touch constantly on public affairs—the Silas Deane controversy, wartime inflation, the rela-

tions of Congress with its ministers—John Adams was too obsessed with the fear of his letters being intercepted to put much political news, or indeed much of any news, into them. The value of the correspondence lies accordingly in the opportunity it offers for probing the character of human relations, especially domestic relations, during the period.

What stands out is the formality, not to say preciosity, with which husbands, wives, children, relatives, and intimate friends addressed one another. A hortatory tone was almost compulsory. Even children felt called upon to insert little homilies into the simplest communication. Abigail, who dominates the correspondence not only by the bulk of her letters but also by the quality of mind exhibited in them, will occasionally break forth in spontaneous angry exclamations, against John for not writing often enough or fully enough (had he "changed Hearts with some frozen Laplander?") or against Franklin ("False insinuating disembling wretch"). But such outbursts are few. Although she obviously chafes at the purely domestic role assigned her, she is the soul of propriety. Her numerous letters to James Lovell are surely as discreet an epistolary flirtation as any lady ever engaged in. And the replies she received from Lovell and from her other principal correspondent, John Thaxter, are equally though differently conventional (and without the benefit of her wit). Lovell in particular affected a tiresome, whimsical facetiousness in the manner of Laurence Sterne, evidently intended as the right posture for a gentleman of high fashion to take with a lady. John Adams was much less inhibited than other gentlemen by literary conventions, and the reader is likely to be as impatient with him as Abigail was for not writing more fully. One is also inclined to blame his neglect for the fact that she should have wasted so many words on the likes of James

The most pathetic figure in the correspondence is John Quincy Adams, who regularly reports to his parents his relentless progress in learning every available language and reading every available book. He receives in return regular admonitions to do more and to curb his "impetuosity of temper." As the editors sagely observe, he was obliged to become a man before he had finished boyhood. One might almost say that he never had a chance to begin it.

The Adamses were doubtless an unusual family, but they were closely attuned to contemporary American values. Their very ambition made them so in a country where a public ca-

reer depended on pleasing the public. The letters will thus bear close study as a repository of those values. The editors have taken pains to make their annotations fit the character of the correspondence. They supply us with the information for understanding the tone as well as the content of the letters. And the index to the volumes is a work of art in itself. The renewed interest among historians in the history of the family should receive a considerable impetus from this extraordinary record, so skillfully presented.

EDMUND S. MORGAN Yale University

FREDERICK W. MARKS III. Independence on Trial: Foreign Affairs and the Making of the Constitution. Baton Rouge: Louisiana State University Press. 1973. Pp. xvi, 256. \$10.00.

This book is a modest survey of American diplomacy in the Confederation era, and an analysis of the effect of foreign affairs on the creation and adoption of the Constitution. It is competently researched, logically organized, and rather gracefully written. It is also something of a disappointment.

This subject has never before been treated in depth. Historians of the Critical Era and diplomatic historians treating later developments have scanned America's foreign affairs in this period, but Professor Marks has organized this scattered information into one convenient volume and added some new detail of his own. For this he deserves our thanks. Unfortunately, however, he has failed in his stated aim "to go beyond factual narrative and to open new avenues of thought." Certainly there is little that is new or startling here.

The basic theme of the book is that previous historians have given too little emphasis to the role of America's perilous diplomatic situation in the movement for a stronger national government. This may be true, although no historians have failed to mention it and none have doubted that it was of vital importance at least to the Federalists. Unfortunately, Marks's attempt to prove what needs little proof leads to a somewhat one-sided and bombastic presentation. A concentration on foreign affairs to the exclusion of domestic and ideological considerations cannot help but lead to the conclusion that a stronger national government was desirable. But Marks overdoes it, and his book takes on an almost Fiskean tone.

A different thesis might have permitted the author to concentrate on some of the more

promising points that emerge from his book. He claims that British trade restrictions affected the economy and mood of the American people more than historians like Merrill Jensen have allowed. The evidence Marks offers for this, unfortunately, is drawn almost exclusively from the well-known works of Curtis Nettles and Lowell Ragatz. I think Marks is right, and it is valuable to have the question reviewed as he has done, but some extensive and original probing might have made his case more solid.

Marks also challenges the importance of Shay's Rebellion in the movement for the Constitution. Even while denigrating its significance, however, he says that its impact had at least something to do with foreign affairs, in that many people blamed British subversion for instigation of the rebellion. But it is impossible to tell from the few snippets of information the author presents how widespread this suspicion was. Marks goes on to point out that the friends of the Constitution differed from their opponents not only in socioeconomic status, but also in their experience and knowledge of foreign affairs. The evidence offered, however, is simply a list of names. There is no attempt at a statistical analysis of this point, as historians of the domestic side of the Constitutional debate have been able to offer. This lack of precise analysis also leaves in doubt his interesting claim that the South welcomed rather than resisted federal control of foreign commerce, fearing only taxes on exports and slaves.

Still, this is a solid and valuable work. The diplomatic situation is clearly outlined, and previously scattered information conveniently organized. Perhaps Marks is now in a position to go on to the more precise analysis this era requires.

JERALD A. COMBS
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ARTHUR M. SCHLESINGER, JR., general editor. History of U.S. Political Parties. Volume 1, 1789–1860: From Factions to Parties; volume 2, 1860–1910: The Gilded Age of Politics; volume 3, 1910–1945: From Square Deal to New Deal; volume 4, 1945–1972: The Politics of Change. New York: Chelsea House Publishers, in association with R. R. Bowker Company, New York. 1973. Pp. liv, 882; xiii, 885–1807; xii, 1811–2669; xi, 2673–3544. \$135.00 the set.

These volumes contain twenty-six essays by twenty-five authors, plus an introduction by Arthur M. Schlesinger, jr. With the exception of the editor's introduction, each essay focuses on a single political party over an extended period of time. While most of the essays examine major parties, there is an essay on the important minor parties in American history. In each case, the emphasis is on presidential politics and parties.

With few exceptions, the essays tend to reflect the current state of his toric scholarship on the history of American political parties, as each author is a recognized authority on his or her subject. A few of our best scholars (i.e., Lee Benson, Walter Dean Burnham, William Chambers, Richard McCormick) on American political parties, however, did not participate in the enterprise, and their work is inadequately reflected in the essays. Just as a historical knowledge of political parties is uneven, so too is there variability in the quality of the essays. The best essays are those by Michael Holt on the Antimasonic and Know Nothing parties, David Donald on the Republican party 1864-76, Paul Kleppner on the Greenback and Prohibition parties, and Richard Wade on the Democratic party, 1960-72.

All of the essays might have been longerand therefore more informative-were it not for the fact that each is accompanied by numerous documents (speeches, party platforms, editorials, etc.) that are well known to the specialist and that unfortunately consume considerable space. Even so, the essays reveal a great deal about present-day historical scholarship. They clearly demonstrate that there is little agreement among political historians either about the concepts they employ or over the questions they wish to raise about parties. For this reason, it becomes rather difficult to assess whether we are advancing in an understanding of American political parties or whether we are simply accumulating at a rather rapid rate a potpourri of disparate, noncumulative information. When Hans L. Trejousse raises quite different questions about the Republican party during the period 1854-64 from those raised by William Harbaugh in his analysis of the Republican party 1893-1932, it is unclear in what respects the functions and the structure of either the party or the political system changed over time. One might make the same type of remark about all other essays in the volumes. In other words, the essays are poorly related conceptually and theoretically to one another. And because political historians have not been very explicit in defining the conceptual framework within which the history of parties might be explored, these essays have little potential for advancing an understanding of party development.

Schlesinger's introductory essay has some of this potential, but it too seems unrelated conceptually to the other essays. I hope the following comments will make a modest contribution to clarifying some of our conceptual confusion.

Most of these essays, like much of our party literature, assume that American political parties may be analyzed by focusing primarily on presidential party politics. The American party system, however, has been a loose confederation of local and state parties. Every four years they have met collectively to choose national candidates and to write a platform, leaving behind an enfeebled presidential party organization. But the presidential party structure has been only one of many—and it has certainly not been the heart and substance of party politics. Indeed, the state and local organizations have historically not been subordinated to some higher party authority.

As we attempt to comprehend the vast number of parties since the 1790s, perhaps we should begin by typing them into two broad classes according to their goals: parties that emphasize their programs over all other considerations and parties that are constituency oriented. The programmatic party has historically been primarily concerned with sharpening issues and with educating the electorate on policy matters, whereas the constituency-oriented party has been primarily concerned with maximizing the size of the popular vote at the expense of clarifying issues. As most parties have both of these goals, the distinction between the two classes is a matter of degree, with minor parties in American history tending to be programmatic types and major parties constituent oriented.

Having typed parties into these two broad classes, it would next be useful to analyze them according to their functions and structures. While there is no definitive list of political functions, there are nevertheless at least five basic functions that political systems perform: (1) they structure the vote by sharpening distinctions among candidates, programs, and organizations; (2) they articulate as well as channel the demands of various groups into policy alternatives; (3) they participate in the formulation and implementation of policy as well as in adjudication of conflicts; (4) they participate in the recruitment of political leadership; and (5) they play an important role in the communication function—the linking of elites and masses, the integration of the various structures of government, and the socialization of groups into the political culture.

By failing to focus systematically on the functions of parties, these essays are not very informative about the changing role of parties in the American political system over time. By emphasizing functions, however, we are better prepared to assess the importance of parties at any one point in time. In other words, a functional approach helps us to determine the extent to which it is worth our while to study parties. Whereas American parties once dominated most of the five functions, their influence over these functions has become less important in recent years. And as scholars focus attention on the American political system during the period after 1900, they probably will have a better understanding of the American political system if they concentrate on functions and. processes rather than exclusively on the institution of parties-for it is institutions other than parties that increasingly fulfill these five functions.

As long as we focus on parties, however, we want to be sensitive only to their functions but also to their structures. And we would be well advised to develop a set of concepts with which we can compare the structure of one party with another and with which we can assess the change in structure of a particular party over time. Without such a set of concepts, it becomes particularly difficult to specify the criteria by which party structures vary. And because the authors of these essays do not share analytic concepts, the various discussions of party structural characteristics are relatively unrelated to one another. Of course, there are implicit in the essays some vague notions of the formal properties of party organizations, but for purposes of future studies, perhaps it will be useful to be explicit about the six basic properties by which party structures vary. They are (1) the level of stratification, which refers to the distribution of rewards between those at the top and the bottom of a party organization and to the difficulty in moving from the bottom to the top of the party structure; (2) the level of complexity, which refers to the number of specified roles within the party structure, especially to the degree of professionalism within a party; (3) the permeability of party organization, which refers to the ease with which new ideas and new members are welcome to a party organization; (4) the intermittency of party structure, which refers to the degree of permanency in party organizations. Thus the presidential party organization is essentially a temporary phenomenon, generally assembled every four years-especially the party out of

power—whereas the party machines of the late nineteenth and early twentieth centuries had an element of permanency to them; (5) the level of participation, which refers to the degree to which party members participate in decision making and elite recruitment. Thus a party primary obviously provides a much higher level of participation than an organization that recruits its leaders by a caucus held in a "smoke filled room"; (6) the degree of formalization, which refers to the degree of codification of rules and procedures in a party organization. The greater the number of rules about recruitment, financing, participation, etc., the higher the level of formalization.

Had the authors of these essays been in greater agreement on the functions and formal properties by which parties vary, their description of political parties would have been much richer. Moreover, they would have been better able to describe the boundaries between political parties and the rest of the political system. But ultimately, we should move beyond sheer description of the processes of change to a different level of analysis. And here we might use our concepts as either dependent or independent variables. Viewing parties as dependent variables, scholars might specify the conditions under which there is variation in their structural characteristics-i.e., the level of participation, complexity, etc. Studies elsewhere by M. Ostrogorski, James C. Scott, and Paul Goodman are very useful in this respect, though the authors in these volumes have not been very receptive to this type of analysis. Historians, moreover, might treat structural characteristics as independent variables in order to explain changes in the functions of parties—the structuring of the vote, political recruitment, etc. For example, we know from previous studies that the greater the levels of stratification, complexity, and formalization of parties, the less likely electoral behavior is to be volatile. And one reason that we find greater volatility in presidential voting than in local and parties is because presidential parties are more intermittent, less complex, and less formalized. In other words, shifts in voting behavior in presidential politics are somewhat more volatile because presidential parties have more amorphous party structures. With similar strategies, we might assess the influence of varying levels of permeability, intermittency, complexity, stratification, etc. on the nature of party recruitment and public policy.

Because the essays in these volumes are conceptually weak, problems such as these are not

systematically raised. Rather, much of the discussion focuses on the role of personalities and idiosyncratic events in shaping the history of American political parties. As long as our political history sidesteps a systematic analysis of the functions and structures of political parties, we are likely to continue having an inadequate understanding of the processes influencing voting behavior, public policy, and elite recruitment.

In sum, the publishers and editor have provided us in one place a handy source for quickly ascertaining the general state of scholarship on each of the major and minor parties in American history. In doing so, they call to our attention just how little consensus there is about research problems and research strategies of political historians.

J. ROGERS HOLLINGSWORTH University of Wisconsin, Madison

ALLAN R. PRED. Urban Growth and the Circulation of Information: The United States System of Cities, 1790–1840. (Harvard Studies in Urban History.) Cambridge, Mass.: Harvard University Press. 1973. Pp. xiv, 348. \$15.00.

For more than a generation geographers have been leaving the land and invading such social sciences as demography and economics. The new field of regional science, for example, is a combination of geographic, demographic, and econometric models. But few modern geographers have explored the causes of social change in relatively remote historical periods. Allan R. Pred's book is, therefore, a pioneer study of the formative era of the business and economic patterns of American cities. Its focus, to be sure, is on flows of information and their correlation with urban growth, but the information was at least 75 per cent business news and the cities grew mainly from the resulting increase in commerce. For those interested in the growth of urban communication and travel in the first five decades of the Republic, this is the most useful and penetrating study that has appeared.

Pred's abundant statistics, culled from many well-known but little-used sources, show the changing quantities of business news and travelers between the major cities of the nation and the time consumed for each type of exchange. Collectively these explain the rank order in population, not only of the great east coast seaports, but of practically all other urban centers as well. The fact that in 1817 an isoline represent-

ing five days of travel from New York City embraced Norfolk, Syracuse, and Portsmouth, whereas the same isoline from Philadelphia reached only from Richmond to Hartford and New London, goes far to explain the ascendancy of New York. The better ocean harbor and water communications of the latter put it in command of the distribution of both European news and goods. A minor criticism of Pred's generally admirable trade-flow estimates is that using common carrier costs does not correctly reveal the area or extent of inland wagon transportation. As Lewis Atherton has shown in Southern Country Store, 1800–1860, a farmer with nothing pressing to do after harvest would hitch a horse or mule to a wagon and take crops long distances to market, camping along the way and carrying feed for the animal, at no outof-pocket expense.

Pred offers an interesting and pursuasive model for the growth of commercial cities. It shows how mercantile investment begets increases in trade and some light industries that together tend to maintain the rank order created initially by geographic factors. In elaborating his model he notes a fact often overlooked, that real estate and construction profits fed back into the expansion and increasing specialization of facilities for trade. Whether or not from the causes he assigns, long-run population statistics bear out Pred's conclusions.

Historical readers will occasionally be bothered by the special vocabularies of geography and economics, and will think that too much time is spent demolishing theories of diffusion based on urban hierarchies arranged strictly by size. But each science has its own sacred cows and this one no doubt needs slaughtering. Historians from all fields of interest, however, should press on and learn what is in this book.

THOMAS C. COCHRAN
Eleutherian Mills-Hagley
Foundation

HAROLD C. SYRETT et al., editors. The Papers of Alexander Hamilton. Volume 18, January 1795–July 1795; volume 19, July 1795–December 1795. New York: Columbia University Press. 1973. Pp. xvi, 557; xv, 546. \$17.50 each.

Volumes 18 and 19 of *The Papers of Alexander Hamilton*, under the editorship of Harold C. Syrett and his associates, include Hamilton's writings for the year 1795. This work sets a high standard of scholarship. Furthermore, unlike the excessive editorial comments of the

current edition of the Jefferson papers, the editors here illuminate but do not obtrude. Some pertinent correspondence to Hamilton is also presented either in whole or in summary.

The period of 1795 marks three principal areas of Hamilton's work. In the first place, he completes his service as secretary of the treasury. One of the concluding papers of this period of service is his final report of recommendations concerning the fiscal policy of the United States. In the second place, there is the long series of articles, signed Camillus, in defense of Jay's Treaty. The third area embraces Hamilton's activities after retirement from the secretaryship in mid-1795. This includes the political and administrative correspondence with Washington and members of the national cabinet. Part of this correspondence is political, such as that soliciting votes in Congress either for funding measures, other Treasury matters, or the ratification of Jay's Treaty. Other correspondence covers the content of Washington's messages to Congress. The latter category emphasizes the continuing influence of Hamilton on public policy after he retired from the government.

This edition underscores, as did the predecessor volumes, Hamilton's efforts in time spent, in correspondence, and in newspaper articles concerned with political matters. Because earlier, more limited editions of Hamilton's writings omit many of his newspaper articles, there is a tendency among writers or prior editors who evaluate Hamilton to give less emphasis to the sheer amount of time he spent on politics. These two volumes show very well how Hamilton was not only the key political figure as the national Federalist party evolved, but he was also instrumental in directing contacts with members of Congress in aligning votes for critical measures. At the same time he was at the heart of the Federalist party operations in New York and in the management of state party affairs. He also wrote for the New York newspapers the leading articles on both Treasury policy and foreign policy. His chief opponent, Thomas Jefferson, referred to Hamilton as a host within himself. Thus, at the time essential United States government policy was first formulated, Hamilton's papers show his key role in determining policy, molding public opinion, presenting matters for interpretation by the courts, and, finally, constructing the political party apparatus.

MANNING J. DAUER University of Florida

JOHN A. MUNROE. Louis McLane: Federalist and Jacksonian. New Brunswick: Rutgers University Press. 1973. Pp. xi, 763. \$22.50.

Louis McLane is probably best known to history as the secretary of the treasury who was booted upstairs by Jackson because he was unwilling to remove the government deposits from the Bank of the United States. But this is only an incident, not quite correctly remembered, in a long and active career that is illuminated in all its ramifications in this excellent biography.

An unreconstructed Federalist, McLane entered the House of Representatives from Wilmington, Delaware, in 1817. Four years later he barely lost the Speakership to Philip P. Barbour of the Crawford faction, who promptly dispelled any rivalry by making McLane chairman of the powerful Ways and Means Committee. Thus encouraged, he supported Crawford in the multicandidate presidential campaign of 1824. He won a Senate seat in 1826 with Democratic support and by 1828 was in the Jackson camp. Jackson sent him as minister to England, recalled him two years later to be secretary of the treasury, then made him secretary of state in 1833. In disagreement over policy, he resigned a year later. There followed an interlude as president of the Morris Canal and Banking Company, and ten years as president of the Baltimore and Ohio Railroad. He returned to public life in 1845 to go once more as minister to England, but retained the post only until the Oregon question was settled.

Although he never achieved the first rank among the molders of nineteenth-century America, McLane left a solid mark in many areas. His Treasury Department report on manufactures remains a classic and an indispensable source for economic historians. He was a successful diplomat, a successful financier, and an unusually successful business executive. His ambition was boundless and he was indeed one who, but for historical accident, might have risen to the first rank among American statesmen. His judgment was usually sound, but he never overcame an irascible temper that merged with an instant combativeness to leave a trail of enemies in the wake of his every success. Yet he was close enough to the wellsprings of power to comment meaningfully on the events of his time. He was an indefatigable letter writer who reported in great detail to members of his family.

It was the discovery of this large family correspondence in two private collections still in the hands of descendants that made Professor Munroe's study possible. He has used this new material, together with previously accessible sources, to add measurably to our understanding of the Jackson period, of the fascinating characters who peopled it, and of the interwoven events that swept it forward. He has given us new insight into the political and economic development of the first half of the nineteenth century in an immensely readable book, which will be invaluable to all who toil in that particular vineyard.

CHARLES M. WILTSE Dartmouth College

SARAH MCCULLOH LEMMON. Frustrated Patriots: North Carolina and the War of 1812. Chapel Hill: University of North Carolina Press. 1973. Pp. viii, 223. \$10.50.

The title of this book is apt for it is a study not only of frustration, but of mismanagement, confusion, and chaos at all levels. Much of the ineptness and haphazardness of the national government was reflected in the state government; both came forth with grandiloquent plans that they managed to execute in a miserable fashion. This work is arranged topically, but every topic staggers under the load of ineptitude recounted therein.

North Carolina had little reason to be irritated by the usual causes of the War of 1812. Professor Lemmon sees the state's reluctant entry into the conflict on the grounds of old resentments still simmering from the American Revolution, added to what its people considered to be insult to national honor. Approximately one-fourth of the congressional delegation and one-third of the state legislature opposed the war, partly because they feared the expanding power of the executive branch of government. Expansionist sentiment was weak in the state with most politicians feeling that the invasion of Canada was a good tactic to bring Britain to a proper way of thinking; yet they feared a successful conquest of Canada would lead to an upset in the balance of power if the new territory were divided into states. On the other hand, interest was expressed in the annexation of Florida.

Military operations were a farce, although in proportion to her population, North Carolina furnished more than her share of troops. The tradition of the worth of the citizen soldier still held strong despite the rather woeful experience with the militia during the American Revolution. The primary activity was marching and countermarching almost aimlessly about the

state. Other phases of the war effort were equally lacking in direction.

Effects of the war on the state were subtle. The war destroyed an already weak Federalist party. Free "men of colour" made a slight advance when they were allowed to enlist in the militia. And in a more direct manner, North Carolina suffered from the war as the opening of the Southwest, freed from the menace of the Indians and the British, lured away many of its best people, who took with them their talents, their wealth, and their labor. For the next twenty years, the state was to sink even deeper into the doldrums, earning the nickname of the "Rip Van Winkle State."

Professor Lemmon has written a good book. Her style is straightforward and her research is a model for such studies. One could quarrel with the organization and length of paragraphs, but that would be nit-picking and this is too good a work to subject it to that treatment.

HUGH F. RANKIN
Tulane University

ROBERT W. JOHANNSEN. Stephen A. Douglas. New York: Oxford University Press. 1973. Pp. xii, 993. \$19.95.

Stephen Douglas did it all in a very short time, and then came up with next to nothing. He won his first election at the age of twenty-two, and became judge of the Illinois Supreme Court before turning thirty. The Vermont-born boy wonder zoomed up the Illinois political ladder, reaching the U.S. Senate in 1847 and achieving stature (if not height) as congressional manipulator, compromiser, presidential aspirant, and defeated Democratic candidate.

Robert Johannsen's massive, well-researched, and stodgily written biography tells us much (at times too much) about Douglas's life, yet reveals surprisingly little. Johannsen warns at the outset: "I make no claims of discovery," and remains true to his word. In almost a thousand closely packed pages of narrative and backnotes, we follow the course of a career singularly devoted to politics, one that represented last-ditch Jacksonianism and its premise that national politics should stop on this side of the slavery issue—"that dangerous distraction," in Douglas's mind. Douglas could not exorcise that devil, of course, not if he hoped to maintain a viable Illinois constituency. Popular sovereignty, Douglas's one-hoss shay, so perfect in theory and supposedly so easily applied, fell apart in the face of Northern moralism and anti-Southernism (two complementary factors of enormous importance that Johannsen does not adequately assess in analyzing Douglas's career).

What sort of person emerges from these pages? It is a curiously one-dimensional politico: talented, hard working, earnestly ambitious, but limited and doggedly unadaptable. Anything but a visionary, even his high-blown rhetoric about American expansion and mission smacks of parochialism, and degenerates into calculating pugnacity. Johannsen reaffirms Douglas's high repute as an orator, citing enough admiring contemporaries to make the point. But the excerpts from speeches in Congress and on the stump leave one wondering over a previous generation's peculiar taste.

The book's principal revisionist feature is the down playing of Lincoln. No Douglas biographer can ignore his man's Whig-Republican nemesis, of course, but Johannsen has clearly chosen sides. References to encounters before the debates tend to place Lincoln in the shade. This is fine because Douglas dominated the politics of the 1850s, while Lincoln did not dominate even the politics of Illinois. But not so fine is Johannsen's reluctance to come to grips with the kind of politics that Lincoln capitalized on between the debates and the election of 1860. The rise of Republicanism seems as mysterious and distasteful a phenomenon to the author as it doubtless did to Douglas. If only sanity, good will, and mutual accommodation had prevailed. If only the Jeffersonian-Jacksonian Democratic republic had continued to exist half-slave and half-free, or maybe twothirds free and one-third slave. Douglas tried to pull it off but could not. He remained too much of a Northerner to become a doughface, and too much of a patriot to have become anything else but a War Democrat had he lived. It all ended for Douglas in 1861 in rum, rebellion, and almost in Romanism (if his second wife had had her way).

FRANK OTTO GATELL
University of California,
Los Angeles

JULIA FLOYD SMITH. Slavery and Flantation Growth in Antebellum Florida, 1821–1860. Gainesville: University of Florida Press. 1973. Pp. ix, 249. \$8.50.

With only a handful of large planters and a slave population that numbered but 62,000 on the eve of the Civil War, Florida could scarcely be classified as a major plantation state. Nevertheless, in this attractive and meticulously docu-

mented monograph, Julia Floyd Smith has provided useful supplementary data for interpreters of the slave South. This study should supersede the old Phillips and Glunt edition of the plantation records of George Noble Jones as the most important single source of information on the plantation-slave society of antebellum Florida.

Focusing upon the middle portion of the Florida Panhandle, principally those counties lying between the Apalachicola and Suwannee rivers, the author documents the growth of a cotton economy which rivaled, in some respects, that of the more prolific Deep South cotton states. Her extensive research in newspapers, county tax books, estate records, unpublished census returns, and other primary materials, is impressive. Of particular interest are chapters on the legal status of slaves, the factorage system, and the large slaveholders of Florida. Earlier chapters on slave trading, plantation management and labor, and slave living conditions contain little that is new (as well as much that does not relate directly to Florida) and are less useful.

The author draws few conclusions of her own, preferring instead to analyze the views of earlier historians. She enters the perennial debate concerning the profitability of slavery, asserting that "slavery, as a business enterprise, was undoubtedly profitable for the owner" -especially in Florida (p. 176). But one searches in vain for unequivocal conclusions concerning other points of historical contention. Smith also succumbs to the temptation, common among those who revise their doctoral dissertations for publication, to include much material that is trivial in nature or of doubtful relevance—a tendency compounded in this case by the relative paucity of data available for the general study of the "peculiar institution" in a marginal plantation state. Thus, it is difficult to discern the relevance of the initial chapter, entitled "Slave Plantations of the New World," which consists largely of an analysis of Carl Degler's recent challenge to the Elkins-Tannenbaum thesis. And the concluding chapter, which commences with a historiographical essay on the profitability of slavery and concludes with brief discussions of religion and education in antebellum Florida, is a veritable organizational nightmare.

Despite its limitations, this book is a significant contribution to the secondary literature on the Old South. Its value is enhanced by an extensive bibliography, several useful appendixes—one of which contains excerpts from the Florida Slave Narrative Collection, a number of tasteful illustrations, and an excellent index.

WILLIAM K. SCARBOROUGH University of Southern Mississippi

EDWARD PESSEN. Riches, Class, and Power before the Civil War. Lexington, Mass.: D. C. Heath and Company. 1973. Pp. 378. \$7.95.

Professor Edward Pessen of City College is fond of asking embarrasing questions, and not even Alexis de Tocqueville, who has enjoyed for so long the prestige of a statue in a public garden, is a sacred figure in his eyes. Perplexed by Tocqueville's inference that the wealth of the United States in the 1830s was democratically distributed, Pessen determined to do his own research on this topic. Unafraid of the drudgery of poring over tax assessments, he has been laboring for some time on those of New York, Brooklyn, Boston, and Philadelphia in the antebellum years. Perhaps because he rereads Vanity Fair every so often to recover from this ordeal, the results of his researches are readable and no one can claim that he puts forward the findings of a pedant. Pessen states that "the that ante-bellum America lacked substantial fortunes is not borne out by the evidence," and he speaks with an authority that may make more than one historian wonder whether Pierre Lorillard was the first New Yorker worth one million dollars.

The author has come to question what might have passed for evidence in the days of Moses Yale Beach, whose listings of the very rich Pessen finds quite unreliable. Philadelphia, he admits, was a quagmire, for assessments on real estate were leveled not on owners but on occupants. And he reminds us that Mayor Josiah Quincy of Boston was no friend of the truth in these matters. To Quincy "an accurate exhibit of personality was ruinous to a businessman, besides being in many cases entirely impracticable."

Pessen's conclusions are respectable and not likely to be challenged. He argues that "in the year of Andrew Jackson's first election to the presidency the wealthiest four per cent of the population, in owning almost half the wealth, possessed a larger proportion of New York City's wealth than the richest ten per cent had evidently owned in the urban northeast half a century earlier." So much for New York. In the Brooklyn of 1845 the richest one per cent had half the wealth, just like New York in that year. Boston was no more demo-

cratic, for eighty-three per cent of the rich of 1838 were already rich in 1833.

"Above all," Pessen asks at the end, "what happens to the romantic glorification of the common man? Amid all the hullaballoo about his alleged dominance in the era, the common man appears to have gotten very little of whatever it was that counted for much."

WAYNE ANDREWS
Wayne State University

CLIFFORD M. DRURY. Marcus and Narcissa Whitman and the Opening of Old Oregon. In two volumes. (Northwest Historical Series, 10 and 11.) Glendale, Calif.: Arthur H. Clark Company. 1973. Pp. 476; 435. \$38.50 the set, postpaid.

Presbyterian minister with a Ph.D. in history, retired professor of Church history at San Francisco Theological Seminary, Clifford Merrill Drury has devoted forty years to the history of the Oregon Mission of the American Board of Commissioners for Foreign Missions. Materials he discovered for his biographies of the leading figures he has edited and published. Although his near dozen books are limited to a narrow specialty, he has uncovered so many materials and has written in such rich detail that all students of the Pacific Northwest during the period of the Oregon Mission, 1835–47, are in his debt.

This is the most complete biography of the Whitmans. Expanded on his long out of print Marcus Whitman, M.D., Pioneer of Old Oregon (1936), these volumes profit from the use of sources not available or not yet discovered by 1936. Only one conclusion is different—he has "given more attention to the significant role that Whitman played in the opening of Old Oregon to American settlement," (vol. 1, p. 19). Its importance, says Drury, is that the boundary was set at forty-nine degrees "largely because of the numerical strength of the American colony in that territory . . ." (vol. 2, p. 91), an interpretation that overlooks many other factors, including Anglo-American relations, internal politics in both countries, and Polk's interest in expansion in areas other than Oregon.

Two more matters—first, students of the Jacksonian period will wish that the book had been set in a broader context. Second, although Drury acknowledges the conflict of cultures—but it began before 1843 (vol. 2, p. 95)—little is said of the culture of the "uncivilized Cayuses" (vol. 1, p. 443), particularly of Indian religious ideas. He is clearly on the

side of the whites. Infant head flattening was dying out by 1836; referring to the Whitman child born in 1837 he says, "It requires little imagination for us to believe that no Cayuse mother would willingly deform her baby's head after seeing Alice Clarissa" (vol. 1, p. 247).

Mr. Drury has done well what he set out to do. He has told the dramatic story of two people whose faith and dedication were so strong that they stayed at Waiilatpu knowing that it might cost them their lives, which indeed it did. This kind of devotion together with the Indians' attempt to preserve their civilization, constitutes the real tragedy—conflict, not between good and evil, but between two goods.

ROBERT L WHITNER Whitman College

JANE SHAFFER ELSMERE. Henry Ward Beecher: The Indiana Years, 1837-1847. Indianapolis: Indiana Historical Society. 1973. Pp. xiii, 317. \$7.50.

This book is a carefully written and thoroughly researched chronicle of the first ten years of Henry Ward Beecher's professional life. Elsmere quite correctly considers these years (particularly the eight years spent as pastor of the Second Presybterian Church in Indianapolis) to be the formative period in which Beecher laid the foundations for his triumphs in the 1850s and 1860s as the prince of Plymouth Pulpit. A descriptive rather than an analytical biography, the book tells the story of Beecher's struggles to establish himself professionally and to support a family as well as of his growing effectiveness as a preacher and the expanding scope of his activities and influence in the community and region. It is a balanced and judicious treatment. Attentive to the variety of pressures Beecher faced, it gives a sensitive account of his decision in 1843 finally to address slavery from the pulpit. In addition, it gives a sympathetic portrayal of the difficult and somewhat pathetic Mrs. Beecher, who was unable to adjust to a society that did not give automatic deference to a minister and his family and did not sufficiently esteem the Beechers' superior refinement and who came to loathe Indiana as she saw herself worn down by childbirth, illness, and the genteel poverty congregations imposed upon young ministers.

As a biography, however, the book presents some difficulties. Though it is accurate and inclusive, the biographical rationale and focus remain somewhat unclear. Elsmere does not

really deal with the problem of Beecher's distinctiveness or significance as a cultural leader. As portrayed in these pages, Beecher is indistinguishable from any other moderately talented, ambitious, and moderately successful minister of the period. We are shown that he developed into an effective preacher, but we are not provided with a very clear idea of what made him effective. In fact, Elsmere does not appear to have a very well worked out conception of what aside from the fact of fame and popularity was important about the fully formed Beecher. As a result, the question of what significance the Indiana years had as apprenticeship for his later career is left unanswered.

If, however, the book in part fails as biography, it succeeds in other ways. In a sense the book falls halfway between two historical genres: biography and local history. In fact, Elsmere is clearly as interested in Indiana history as she is in Henry Ward Beecher, if not more interested. Unlike most biographies in which the context is portrayed through the eyes and largely from the sources concerning the biographical subject, Elsmere gives the Indiana context equally careful attention. The book is thoroughly grounded in manuscript materials of Beecher's parishoners, and, in this sense, Beecher is used as much to illuminate Indiana society and culture as it is used as a backdrop for Beecher. In the end, perhaps, Elsmere's failure to pose some of the biographical issues precisely enough is compensated for by the portrait she gives of the growth and workings of a portion of Indiana society in the 1840s.

DONALD M. SCOTT

North Carolina State University

JOHN H. SCHROEDER. Mr. Polk's War: American Opposition and Dissent, 1846–1848. [Madison:] University of Wisconsin Press. 1973. Pp. xvi, 184. \$12.50.

Writing in the last years of American fighting in Vietnam, the author of this book sees a recurrent pattern of controversy during American wars. Dissenters have questioned the maneuvers by which America has been led into war and the objectives for which wars have been fought; their questioning has been portrayed by governmental warmakers as giving aid and comfort to the enemy. In the author's words: "Representative of this persistent war debate in American history is the Mexican War. With notable exceptions and variations,

the motivations, composition, arguments, and ultimate failure of the opposition between 1846 and 1848 are characteristic of other antiwar movements" (p. xiv). Unfortunately, Professor Schroeder's discussion of ideas and events of the 1840s is too sketchy for such comparisons to be convincing.

Schroeder's antiwar movement includes both congressmen who opposed President Polk, often for partisan reasons, and reformers who deplored what they saw as an immoral, proslavery, or anti-Republican war. Most of the book retraces the efforts of various factions of Whigs and Democrats to find viable positions on the war, slavery, and expansion. Since that story is largely familiar, it is regrettable that this book fails to show what links existed, if any, between congressional opposition and dissent outside of Washington. Although the book occasionally refers to a "small army" of dissenters, only twenty-eight pages (pp. 92-119) describe the ranks of clergymen, pacifists, abolitionists, and littérateurs who opposed the war. What are we to make, for example, of the claim, offered with little evidence, that antiwar sentiment was "unanimous" among Congregationalists and Unitarians? Are we to believe, then, that all Cotton Whigs in Massachusetts belonged to other denominations (or does their antagonism toward Polk qualify them as part of the antiwar movement)? The pages on abolitionism illustrate some of the book's shortcomings. All abolitionists are slighted except New Englanders and disunionists. Instead of exploring the relationship between the work of Joshua Giddings in Congress and the agitation of abolitionists outside, the book consigns Giddings to the chapters on politics and pigeonholes the abolitionists elsewhere.

In Dissent in Three American Wars (Samuel E. Morison et al., 1970) Frederick Merk suggests that the United States would have grabbed more of Mexico if there had been less vociferous opposition to the war. Because Schroeder's treatment of "dissent" is so superficial, it is hard to assess his contrary assertion that this antiwar movement was, like all others, unsuccessful.

LEWIS PERRY
State University of New York,
Buffalo

STUART BRUCE KAUFMAN. Samuel Gompers and the Origins of the American Federation of Labor, 1848–1896. (Contributions in Economics and Economic History, number 8.) Westport, Conn.: Greenwood Press. 1973. Pp. xiv, 274. \$11.50.

In a strongly revisionist thesis Samuel Gompers and the AFL are presented here as emerging from a radical tradition in a direct line from Karl Marx and the First International. Gompers is viewed as thinking "like Marx" with a visionary perception of a class movement built solely from the economic organization of the workers, and throughout the work Gompers is interpreted as having an ideologically consistent position, similar to Marx. Thus Kaufman can argue that Gompers's philosophy permeated the AFL to such an extent that the AFL was not an abandonment of Marxist principles but "a product of the assimilation of a Marxian tradition to the American circumstance" (p. 221). Even on the opening page we are told that a temperamental bond must have been established between the ten to thirteen-yearold Gompers and the Socialist sage, Marx, a story based on the feelings of the author.

Although Kaufman admits that the unification of American labor into a movement was an independent achievement of the trade unions, he is so interested in proving his thesis that Gompers was a radical that he does not recognize the import of the trend to the emerging AFL. Gompers moves slowly and cautiously in practical steps pressing for a benefit system and a strike fund in order to weld the diverse elements of the labor movement together into an effectively tight federation. But Kaufman sees an ultimately radical motive and design in these actions, which envisions a class organization that could eventually contest the power of the bourgeoisie. The activities of the independent trade unions over which Gompers had little or no control in the early years is completely neglected.

Faced with the problem of conflict between Gompers and the socialists, Kaufman presents a picture of the international trade unionists. He defends their practicalism and pragmatism as having been developed out of the native American environment, thus having a more effective national, social base. Through tortuous reasoning, this position is then interpreted as more truly in the Marxian radical tradition in that it contains the necessity of building an economic base for eventual effective political action in opposition to the socialist demand for immediate political action. Gompers appears again as having the more radical ideology, but Kaufman does not consider the possibility that Gompers is acting out of an essential institutional conservatism with tactics aimed to please the skilled trade unionists in order to protect his own position from the socialist challenge. Gompers was, of course, ideologically inconsistent even on the political action question, although the author depicts the changes as being consistent.

The challenge to existing interpretations of Gompers and the early AFL is easily apparent, but Kaufman stops short of proclaiming that Gompers was a theoretical Marxist. Using examples from Gompers's letters as his primary base, Kaufman argues that the radical Marxian tradition persisted in Gompers as an "operating motivation" and "a deeply held commitment to radical change" that resulted in his leading the AFL to "a class movement" that was within the Marxian tradition (pp. 217, 219, 222). The thesis is not convincing in either evidence or argument.

LOUIS L. ATHEY
Franklin and Marshail College

WILLIAM C. WRIGHT. The Secession Movement in the Middle Atlantic States. Rutherford, N.J.: Fairleigh Dickinson University Press. 1973. Pp. 274. \$15.00.

Dr. Wright's book examines support for secession in Maryland, Delaware, New Jersey, Pennsylvania, and New York between Lincoln's election and the firing on Fort Sumter. It finds that a few secessionists wanted their state to join the Confederacy, more proposed establishing a central confederacy with the western border states, and most simply favored letting the South go in peace. Almost all secessionists were Democrats, residing either in slave or onetime slave counties or in cities with substantial economic and social ties with the South. They failed to see any of their ideas adopted in their states because they lacked effective leadership, failed to do more than speak for their cause, and waited for their neighboring states to act. In the end they succeeded only in giving the Confederate leaders a false impression of substantial Northern support and thus encouraged them to seize federal property and to fire on Fort Sumter. Ironically, these acts undermined peaceful secession in the North and especially intimidated Maryland and Delaware secessionists, who feared secession by their states would turn them into bloody theaters of war.

Wright develops his thesis by examining the statements and voting records of the governors, state legislators, congressmen, and newspapers of each state. His chapters on Maryland, Delaware, and New Jersey are his strongest, and his conclusion that Maryland Governor Thomas

Hicks supported a central confederacy is a fresh and convincing contribution. Running through his chapters on Pennsylvania and New York, however, is a disturbing confusion of those who opposed coercing the South with those who favored peaceful secession. Now, as David Potter has shown, these two positions were fundamentally different. Many Northerners opposed coercing the South because they believed that without hostile provocation the secession movement would fail politically in the cotton states. Those noncoercionists were anything but secessionists; yet Wright says they were. He disposes of Horace Greeley's attitudes toward secession by quoting one letter from Greeley to Lincoln and one to William Herndon and by noting in a footnote that Professors Potter and Thomas N. Bonner have disagreed on the subject. Other examples of this kind of thin, uncritical research occur often enough to raise doubts about the book's value. Indeed, if Wright's general conclusions were not so thoroughly predictable and so many of his quotations so clearly unambiguous, one might recommend that scholars disregard his book altogether. As it is, this book should serve as a starting point for anyone wishing to explore the subject's intellectual and political complexities.

GEORGE T. MCJIMSEY
Iowa State University

MARILYN MCADAMS SIBLEY. George W. Brackenridge: Maverick Philanthropist. Austin: University of Texas Press. 1973. Pp. xiii, 280. \$8.50.

Pleasingly written is this detailed account of the career of George W. Brackenridge, banker and philanthropist of San Antonio. From Indiana Brackenridge had come to Texas at the age of twenty in 1853, but he left the state as a Unionist early in the Civil War and returned in the capacity of United States Treasury agent to the Rio Grande area. Here along the river he engaged in wartime cotton trade. Later Brackenridge settled in San Antonio where he founded a bank and invested in a utility company, a newspaper, and other enterprises. Brackenridge turned from business long enough to become president of the San Antonio school board.

The author tells of the financier's leadership as a regent of the University of Texas, which office he filled from November 1886 to January 1911, only to return to that position in 1917 for two more years. As regent he aided in collecting back rents on University of Texas

properties, and he placed these on a paying basis. His benefactions to the University of Texas included a dormitory for men, money for the founding of the school of home economics, a loan for women students in architecture, law, and medicine, and a final gift for the school of 500 acres of land on the Colorado River in Austin. Brackenridge proposed that the main campus of the University of Texas be moved to this land, and he was disappointed when his proposal met defeat. Still his loyalty to the university continued. Brackenridge agreed, if necessary, to underwrite the expenses of the school for the next biennium out of his own funds after Governor James E. Ferguson vetoed the university appropriation bill for 1917-19. Fortunately, Brackenridge did not have to make this sacrifice. He contributed to other educational purposes as well, and these included four school buildings in San Antonio, a college for Negroes in Seguin, Texas, and money to women medical students at Galveston.

Perhaps the people of San Antonio remember Brackenridge most for the two parks, one of which bears his name, that this man who never married donated for the youth of the city.

This commendable book is well documented and indexed.

WILLIAM CURTIS NUNN
Texas Christian University

SAUL SIGELSCHIFFER. The American Conscience: The Drama of the Lincoln-Douglas Debates. New York: Horizon Press. 1973. Pp. viii, 15-488. \$12.95.

Since 1958, when Paul M. Angle published his superb edition of the Lincoln-Douglas debates to commemorate the centennial of the encounter, a number of studies of the confrontation between Abraham Lincoln and Stephen A. Douglas have appeared, including Harry Jaffa's in-depth interpretation of the issues of the debates, Robert Allen Heckman's descriptive account of the 1858 campaign in the context of state and national politics, Don Fehrenbacher's fresh analysis of Lincoln's role in the 1850s, and, from the Douglas side, the recent works by Damon Wells and this reviewer. To these accounts Saul Sigelschiffer's The American Conscience adds very little.

About half of the book deals with the background and aftermath of the debates, as well as the characters and careers of the two debaters; the other half focuses on the debates themselves. Each of the seven joint debates is dealt

with in a separate chapter. Believing that the two rivals should speak for themselves as much as possible, Sigelschiffer quotes frequently and extensively from their statements. He has given some attention to contemporary newspaper reaction and to the activities of the candidates as they journeyed from one debate site to another, relying heavily on Angle's edition, the highly useful 1908 collection of Edwin Erle Sparks and a scattering of older secondary works and reminiscences. In contrast with the epic qualities that the author ascribes to the debates (and with the "drama" promised in the book's subtitle), the discussions are rather flat and the chapters assume an almost mechanical sameness.

Sigelschiffer's evaluation of the debates in the perspective of American history and his assessment of the two protagonists may well raise some eyebrows. The conscience of America, he writes, "could not rest with the paradox of liberty and slavery living side by side." The Lincoln-Douglas debates, he believes, were the high point in this struggle of conscience, a turning point for the nation. "Since the biblical struggle between David and Goliath, few personal encounters have been as fateful to a nation as to the combatants themselves." While his claims for the debates border on the extravagant, the author's discussion of the issues on which they were based is over-simplified to the point of distortion. He has made little use of the tremendous amount of scholarship on the sectional conflict, holding tenaciously instead to views that have long been challenged and even discredited by historians.

Sigelschiffer is lavish in his praise for Lincoln. Lincoln, he suggests, was the instrument of God to destroy slavery; for years "he pondered the riddle of slavery" and then emerged from obscurity at the right moment to play his role. The Declaration of Independence is "the reflection of the conscience of America" and Lincoln has been its foremost interpreter, dedicated to the achievement of its ideals, a principal actor in the great laboratory for freedom that was, and is, the United States. His only shortcoming, his "Achilles heel," the author believes, was his failure in 1858 to accept all the implications of his moral opposition to slavery. Although Sigelschiffer is often ambivalent in his judgment of Douglas, he regards the "Little Giant" as basically misguided and mistaken; Douglas's tragedy "was moral failure." His program would have reduced the Declaration of Independence to ashes and snuffed out this beacon of inspiration. Douglas, Sigelschiffer charges, failed to comprehend the true nature of his country's destiny and contributed to the breakup of the Union by convincing Southerners of their peril if a Republican should be elected in 1860. The South, he contends, was in the grip of a small clique of unscrupulous slaveholders for whom democratic government was of no consequence. Lincoln recognized the true character of this clique while Douglas became its too. The author clearly writes from strong conviction but his assessments are less than convincing.

ROBERT W. JOHANNSEN University of Illinois, Urbana-Champaign

WILLIAM E. PARRISH. A History of Missouri. Volume 3, 1860 to 1875. [Columbia:] University of Missouri Press. 1973. Pp. vii, 332. \$9.50.

The period covered in this fine history of an important border state is from the outbreak of the Civil War until the end of Reconstruction in Missouri and the defeat of the Liberal and Radical Republicans. Although the Look lacks footnotes, it is based on massive research and is bulwarked by a large annotated bibliography.

The Civil War, which assumed a sectional character in most areas, was truly a civil war in Missouri. Settlement patterns had inextricably mixed slaveholders with German Racicals and other free-soilers. These neighbors now met to settle political differences on bitter battlefields or in the ambush of guerrilla warfare. All of the range of political divisions on the national scene could be found in concentrated form in Missouri, but in addition Missouri had the problem of governing itself when the governor and legislature had fled the state. Besides the division over the nature of the Union and of slavery there were divisions within Northern and Southern ranks. Union men were divided over how to get rid of slavery, and Southerners. were split over its support and where to fight the war. The quarrels in the Union ranks between the Charcoals and the Claybanks sorely tried even the political genius and patience of Lincoln and eventually led to a split among the victors after the war. Passions aroused by the terrible nature of guerrilla warfare in Missouri or by the war itself were slow to cool, and Reconstruction in the state reflected attempts by the Radicals to secure the peace. Proscriptive measures against rebels and traitors failed, and the lasting monuments of the Radicals were in education, suffrage, and the econ-

Missouri's history during the troubled times of the Civil War and Reconstruction, perhaps more than that of any other state, offers in concentrated and acute form the national divisions. There, too, battles and other military measures, including military government, were simply politics in another form. Thus, the story deserves the close attention that the writer has given it. But he has not neglected other aspects of Missouri's history, such as the growth of cities, economic development, the fate of the blacks, cultural life, and life in general under the pressures of bitter strife. This excellent study deserves emulation in other states.

RODNEY C. LOEHR
University of Minnesota,
Twin Cities

JOHN W. BLASSINGAME. Black New Orleans, 1860-1880. Chicago: University of Chicago Press. 1973. Pp. xvii, 301. \$9.95.

What he did for the bondsman in The Slave Community: Plantation Life in the Antebellum South (1972) John W. Blassingame now does for the freedman in Black New Orleans. Blending historical and sociological perspectives, and drawing with skill and imagination upon a variety of sources, he offers fresh insights into an oft-studied period of Southern history. His setting in this volume is Reconstruction, but, as before, his subject is the quality of black life and the nature of black institutions. Black political aspirations are not ignored, but the emphasis is on "those areas of life-education, family, religion, social and economic activities-which were of more immediate concern to blacks than politics" (p. xv).

Crescent City blacks were in most respects the region's most advantaged, yet they too staggered into citizenship under the burden of slavery and antebellum proscriptions. Not remarkably, theirs was a mixed record of achievement during the two decades following the outbreak of of the Civil War. Reconstruction brought few lasting political benefits; but in other areas, the author concludes, these were for blacks years of significant change and modest success. They "quickly learned the responsibilities of free labor and managed to compete successfully against whites in many areas of economic life" (p. 49). Racial discrimination and a generally depressed New Orleans economy conspired severely to handicap most freedmen. But a few blacks became wealthy merchants and real estate brokers and many more became highly skilled tradesmen. During Reconstruction New Orleans blacks also overcame "slavery's legacy of immorality and instability" and developed patriarchal families "almost as stable" (p. 79) as those of whites. Eager to improve their lot they organized schools, churches, orphanages, mutual aid societies, and social and literary clubs. While their demands for equal access were often frustrated, blacks enjoyed greater recourse to public accommodations in New Orleans than in any other Southern city. Segregation was widely practiced, but schools and housing patterns were integrated and interracial sexual contacts numerous. Thus Reconstruction created what Blassingame aptly terms a "mosaic in race relations without design" (p. 173), and it "laid the foundation in New Orleans for Jim Crow's strangest career" (p. 217).

Although specialists will find few surprises in this brief study, it is nonetheless a welcome contribution. Dispassionate and well written, based on extensive research, it contains copious notes, numerous tables, a functional index, and a selective bibliography. In both time and place the author has chosen an extraordinarily revealing vantage point from which to view his subject. He is doubtlessly correct in believing that his work provides a standard by which to measure black life in other Southern cities.

NEIL R. MCMILLEN
University of Southern Mississippi

G. R. TREDWAY. Democratic Opposition to the Lincoln Administration in Indiana. (Indiana Library and Historical Board.) [Indianapolis:] Indiana Historical Bureau. 1973. Pp. xv, 433. \$10.00.

For students of Civil War politics this is an important book. The effort here is to examine the origins and sources of Indiana's Democratic opposition to the Lincoln administration, to probe that opposition's action, and to develop a clearer understanding of wartime secret societies, the much-controverted Northwest Conspiracy of 1864 and the actual functioning of a military commission. In this effort the author succeeds admirably.

This thoroughly documented study (118 pages of notes) reveals clearly the bases of Democratic dissatisfaction with administration policies—notably, adding emancipation to the war's original purpose of saving the Union, the army draft, suppression of dissent, suspension of habeas corpus, subsequent arbitrary arrests, and military trials of civilians. "That ephemeral and elusive order, the Knights of the Golden Circle," is examined and exonerated, although the later secret societies, Order of American

Knights and Sons of Liberty, do not come off so cleanly.

The evidence offered here demonstrates convincingly that Indiana Democrats were for the most part loyal citizens willing to support the war to restore the Union they had known and loved. They were less than eager for emancipation that might flood their state with Southern Negroes. Repressive administration tactics generated a resistance of its own or, as the author puts it, "restoration of the Union was actually jeopardized by the policies" of suppressing free speech, press, and open criticism. In Indiana Colonel Henry B. Carrington, the adjutantgeneral, grossly exaggerated for his own ends the dangers of subversion and greatly overacted in dealing with them. The author shows that although often distorted in historical writings, the Northwest Conspiracy of 1864 was a reality, but it dwindled as a potential danger when responsible Democrats dropped away from it on learning of Confederate participation. Leaders who were arrested in Indiana and tried by military courts, including the legendary Lambdin P. Milligan, of whom this work gives full coverage, were convicted out of prejudice and not on the evidence.

Tredway's presentation and conclusion essentially reject the more traditional picture of disloyalty on the part of Democrats offered in George F. Milton's and Wood Gray's works as reflecting contemporary Republican distortions. Some serious questions are raised about James G. Randall's treatment. The author applauds Frank Klement's fairer, more judicious view, although he thinks Klement dismisses the later secret societies too lightly. This is a solid, well-developed study that deserves the attention of all students of Civil War politics.

DAVID LINDSEY
California State University,
Los Angeles

JOHN NIVEN. Gideon Welles: Lincoln's Secretary of the Navy. New York: Oxford University Press. 1973. Pp. xii, 676. \$17.50.

The subtitle of this book may have prompted the AHR editors to invite a review by a naval historian. The naval aspects of the study, however, prove to be secondary. Rather it is an excellent political history from Andrew Jackson through the agonizing term of Andrew Johnson played against a backdrop of the life and public career of Gideon Welles.

Overshadowed by Chase, Seward, and Stanton in the Lincoln cabinet, and in his own depart-

ment by Gustavus Fox, the flamboyant assistant secretary of the navy, Welles has emerged in historical literature as a nobody who appeared from nowhere; served Lincoln faithfully, if not with marked distinction; and silently returned from whence he had come. Professor Niven's sparkling scholarship sounds the death knell to this simplistic myth.

In his native Connecticut, Gideon Welles was a veteran of local and state political infighting long years before he came on the Washington scene. He was an oddity with curled wig and flowing white whiskers. The incisive, logical, and frequently vitriolic editorials written by Welles for the Jeffersonian Hartford Times under the guidance of his mentor, John Milton Niles, were a major factor in shaping the political thinking and actions of the Connecticut electorate.

A political radical and staunch Jacksonian Democrat, Welles fought the banks with a fervor worthy of the old Hero of New Orleans. He would ultimately abandon the Democrats on the slavery issue and become a founder of the Republican party. Welles was a rigid idealist who was incapable of accommodating political theory to stark reality. He held the truth, as he saw it, to be self-evident. At the same time, Welles was also a wire-puller and manipulator of the first order, a man with strong personal ambition and an adept patronage jobber for party faithful and himself. This is the profile of Welles that comes through clearly in the Niven book.

Welles's first federal appointment was as postmaster of Hartford. Then from 1846 to 1849 he was chief of the Navy's Bureau of Frovisions and Clothing; an experience that stood him in good stead when he assumed wartime leadership of the department.

The author brings us to Gideon Welles as secretary of the navy and Lincoln's New England man in the cabinet after more than 320 pages. Treatment of naval administration and operations is highly selective, but perhaps adequate for a study not intended to focus on naval history. Mr. Niven seems unjustly critical of Du Pont and other senior commanders, and he demeans certain naval actions For example, he dismisses the Hatteras Inlet operation, the initial morale-building Union victory after the first Bull Run debacle, and one which sealed off blockade running from Pamlico Sound, as a trifling affair. Several factual errors creep into the naval coverage, but nothing really damaging. I did gain the firm impression that, while writing, Mr. Niven issued a long sigh of relief whenever he could put aside naval matters and return to the intrigues of the more familiar and comfortable political arena.

Welles stayed on as secretary of the navy through Johnson's presidency. One would scarcely be aware of that fact from reading this book. Rather than the multiple problems of demobilization and of restructuring the navy to a peacetime posture, Mr. Niven concentrates his final chapters on Reconstruction, the president's clash with congressional radicals, and the ultimate impeachment proceedings.

While Gideon Welles may never have been in total oblivion, he was in a limbo from which Professor Niven has masterfully rescued him and placed him in proper perspective among the opinion-makers and influential political figures of the nineteenth century. Still, if anyone wishes to look more deeply at the Civil War navy department and the conflict at sea, he must turn elsewhere.

WILLIAM JAMES MORGAN U.S. Department of the Navy

THOMAS LAWRENCE CONNELLY and ARCHER JONES. The Politics of Command: Factions and Ideas in Confederate Strategy. Baton Rouge: Louisiana State University Press. 1973. Pp. xv, 235. \$10.00.

In the years since our Civil War, scholars and buffs have mined much rich ore from this great national tragedy, and significant works continue to appear. New scholarship and research techniques bring fresh interpretations with each succeeding generation. Special interests continue in the Confederacy's "tattered flags," and in the leadership that failed before the challenges of a horrible war.

Few scholars bring better credentials to the study of Confederate leadership than do Archer Jones and Thomas L. Connelly. In Confederate Strategy from Shiloh to Vicksburg (1961), Jones broke important ground in his analysis of the South's command system during the first two years of the war. Connelly's monumental two volumes on the history of the army of Tennessee also suggested important fresh interpretations of Confederate leadership. In The Politics of Command, the two men combined talents to bring other new views of Confederate decision making at the top, with an emphasis upon a continuing leadership crisis in the South during the war.

The scholarship that marks their cooperative venture shows dependence upon several disciplines for their conclusions: psychology as they probe personalities and ask why men acted as they did; sociology, in an inquiry into the particulars of Southern society that caused disagreements among the leaders; political science, in an analysis of the relationships among departments in a government shackled with an overdose of states rights; military science, in an explanation of the complexities of policy and strategy within the command; and history, as they trace the sequence of selected events, influences, and decisions of the war years.

From its beginning, the Confederacy was honeycombed with personality clashes, political intrigue, and jealousies among the leaders and those who sought to lead. Nothing was so exasperating to the Confederacy's hopes for success than the internal dissension, especially the "continuing debate over war policy, particularly in the Confederacy's proper strategic course" (p. x). Through it all, a harried leader moved from crisis to crisis, his key decisions often unpredictable in the face of conflicting influences and counsel.

Jones and Connelly identify five important and often competing influences that played upon President Jefferson Davis and contributed to his indecisive leadership. These were European military ideas, especially those of Henri Jomini; the views of General Robert E. Lee; the opinions of a "western bloc" that was usually dominated by General P. G. T. Beauregard; those of a "broad network of informal associations within the Confederacy"—political blocs that brought pressures to bear upon singular interests; and finally, Davis's own views that usually focused upon a departmental system around which he shaped his strategy—instead of basing his organization upon a strategy.

The president is not the only Confederate leader to fall before this assault of new criticism. Generals Bragg, Polk, Breckinridge, and Longstreet also feel the pinch. Supporters of Robert E. Lee will shudder at the image of their hero—a general obsessed with Virginia, possessed of a naive provincialism best illustrated by his narrow loyalty to region instead of nation, and myopic in his application of the strategies of Jomini and Napoleon Bonaparte in his military actions. These authors also take issue with the contention that Lee's lack of power was responsible for poor decision making at the top, suggesting instead that Lee as one of the leading advisers to the president must share blame for poor and narrow decision making.

Meanwhile, General Beauregard emerges as the most persistent advocate of a broad national strategy and policy, more congruent with modern warfare than the more provincial views of Lee. In his actions, though, the Creole general is not above reproach, for he often stretched his good plans too far for the resources available. Despite this visionary weakness and a personal antagonism with Davis, Beauregard finally gained the president's support late in the war to extend the war to a broader front.

The strengths of this book are many. It is an excellent survey of the Confederate decision-making process, and offers an impressive summary of the writings of Jomini and Napoleon. It identifies new and important influences that faced the president and his advisers, especially those from the too-often neglected Western front. The book outlines the problems of a complex departmental system across the broad Southern front and suggests human weakness in this system as important to the final outcome as logistical and strategic considerations.

These criticisms are well made and most persuasive when considered in terms of modern warfare. Concerning the Confederacy, one important question remains: how did it survive and offer such spirited resistance for four terrible years under such leadership? While torn constantly by conflicting and divisive decision making, it also must have possessed some mysterious and mighty elan that held the "Johnny Rebs" together. Certainly, this cohesiveness was, at least in part, a result of persuasive leadership. Leaders, North and South, paid a heavy price for inadequate training. Many of them used the war for personal gain and to promote narrow objectives. But others sacrificed property, reputations, and even their lives to lead, as best they could, a cause they believed to be right. The ultimate challenges of the war may have been too much for mortal men.

This is a timely study of leadership in a nation beset with leadership problems and concerned with the question of integrity in crisis decision making in the highest echelons of government. The Politics of Command also points up a need for other studies of leaders and leadership, including a similar one on decision making in the North. It also suggests the need for a new definitive biography of Jefferson Davis, and it is hoped that one will come from the Papers, now being published by Rice University.

ROBERT HARTJE
Wittenberg University

JAMES A. WARD. That Man Haupt: A Biography of Herman Haupt. Baton Rouge: Louisiana State University Press. 1973. Pp. xvi, 278. \$11.95.

The small part of American Civil War history that is not devoted to military or political events frequently emphasizes the railroads as a new factor in warfare. "That man Haupt," President Lincoln exclaimed upon viewing one of the first great examples of military railroad engineering, "has built a bridge across Potomac Creek . . . and there is nothing in it but bean poles and cornstalks." He was referring to Herman Haupt, chief engineer of the Pennsylvania Railroad and superintendent of the Northern Pacific Railroad during their most critical phases, first man to try to bore a tunnel through the Hoosac, organizer of the northern railroads into an effective strategic force at the start of the war, rugged individualist investor in numerous unsuccessful ventures, and one of the first and most successful consulting engineers in America. Professor Ward has written a biography of Haupt based on original documentary materials, and it will be read eagerly by many who have wanted an answer to the question of why this enigmatic man, who got in so firmly on the ground floor of railroacing, just missed greatness in its history.

The man who emerges from this book spent his energies on a bewildering succession of enterprises, many of which he was poorly prepared for, and in few of which he evidenced the talent for compromise that was the key to success in the large-scale enterprises that arose after 1850. For the Pennsylvania Railroad, Haupt relocated large sections of the faulty original survey, built major bridges and tunnels, and drafted a plan of organization when it was ready to operate. That the first modern economists were engineers is revealed in the fact that Haupt worked out data separating fixed from variable costs of railroading, which convinced him that the early concept of the railroad as a high-priced, low-volume mode of transportation was wrong, and that a policy of low rates would enable the railroads to achieve their true destiny as haulers of virtually all the nation's burdens and to maximize profits at the same time. Finding himself ahead of his time on the Pennsylvania, he proceeded to lose nearly everything he had on a contract to build the Hoosac tunnel, a costly project intended to give Boston a second trunkline railroad to the West. Except for his demonstration that running a railroad, in time of peace or war, was no fit task for men in uniform with military mentalities, and a two-and-one-half-year tenure as superintendent of the Northern Pacific during the years when it was completed to the Pacific Coast, Haupt's career thereafter seems to have been a succession

of hare-brained ventures on which he netted huge losses.

This book is something of a disappointment, and perhaps not just because Haupt's career fell so far short of what it might have been. Ward's sources, which include a body of forgotten Haupt Papers at Yale and valuable materials in the possession of one of his grandchildren, are basic, but while they yield a story that is rich in controversy and the details of inane business schemes, there is not nearly enough on Haupt's solid accomplishments. A major gap in the sources is the records of the Northern Pacific, which are readily available at the Minnesota Historical Society and would have added much on Haupt's contribution to this badly managed railroad during its Villard period. Even so, in an era in which railroad history has suffered from the transmogrifications of the various new schools, it is reassuring to find scholars still writing books on the subject that are honest, professional, and useful as far as they go.

ALBRO MARTIN
American University

ERNEST N. PAOLINO. The Foundations of the American Empire: William Henry Seward and U.S. Foreign Policy. Ithaca: Cornell University Press. 1973. Pp. xii, 235. \$9.75.

Since I am not a diplomatic historian, I was the more grateful for R. M. Abrams's "United States Intervention Abroad: The First Quarter Century," (AHR, 79 [1974]: 72-102). It eased greatly my task of reviewing briefly yet adequately Professor Paolino's significant Foundations of American Empire. I refer readers to the Abrams survey and turn to other aspects of the Paolino volume. Paolino's judgment is that the first quarter century of sustained American intervention abroad began not in the late 1890s but in the early 1860s. Quietly and impressively, though too sparsely at almost every point, Paolino sketches in his estimate of the Civil War and Reconstruction as a commercial expansion watershed. He connects Seward's State Department aspirations and operations during 1861-69 to a consistent emphasis on overseas ventures involving, as examples, creation of a global telegraph link and a unified world coinage by Washington officialdom and mercantile and financial makeweights. These efforts failed. Spanning continents, Paolino traces the more effective policy alternatives-Alaskan and insular coaling station acquisitions, isthmian canal interests, and a sometimes bellicose search for trading privileges in Japan to which Seward and his coadjutors also turned their attention.

I am impressed, as Paolino is, by Seward's educability. The secretary dropped his prewar rhetoric about Canadian territorial annexation. He quickly sensed potentialities in the new telegraph technology for the swift transmission of intelligence (concerning which, L. Lindley's 1973 Rice dissertation on the constitutional relationships of government and the telegraph would have served Paolino very well). A brilliant anticipator of future American foreign policies, Seward's achievements and frustrations, according to Paolino, must be measured in light of "the limited materials and opportunities at his [Seward's] disposal" (p. 212). A sound point. Certainly the State Department's miniscule staff was an unlikely machine for achieving grand goals. But Seward's generation would not have marveled much at the apparent disparity between his ends and means. His contemporaries did not assume that government must carry the whole burden of public policies or that government officials should function isolated from market-place entrepreneurs.

As Lincoln's first internal security administrator and as Stanton's cabinet colleague, Seward was intimately familiar with the capacity of more-or-less private associations including the Christan and the Sanitary Commissions and the Freedmen's Bureau field staff to advance public and private interests. Expedients like these avoided easier options of wholly public administration involving large, permanent, costly bureaucracies. Such options were unpalatable to men of Seward's time. They were busily involved in the energy releases that Willard Hurst has described so well, employing as little public sector machinery as possible; less than now seems to have been possible or perhaps even desirable by our standards.

Of course Seward's dreams as secretary had to be placed in the Civil War and Reconstruction context. Only when and if Union victory occurred could Seward win. Paolino provides a substantial service in directing attention to aspects of Lincoln-Andrew Johnson history too little studied. But I fear also that Paolino has exploited too little the large literature on the Civil War and Reconstruction, especially recent reconsiderations of social organization, public administration, and the limitations of political commitment. These estimates make dubious his concluding suggestions about the impediments that the war and Reconstruction

placed in the way of entrepreneurial organization and government-functional outreaches.

Nevertheless Paolino's Foundations possesses large assets. Blessed with reasonable internal logic and unshrill tone, nourished by impressive archival documentation, and sparked by wide-ranging perceptions, the book deserves respectful consideration.

HAROLD M. HYMAN Rice University

LOUIS S. GERTEIS. From Contraband to Freedman: Federal Policy toward Southern Blacks, 1862-1865. (Contributions in American History, number 29.) Westport, Conn.: Greenwood Press. 1973. Pp. xii, 255. \$11.50.

How much change did former slaves experience as a result of federal policy during the Civil War? To explore the issue Professor Gerteis presents chronological accounts of policy development in the three major areas of Union occupation: Virginia and the Carolinas, Louisiana, and the Mississippi Valley. Thus his volume becomes the most detailed study of a topic previously considered by several historians including Bell Wiley, Benjamin Quarles, and Willie Lee Rose.

Official Union attitudes toward defecting slaves did not evolve smoothly, but rather amid disputes between the War and Treasury Departments, among military commanders, and between freedmen and their emancipators. Reformers desired land redistribution to create a stable economic base for freedmen. If property could not be acquired, blacks hoped for the partial independence of sharecropping. The author concludes, however, that the federal government essentially sought to mobilize exslaves as laborers and soldiers and to avoid disruptive change, especially in Virginia and Louisiana where some planters had declared their loyalty to the Union. Even government direction and protection of labor systems for freedmen declined when military needs ended with the war, although the Freedmen's Bureau continued to promote the wartime patterns of wage labor and sharecropping during Reconstruction.

The author's analysis appears generally sound, although a few specific conclusions seem open to debate. He suggests that "blacks on the [South Carolina] Sea Islands fared no better than the freedmen on Virginia's Eastern Shore" (p. 50), despite evidence that former slaves on the Sea Islands did acquire more land during the conflict than those of the Eastern Shore.

The notion that wartime federal policy foreshadowed the lack of postwar land reform represents a subtle shift of emphasis from the interpretations of LaWanda Cox, James Mc-Pherson, and William McFeely, who believe Andrew Johnson extinguished that hope during the early Reconstruction years. Since economic status is crucial to social change, and since previous works have covered the subject of black troops, it seems reasonable for the author to emphasize federal labor policy. Yet the description of educational and religious efforts by freedmen's aid societies as "inconsecuential" (p. 183) appears too harsh, especially in view of recent debates over the psychological impact of slavery. By contrast, John Blassingame in Black New Orleans, 1860-80 (1973) ranks education as the greatest advantage gained by blacks from the war.

With this well-researched contribution to our understanding of the emancipation process, Gerteis joins other recent students of the period who see crucial limitations within Northern attitudes that assured the failure of fundamental reform.

ALWYN BARR
Texas Tech University

MILTON LOMASK. Andrew Johnson: President on Trial. Reprint; New York: Octagon Books. 1973. Pp. viii, 376. \$14.00.

The 1973 reprinting of Milton Lomask's thoughtful and highly readable study of Andrew Johnson's presidency is obviously timely, inasmuch as a host of Americans today are eager to discover how closely Richard Nixon's position in 1974 approximates that of the Democratic chief executive from Tennessee in the early months of 1868. In actuality the differences between the two cases appear to be more pronounced than the similarities. Certainly the author's swift-paced narrative indicates that Johnson's impeachment rested more directly upon the political skill of his principal adversary, "Old Thad" Stevens of Pennsylvania, than upon the president's own words and deeds. No observer of the scene today, however, has publicly attributed Nixon's situation to his political inexperience or to a naive faith in the fair-mindedness of his fellow citizens. To historians of the Reconstruction period nevertheless, more interesting than the somewhat farfetched analogies between two presidents whose characters had so little in common are the arguments arising from the relatively recent re-examination of Johnson's attitude toward Negroes and bills that might conceivably have ensured freedmen in the South both civil rights and political equality before the end of the 1860s.

Andrew Johnson's failure to offer positive leadership in effecting social reform in the former Confederate states while he still had some control over Reconstruction constitutes the major charge against him in the view of his present-day critics. Nothing in Lomask's text supports so severe an attack on the performance of the "Tennessee mudsill" in the White House. On the contrary, the author presents him as a scholarly upholder of the Constitution, a man who believed firmly in the principle of leaving to the states all powers not explicitly vested in the federal government. Lomask portrays him as stubborn and frequently maladroit, but never malicious. Although he disapproved of the Black Codes enacted during 1865 and 1866 in state after state of the former Confederacy, he remained silent when a rebuke from him might have induced white leaders in the South to recognize blacks as fellow citizens. He won the dubious distinction of being the first American president to veto bills that House and Senate promptly passed over his veto. But at every turn his tactless pronouncements, his deeds, and his omissions stemmed not from self-aggrandisement but from convictions that he was protecting the interests of his countrymen as a

The lack of explicit documentation for some of the author's judgments may trouble the avid seeker for truth, but on the whole that fault is infrequent in a book manifestly designed for others besides professional historians. The weakness, moreover, is largely offset by the numerous adroitly drawn vignettes of the main actors in this drama. Real people walk across every page encouraging every reader to form his own opinion of who was hero, who was knave, who was some of both.

CONSTANCE MCLAUGHLIN GREEN Washington, D.C.

PEGGY LAMSON. The Glorious Failure: Black Congressman Robert Brown Elliott and the Reconstruction in South Carolina. New York: W. W. Norton and Company. 1973. Pp. 330. \$7.95.

This study of Robert Brown Elliott is a compassionate, forceful, and lucid political examination of one of the more extraordinarily active of the black Republicans of the Reconstruction period. Elliott served in many capacities and fought many political skirmishes and wars as a member of the South Carolina Legislature, during which he eventually became Speaker of the House; in the U.S. House of Representatives—the first full-blooded black; as a member of various state assemblies and national conventions; and as a lawyer on the local scene. He died young, in the exiled state of Louisiana, without power and financially driven. He had been deeply involved in the complex racial and political issues of the era; his epitaph, says the author, was a "glorious failure."

Lamson wrote this work with an increasing admiration for Elliott, for his life and pain, although she occasionally dips into his shortcomings, errors, and financial machinations. Of importance, she tackles the problem of Elliott's origins. Contemporary accounts and Elliott himself claimed Massachusetts for his birthplace and England for his educational background. Lamson incisively postulates, however, that he may well have been born and educated in England, that he never became an American citizen (thus he would be "the only British subject ever to be a member" of the U.S. House of Representatives), that he had been trained as a printer, and had served in the British navy. He may well have entered the country by jumping ship in Boston.

Once ensconced in South Carolina, Elliott plunged into Reconstruction politics and immediately assayed the situation for blacks. At the state constitutional convention in 1868, he led the fight against the poll tax and the literacy clause, correctly sensing the techniques by which blacks would be disenfranchised. After a stint in Congress, becoming known as a powerful speaker, he resigned and spent the bulk of his energies helping to put into office a reform governor and constructive policies. Appointed speaker, he used that office to stock the committees with able individuals, some of them white and Democrats. Consequently, he became one of the powerful blacks in the state. When the compromise was hacked out in Congress in 1877, Afro-American legislators in South Carolina, as elsewhere, found their unstable positions eliminated and their power emasculated. Despite Elliott's election as attorney general, he was mangled in the intricacies of state and national politics and was legally forced out by the redeeming Democrats. Elliott attempted, unsuccessfully, to recoup his power by acting as floor manager for John Sherman's candidacy at the Republican National Convention in 1880. By that time he was a special inspector of customs for the Treasury Department in Charleston.

The thrust of the book is focused upon the political infighting in South Carolina and the interrelationships between blacks and whites in their attempts to deal with the problems wrought by a sudden leveling of status. Similar to other blacks, by the end of his life Elliott "was all too aware that the revolution to which he devoted his life had faltered" (p. 290).

JOSEPH BOSKIN

Boston University

TOM E. TERRILL. The Tariff, Politics, and American Foreign Policy, 1874–1901. (Contributions in American History, number 31.) Westport, Conn.: Greenwood Press. 1973. Pp. x, 306. \$12.00.

Though he acknowledges his ties to William Appleman Williams, Professor Terrill has no apparent quarrel with Paul Holbo's judgment that "far more was involved in the tangle of tariff politics than a search for foreign markets." Carefully and clearly he shows the many facets of the tariff issue: high wages for American workers versus cheap goods for consumers, governmental stimulus to economic growth as against special largess for favored interests, ideological battles over the principle of laissez faire, the use of the tariff issue by party leaders to starve off divisive quarrels over the currency, even the simple need for each party to establish its own political identity. Indeed, one of the strengths of this thoroughly researched study is its presentation of the familiar reciprocityforeign-market theme in its full political context. There is no effort to oversimplify the story or to ignore any of its divergent aspects.

Nevertheless, among these various themes Terrill finds a "fundamental consensus among American leaders about American foreign policy and the relationship of the tariff to that policy" (p. 97) appearing in the 1880s and 1890s. Originally "a small group of Democratic leaders, guided by David Wells and Abram Hewitt, turned to a free-raw-materials strategy designed primarily to expand the foreign sales of American manufacturers. Republican leaders-first James Blaine, then Chester A. Arthur and Frederick T. Frelinghuysen, and later Benjamin Harrison-answered with their reciprocity strategy. They hoped to keep protectionism largely intact while expanding overseas" (p. 214). The opposing party positions came to full flower under the respective presidencies of Cleveland and McKinley. But the difference was only tactical: the common goal was the expansion of foreign markets through manipulation of the tariff.

One may pay tribute to the substantial merits of this relatively short but significant work while questioning some of its interpretations. If there was a consensus such as the author describes, it must have been a limited one. By Terrill's own account, the interest of most manufacturers in foreign markets was sporadic and transient until the great depression of the 1890s, while even after that 'congressional Republicans were only mildly inclined to deviate from rigid protectionism" (p. 201). With the death of William McKinley, the tariff reciprocity approach quickly lost strength, not to come into its own until Franklin Roosevelt's New Deal. In short, while Terrill convincingly elucidates the thinking of a number of important political leaders on the subject, he shows with equal clarity that the implementation of their ideas through legislation and treaties was fragmentary, short-lived, and of limited importance except as it revealed their general views on foreign policy.

Such matters, however, fall well within the normal range of scholarly disputation. It is perhaps more important to note that this book should be of value to scholars of widely differing views. It is impressively grounded in government documents and other primary sources and shows a good grasp of the partisan politics of the time. Clear and informative, it condenses a great deal of scholarship into a short compass; it is a major and welcome new addition to the literature of a difficult subject.

DAVID HEALY
University of Wisconsin—
Milwaukee

KENNETH J. HAGAN. American Gunboat Diplomacy and the Old Navy, 1877–1889. (Contributions in Military History, 4.) Westport, Conn.: Greenwood Press. 1973. Pp. x, 262. \$11.50.

This small book is the fourth study in the series known as Greenwood's Contributions in Military History. The author demonstrates the consistency of United States naval diplomacy in the pre-Mahanian era of wooden warships just before the new steel navy was built. The book's major theme is that United States naval operations from 1877 to 1889 were designed to develop a possible market for exports in underdeveloped or "semi-civilized" parts of the world. The author first discusses naval strategy as it was debated by three admirals, David D. Porter,

Stephen B. Luce, and Robert W. Shufeldt, and by a large group of younger and subordinate officers. While all participants in the debate held varying views on technological issues, they agreed that the basic mission of the navy was to show the flag and to protect American citizens, property, and commerce. The second part of the book is divided into chapters covering diplomatic events and encounters in various geographic regions. It traces naval operations in Liberia and the Congo, East African waters, the Indian Ocean, China, and Latin America.

Hagan stresses naval policy in Central America since the area reflected the national concern of the United States with any canal or waterway that might be built between the Atlantic and the Pacific. His chapter on Panamanian intervention in the 1880s is excellent and reveals much hitherto unknown data. It is shown that United States diplomats and navalists even hoped initially to penetrate the markets of Liberia and the Congo, but the thrust of European imperialism circumvented the effort as it did in several other remote regions. On the other hand, the small United States Asiatic Squadron off the China coast followed an interventionist policy and cooperated with European nations to protect Western lives and prop-

In one sense this study is an extension of the book by Charles Oscar Paullin, Diplomatic Negotiations of American Naval Officers 1778-1883 (1912), but it is more penetrating and interpretive. Highly readable and flawless, it is thoroughly researched from manuscript sources located largely in the National Archives and the Library of Congress and from numerous published sources. Its chief values are that it helps to fill the gap in our knowledge of diplomatic and naval history between the eras of the Civil War and the expansionism of the 1890s and to show that American isolationism was indeed a myth. The book has detailed footnotes, a selected bibliography, and an analytical index.

BENJAMIN F. GILBERT
San Jose State University

HERBERT APTHEKER, editor. The Correspondence of W. E. B. Du Bois. Volume 1, Selections, 1877–1934. [Amherst:] University of Massachusetts Press. 1973. Pp. xxv, 507. \$20.00.

Herbert Aptheker and W. E. B. Du Bois were related by friendship, sympathy, and circumstance, making it fateful that the one's papers should be edited by the other. This project

was begun in 1946 when Du Bois was seventyeight years old. They had kindred interests even then. Both were scholars of Afro-American history, persuaded toward a reinterpretation of American history through a radical and Marxian perspective. Through the next ten years, their shared experience as targets of Mc-Carthyism welded the bond even closer. Fellow historians were shamefully feckless to infringements of their civil liberties and academic freedom. The resulting isolation probably strengthened the resolve that Aptheker be the custodian of Du Bois's literary remains, even though that choice (because of Aptheker's continued radicalism) would make it harder to find sponsorship for the project. It is now twelve years since Du Bois's death, and the first of a projected three volumes of selected correspondence is in hand.

Aptheker has limited this collection to representative letters "having significant historical and public quality." The major issues are represented: Booker T. Washington, the NAACP, changing political positions, World War I, Pan-Africanism, and the depression. Since Du Bois's life was principally involved in public issues, such an emphasis seems reasonable. Yet it is just these matters that have been, heretofore, most widely published through Du Bois's scholarship, journalism, autobiographies, and novels. Those who have read widely will find little new in this volume. There are details that we would not otherwise have, of course. Letters of O. G. Villard and Arthur Spingarn carry the full weight of white condescension against which Du Bois struggled. Du Bois's own public image, his imperiousness as well as his integrity, is clear.

What has always been obscure, however—Du Bois's private and personal character—is nowhere revealed. Despite the fact that he lived apart from his family for long stretches of time, there is no correspondence with Nina Gomer Du Bois, his first wife. Except for three letters to his young daughter, there is no really private correspondence.

One must question Aptheker's equation of historical significance with public quality. That judgment has made this volume less important than it might have been. Since this vast collection, "tens of thousands of documents and letters," has remained in the editor's hands and out of the view of other scholars, and since his introduction says nothing about its contents, it is impossible to say how wise a selection this is. Aptheker should provide a full description of the contents of the collection,

and he must devote more than a scant paragraph to the rationale of his selection. Perhaps these omissions will be corrected in forthcoming volumes.

This is a welcome volume. Herbert Aptheker should be commended for his care and persistence. But after such a long wait, scholars must still see the full collection to find out more than they must know already.

NATHAN IRVIN HUGGINS Columbia University

CARTER E. BOREN et al. Essays on the Gilded Age. Introduction by JENKINS GARRETT. Edited by MARGARET FRANCINE MORRIS. (The Walter Prescott Webb Memorial Lectures: 7.) Austin: University of Texas Press, for the University of Texas at Arlington. 1973. Pp. 108. \$5.00.

This book is composed of four essays that were delivered as the Walter Prescott Webb Memorial Lectures at the University of Texas at Arlington. The essays are uneven in quality and interest. The first, written by Carter E. Boren, argues that Protestantism in America was "a unifying force in nineteenth-century American culture." Based almost entirely on secondary sources, the essay relies heavily on the work of Willard Sperry, William Warren Sweet, Sidney E. Mead, and Winthrop Hudson. Indeed, twenty-four of the eighty-seven footnotes refer to two books by Hudson. At best, the essay is a useful review and summary of some of the standard historiographical work on Protestantism in the nineteenth century. In the next two essays, Robert W. Amsler argues that science and technology had not opened up a new frontier by 1900 comparable to the disappearing geographical frontier, while Audra L. Prewitt explores the origins of the legal reformers who later participated in the Progressive Movement.

The last essay, entitled "America's First Environmental Challenge, 1865–1920," is the most interesting in the book. Based on primary research and an understanding of recent secondary literature, the essay also possesses the most value for historians. H. Wayne Morgan first summarizes the efforts of Americans to deal with the problems of air, sewage, water, and noise pollution as well as early attempts to understand the relationship between population and the environment. He concludes that Americans met their challenge with technical imagination and inventiveness, but that "the failure to question basic social values that promoted waste and inhibited social planning"

prevented any fundamental resolution of the environmental dilemmas.

The essays are brief and well written. They possess the virtue of dealing briefly with important topics that interest the historian and the general reader. But the second and third essays are too brief for their topics, and only the last successfully combines brevity and analysis to add to our understanding of the Gilded Age.

J. PERRY LEAVELL, JR. Drew University

STEPHAN THERNSTROM. The Other Bostonians: Poverty and Progress in the American Metropolis, 1860–1970. (Harvard Studies in Urban History, published in cooperation with the Joint Center for Urban Studies of the Massachusetts Institute of Technology and Harvard University.) Cambridge, Mass.: Harvard University Press. 1973. Pp. xvi, 345. \$12.00.

This new study of the common people of Boston, and especially their occupational career patterns between 1880 and 1970, is based on a careful, computer-assisted analysis of a sample of about eight thousand adult males drawn from such sources as federal census schedules, marriage license applications, birth records, and city directories. The resulting data constitute the longest and fullest series of mobility measurements yet published and provide a firm foundation for broad generalizations about the nature of societal processes in large metropolitan areas.

In his earlier study of Newburyport, Stephan Thernstrom was pessimistic about the economic opportunities available to laborers and their sons. In The Other Bostonians, however, he unveils a remarkably fluid city in which there was substantial and relatively constant upward intergenerational mobility. Within this broad pattern he finds important variations among various ethnic groups and between the races. Russian Jews achieved extraordinary success, surpassing even the Yankees in the second generation; the Irish and the Italians fared very poorly; black Americans remained mired in the weakest economic positions. In a useful final chapter, Thernstrom brings together the research of a number of other scholars in order to compare the Boston case with the national pattern. The Hub, it seems, was much closer to the norm than Newburyport.

As with most historical questions, the incompleteness of the data is a persistent problem. For example, the city directories yielded information only about the city of Boston and a few inner suburbs. This is an unfortunate and probably unavoidable circumstance particularly damaging in this metropolis surrounded by independent communities even before the turn of the century. In addition, there are the usual difficulties and compromises that beset any mobility inquiry—the inaccuracies of the sources, the incompatibility of the cohorts, the crudity of classification schemes, and the lack of information about skill levels. Suffice it to say that Thernstrom is completely open about his research problems and in fact usually anticipates objections or questions about procedure. On balance, his judgment appears to be excellent and his decisions entirely reasonable.

Despite its general excellence, the book suffers from an almost exclusive reliance upon quantifiable sources. Daily and ethnic newspapers, diaries, other forms of manuscript sources, and most types of government and business records were ignored. As a result, the text is primarily an elaboration and explanation of the eighty-one tables. Thernstrom conveys very little sense of Boston as a place; he does not mention its port, its neighborhoods, its transportation system, or its colorful political figures. By changing a few words, the subject could as easily have been Spokane or Buffalo.

To be sure, Thernstrom's purpose was to examine poverty and progress in "the" American metropolis and thus the search was for general patterns, not local peculiarities. He disclaims any intention of writing a popular book and argues that quantitative techniques are appropriate to the questions at hand. But one can concede those points and still insist that this is not a zero sum proposition. The decision to count need not be an exclusive research strategy, especially when, as in this case, so many of the conclusions have to be hedged because of small sample sizes and similar problems.

Within the limits he has set for himself, Thernstrom has written a superb book. It is the best and most ambitious analysis of social mobility yet to appear and will undoubtedly serve as a model for future studies. Not the least important of the lessons to be learned in its pages is that it is possible to move clearly, logically, and systematically through complex historical problems.

KENNETH T. JACKSON Columbia University

WILLIAM W. SAVAGE, JR. The Cherokee Strip Live Stock Association: Federal Regulation and the Cattleman's Last Frontier. [Columbia: University of Missouri Press.] 1973. Pp. 154. \$8.50.

An Oklahoman once described his state as "shaped like a heavy-handled pistol pointing west." If he carried this simile further, the area known as "No Man's Land" would form the barrel of the revolver, the Cherokee Nation in the upper right-hand corner would form the hammer, and the Cherokee Outlet would form the cartridge chamber or cylinder. The 6.5 million acres of land situated between the 96th and 100th meridian was deeded to the Cherokee Nation by the federal government in 1819 as an outlet to the West. Since the sixtymile-wide belt of prairie grasslands had been and would remain the hunting grounds of various tribes of the Southern Plains, the Cherokee found it worthless until cattlemen from Texas and Kansas moved in during the post-Civil War decades.

At first, the Cherokees collected a small toll for each longhorn steer driven up the Chisholm and Western trails, which bisected the Outlet enroute to Abilene, Dodge City, and other Kansas cattle towns. As the markets at the railheads became saturated and cattle had to be held over, ranchers resorted to grazing their herds in the Outlet immediately south of the Kansas line. The Cherokees assessed an annual fee of fifty cents per head, but the task of collecting it from all of the ranchers proved impossible. Eventually, the Indians leased the entire area to an organization of Westerners known as the Cherokee Strip Live Stock Association. Nonmembers were driven out and by 1884 the Cherokee Nation was realizing \$200,000 in annual revenues. But trouble from various sources was fast reaching a climax.

For one thing, farmers believed that they had as much right to the Outlet as cattlemen, or even the Indians. Groups of "boomers" moved in from time to time, only to be driven out by the United States Army. In 1891 the federal government also expelled the cattlemen from the Outlet and forced the Cherokees to relinquish title to the entire area for \$8,595,736.12, considerably less than the \$30 million offered by the Lucas Cattle Company of Colorado Springs. A short time later the former Indian lands were thrown open to homesteaders in what turned out to be the largest of the various Oklahoma land "runs."

According to the author, the sale of the Cherokee Outlet marked the passing of the cattlemen's last frontier. His brief, well-written and copiously illustrated history of the association documents the role of federal governmental agencies in dealing with both white ranchers and Indian entrepreneurs. The book contains little information that is not already familiar to students of Oklahoma history. But it is a straightforward story, stripped of the romantic notion that cattlemen were rugged individuals molded in the image of John Wayne. Rather, the ones dealt with in the present study relied more on cooperative effort and due process of law in preference to direct recourse or personal action—even during times of extreme provocation.

W. EUGENE HOLLON
University of Toledo

D. JEROME TWETON. The Marquis de Morès: Dakota Capitalist, French Nationalist. Fargo: North Dakota Institute for Regional Studies. 1972. Pp. x, 249. \$8.95.

The marquis de Morès was typical of the gallery of individuals who appeared larger than life on the American frontier and gave it so much of its bravado and color. Whether the West bred individualism or merely attracted it has long been the subject of debate among historians, but few would question that the relationship exists.

A French nobleman who married the daughter of a prominent New York banker, the marquis arrived in the United States in 1882 and immediately speculated heavily in the Western cattle industry. Determined to become "the richest financier in the world" he quickly acquired a ranch in the Dakota Badlands-one of his neighbors was Theodore Roosevelt-began a small town that he named after his wife (Medora), and erected a slaughterhouse with a view to shipping dressed beef to Eastern markets in refrigerator cars. When this scheme failed he plunged into one venture after another, all of which came to grief. Five years later, his American business interests a shambles, he returned to France and undertook an ill-fated effort at railway building in French Indo-China. Turning to politics he soon became one of France's most outspoken nationalists and anti-Semites. After spearheading a campaign to discredit Georges Clemenceau, whom he believed was acting as a British agent, the marquis embarked on a crusade against the British in Africa, where he was ambushed and killed while leading a one-man expedition in the eastern Sahara.

In seeking "to sort fact from fiction and to

present a well-balanced study of the Marquis' American and French careers" (p. ix) the author has set for himself an ambitious task. Because the marquis left few private records D. Jerome Tweton was forced to rely primarily on newspaper reports, admittedly "not a happy situation for the biographer to face" (p. vii). Nevertheless, by judiciously sifting through the available accounts, he has recaptured much of the flavor and excitement of this flamboyant Frenchman and his times and provided the first comprehensive account of the marquis's public career. He is less successful in illuminating in any significant way many of the larger themes of either frontier or French historiography.

Tweton has a ready eye for the apt phrase and telling detail, and his writing is generally straightforward and well paced. Sex useful maps and sixteen pages of well-chosen photographs enhance the volume. The footnotes, unfortunately, are inconveniently tucked away at the end of the book.

JAMES E. HENDRICKSON University of Victoria

SALVATORE PRISCO III. John Berrett, Progressive Era Diplomat: A Study of a Commercial Expansionist, 1887–1920. University: University of Alabama Press. 1973. Pp. xi, 149. \$5.75.

In this study Salvatore Prisco examines John Barrett as a "concrete universal" representing "the new generation of American diplomats" in the Progressive Era (p. x). After a disconnected sketch of his subject's background and the milieu of economic expansionism in the 1890s, the author presents the highlights of Barrett's career as minister to Siam (appointed at age twenty-seven); advocate of commercial development in China; minister in quick succession to Argentina, Panama, and Colombia; and finally his lengthy service from 1907 to 1920 as director general of what became the Pan-American Union. Barrett makes a good subject for a study of economic expansion, for his efforts as both diplomat and publicist all revolved around seeking to expand American markets in Asia and Latin America. He was the perpetual booster of American industry with a firm belief in Yankee efficiency and superiority. A consistent opponent of territorial annexation and armed intervention, Barrett sought to extend United States influence in the developing areas of the world through trade and the sharing of educational and scientific informa-

Although it contains some fascinating details, Prisco's thin volume is ultimately disappointing. The author frequently credits Barrett with advancing an innovative proposal, yet he seldom places it in context nor shows what impact it had. The examination of economic expansionism does not proceed beyond Barrett sufficiently to show whether he was representative of Progressive-era diplomatists or not, and Prisco relies heavily on two unpublished studies of Barrett, a biography by his niece and a dissertation by George B. Lane on Barrett's years as head of the Pan-American Union. The book is written poorly, and the author attributes to terms such as "progressive business interests" and "commercial publicist" a self-evident meaning and precision that they fail to convey.

While John Barrett was not a major figure in American diplomacy, he did develop significant ideas that were incorporated in the Open Door policy and in multilateralism toward Latin America. He deserves a more wideranging and incisive analysis than Prisco has provided.

SAMUEL F. WELLS, JR. University of North Carolina, Chapel Hill

JOHN S. GOFF. George W. P. Hunt and His Arizona. Pasadena, Calif.: Socio Technical Publications. 1973. Pp. 286. \$10.00.

Historians of the Southwest will welcome a biography of George W. P. Hunt, Arizona's first governor under statehood, although this work will fall short of their expectations. John S. Goff, a member of the faculty of Phoenix College, is well known among Western scholars. In addition to several useful articles about the territorial justices of Arizona, he is the author of Arizona Civilization (1968) and the coauthor of Arizona, Past and Present (1970). A biography of the son of Abraham Lincoln, Robert Todd Lincoln: A Man in His Own Right (1969), rounds out the writer's interest.

George W. P. Hunt participated in the political affairs of Arizona from 1892, when he entered the territorial legislature, until 1934, the year of his death. He was elected the first governor under statehood, in 1912, and won reelection six additional times. Aside from a brief stint as ambassador to Siam in 1920–21, Hunt confined his remarkable energy to the promotion of progressive democratic reforms in Arizona. (The author characterizes him as "often more Populist than Progressive.") Hunt upheld the rights of labor, detested capital punishment,

and deplored war. Yet, he shared the narrower views of the Progressives—segregated schools and the Americanization of minorities—and thus appealed to a wide category of Arizonans, the so-called "Texas Democrats." He delayed the construction of the great Colorado River Project in order to make the Southwesterners "aware of the water problem." In his last term, 1931–32, the governor experienced the frustrations of the depression, which exceeded the limited powers of the state executive to contain it.

The subject of George W. P. Hunt is difficult for the biographer. As governor of Arizona, he guided the state through the turbulent era of transition from frontier settlements to sophisticated communities. The author employs the technique of political biography and, in so doing, he confines his sources to the Hunt Papers and a few newspapers. A very narrow work results. The writer neglects the "times" and leaves unexplained the puzzling fact that, while the Arizonans elected Hunt to the governorship on seven occasions, they seldom provided him with a loyal following in the legislature. Much of the material of this biography is new and of considerable interest, but it is often undigested and the reader must struggle through inordinately long paragraphs about recurring political campaigns. Photographs and a section of biographical notes contribute to the book; but, aside from its attraction to students of Arizona history, historians must still await a full-bodied biography of this controversial statesman.

LARRY D. BALL
Arkansas State University

JUNE SOCHEN. Movers and Shakers: American Women Thinkers and Activists, 1900-1970. [New York:] Quadrangle/The New York Times Book Company. 1973. Pp. xi, 320. \$8.95.

In her preface June Sochen defines her movers and shakers as "selected women writers and feminist intellectuals who groped and struggled with the narrow definition of woman's role imposed by society" in twentieth-century America. She has dealt with this theme before, in The New Woman: Feminism in Greenwich Village, Nineteen-Ten to Nineteen-Twenty (1972), a rather thin book derived from her Northwestern University dissertation. Movers and Shakers is a more ambitious undertaking covering four periods: the Progressive era, the interwar decades, the post-World War II years, and the 1960s.

The scholar will expect a synthesis of feminist thought and action within and across these boundaries of time, but the author seems to have been more interested in contemporary feminism than in intellectual history. The book is oddly reminiscent of works published during the forty-year ebb tide of feminism by dedicated and industrious amateurs. At every point it lacks depth and cohesion. The selected feminists and their ideas are introduced in a series of disconnected summaries, with only elementary efforts to trace roots or interrelationships. We find no account, for instance, of the connections, intellectual and personal, between Village radicals and academic feminists at Columbia like Leta Hollingworth and Elsie Clews Parsons. As presented here, Zona Gale, Fannie Hurst, and Freda Kirchwey seem an odd combination; actually these and other successful young women writers and journalists in the New York of the early twenties banded together to continue the woman's rights crusade by forming the Lucy Stone League to encourage married women to retain their maiden names. (One ex-Lucy Stoner, Doris Fleischman Bernays, could have exemplified the transition to the hearth-and-home fifties, when her book A Wife Is Many Women was a best seller.) We are distracted by pages devoted to Freda Kirchwey's editorial stands in The Nation and to other nonfeminist activities of some writers and intellectuals, and confused to encounter women who were neither (Lady Bird Johnson, sandwiched between Betty Friedan and Masters's and Johnson's Human Sexual Response).

Historians will recoil from many of the author's generalizations, large and small. On the opposition: "The preservers of the status quo, in 1900 as in 1970, of course saw every secretary, every clubwoman, and every professional woman as a vital destroyer of the home and family" (p. 13). On the woman's club movement of 1900, already strongly civicminded: "Instead of making their families' clothes, women attended lectures on the latest piece of romantic fiction" (p. 5). On the 1920s: "The flapper image dominated; its force dissipated all others" (p. 104). And on one special heroine: "The only woman in this whole century who emerged as a popular example of a fully realized human being was Eleanor Roosevelt" (p. 151).

One wishes for better justice to the vital and venturesome women who lie inert on these pages—and perhaps for a nod in passing to Mabel Dodge Luhan for the book's title. Movers and Shakers may have some value for

consciousness raising. The historian seeking an understanding of women in twentieth-century society will be better served by the recent books of William Chafe, Stanley Lemons, and Anne Scott.

JANET WILSON JAMES Boston College

LAURENCE VEYSEY. The Communal Experience: Anarchist and Mystical Counter-Cultures in America. New York: Harper and Row. 1973. Pp. xi, 495. \$15.00.

Mr. Veysey has given us a superior book. It is an exercise in the way a historian can "make" a history without violation of objective standards. The author wanted to come to an appraisal of radical communitarianism. His quest required looking into a greater variety of radical ventures than could fall within one period or conscious tradition. So he selected, out of different moments and impulses in twentieth-century America, some radical communities sufficiently similar, and sufficiently unlike, that he could make them speak elaborately to one another. Although the radical tradition he has uncovered is his composite, it is real in the genuine argument that can be set up among his communities. Veysey hit upon an intellectually engaging distinction between the anarchist community, in which the person is expected freely to develop his capacities, and the mystical community, wherein the member under the domination of a guru (here the author should exclude Christian mysticism)subdues the inferior self and seeks to be remade and taken into the cosmic order. Veysey achieves an extraordinarily close and versatile examination of his subjects. His intellectual perceptions are excellent: his contrast, for instance, between the older anarchist view of the free self as centered in the controlling will and the newer radical concept of the self as a complex of emotions and urges.

It appears a temptation of radical experimentalism to reduce the cosmos and the moral problem to a clean formula pointing to a utopian or a perfectionist program. Veysey himself senses the variousness and the stubborn mysteriousness of existence, and the limits and contradictions that attend the human project; but some of his admirable communitarians display little feeling for these things—though he includes such groups as the Rockridge community, which seems content to live modestly with the goods the earth awards to hard work. The book might have balanced its story with

some account of other radical alternatives that are at home with the world's intractabilities. Two examples come to mind. The rich anarchist imagination of Paul Goodman envisioned a free communal life all full of rough edges: the confusions, gropings, and painfully acquired self-disciplines whereby individual human beings complete themselves. And Christianity, whether pietistic or monastic, has sought to break and renew a human nature that may not in this time and world shed its capacity for evil, but must work its slow way toward mystery through a contingent universe.

THOMAS R. WEST
Catholic University of America

EUGENE LEVY. James Weldon Johnson: Black Leader, Black Voice. (Negro American Biographies and Autobiographies.) Chicago: University of Chicago Press. 1973. Pp. xiii, 380. \$14.50.

Beginning in 1970 several biographies of history-making Afro-Americans of the late nineteenth and twentieth centuries have appeared in print. Most noteworthy among these is Louis R. Harlan's first volume in his projected two-volume biography of Booker T. Washington, but other significant books are Stephen R. Fox's Guardian of Boston: William Monroe Trotter; Emma Lou Thornbrough's T. Thomas Fortune: Militant Journalist; Jervis Anderson's A. Philip Randolph; David M. Tucker's Lieutenant Lee of Beale Street; and David Lewis's King: A Critical Biography. Further enlarging our knowledge of the Afro-American past are the recently published memoirs of John Roy Lynch and Ida B. Wells. A most worthy addition to this growing list of biographies and autobiographies is Eugene Levy's book.

As John Hope Franklin has written in his foreword to Levy's book, James Weldon Johnson "would have made his mark if he had done no more than edit the New York Age, or written the Shoo-Fly Regiment, 'Lift Every Voice and Sing,' and The Autobiography of an Ex-Colored Man, or become the first black executive secretary of the National Association for the Advancement of Colored People." But these were only some among the many accomplishments of a person who, Franklin has added, was "one of the very unusual men of this century." Johnson was also a high school principal in his hometown of Jacksonville, Florida; a lawyer; a diplomat as U.S. counsel in Venezuela and Nicaragua; the author not only of a novel and "The Negro National Anthem"

but also of lyrics for "coon songs and ragtime," newspaper editorials, poems, and an autobiography; a progenitor of and active participant in the Harlem Renaissance; and a professor at Fisk University. In terms of the diversity and high level of his artistic, intellectual, professional, and political attainments, Johnson had been surpassed among twentiethcentury Afro-Americans only by W. E. B. Du Bois, with whom he shared an intense commitment to both first-class citizenship for the nation's blacks and the need to expand white awareness of and black pride in "the race's past and present accomplishments." But at one crucial point Johnson and Du Bois diverged; Johnson, unlike Du Bois, did not "reject the desirability of ultimate assimilation, but [he believed] if he could convince both black and white Americans of the race's fundamental cultural contributions to American society, then the process of blending would become far more palatable."

To deal with such a multifaceted person requires competency in a variety of fields, and this competency Professor Levy displays in admirable ways, especially in his handling of literature and music. Moreover, Levy has had to do a prodigious amount of research, and this is reflected in his extensive bibliography. There is, however, one discordant note in this positive review: James Weldon Johnson, the person with feelings and probably self-doubts, seldom emerges from these pages. It seems from Levy's book, for example, that Johnson easily and naturally piled achievement upon achievement, but one still wonders about the sources of his talent and motivation. It is insufficient simply to state that Johnson "dreamed of making a 'name' for himself," or to quote from a friend who wrote: "Jim, I think you are more ambitious for honor and fame than for anything else." Johnson was a man of emotional detachment, and perhaps the sources for definitive statements about his motivation are lacking, but Levy could have and, I think, should have speculated about what drove this remarkable person to such heights of accomplishment.

WILLIAM M. TUTTLE, JR. University of Kansas

THOMAS D. CLARK. Indiana University: Midwestern Pioneer. Volume 2, In Mid-Passage. Bloomington: Indiana University Press. 1973. Pp. xviii, 429. \$17.50.

The objective scholarship, lively style, and probing analysis of educational and institu-

tional problems in a state and regional context that marked Thomas D. Clark's first volume of Indiana University: Midwestern Pioneer are sustained in the second volume. This tells the story of the transformation of the small college of 1902 into the broad-based university of 1937. In these thirty-five years President William Lowe Bryan dominated the institution. Professor Clark skillfully analyzes and balances the contributions and limitations of this complex and paradoxical man: a rural patrician, a puritan of conservative religious and moral values, in many ways an intellectual liberal, and always a strong personality. Full account is taken of the impact of revolutionary changes in American life on the operational management of the institution and on the relationships of administration, trustees, politicians, faculty, and students. Despite the continuing battle for financial support, for acceptance of the university's role in the competition with other state institutions, despite the problems associated with the First World War, the Ku Klux Klan, and the Depression, President Bryan established the base of professional education, including medicine, engineering, and music. He also played a major role in the national recognition of Indiana's status as a university. Unlike Wisconsin, Indiana took full advantage of the availability of federal funds in the New Deal period for physical expansion of the plant, to which Clark devotes detailed attention. The role of athletics, perhaps appropriately, receives as much space as the account of the scholarly achievements of the faculty.

Those interested in explanations of how Indiana has subsequently come to be the distinguished university that it is will be rewarded by Clark's account of the bold and even brutal self-study and assessment of 1937–38 that provided Indiana with a blueprint for a very different kind of institution. The account of this turning point from an admitted mediocrity to distinction under the leadership of Herman Wells, who became president just as this volume ends, whets the appetite for what we hope will be a third volume of Indiana University's history.

MERLE CURTI
University of Wisconsin,
Madison

JOHN D. BUENKER. Urban Liberalism and Progressive Reform. New York: Charles Scribner's Sons. 1973. Pp. xi, 299. \$8.95.

For those familiar with Professor Buenker's numerous journal articles concerning urban liberalism and progressive reform, this book will serve as a handy compilation. It does not venture far beyond these articles, but solidly documents J. Joseph Huthmacher's thesis of a dozen years ago that the urbar lower class contributed to progressive reform.

In constructing his argument, Buenker elaborates upon several subthemes. He adheres to the coalition theory of progressivism, noting cautiously and correctly that while the urban ethnic working class supported many progressive reforms, it did not have sole responsibility for their enactment and implementation; progressive reform resulted from the interest and activity of several groups, which sometimes, but not always, worked together. Another theme, the importance of ethno-cultural issues such as prohibition and sabbatarian laws, emerges when Buenker investigates why certain groups in the coalition were often at cross purposes. Here, while considering other variables, he puts the ritualist-pietist religious dichotomy to good use. Rather than viewing political bosses and machines in the context of morality and emphasizing corruption, the author's third theme focuses on the functions of the machines, such as providing welfare services for their constituents. The book provides a clear understanding of how and why these machines supported many progressive reforms, both structural and social, once believed antithetical to the nature of machine rule.

The support of reform generally by the urban liberals, especially welfare reform, dovetails into another theme of the book, the continuity between the Progressive Era and the New Deal; the roots of the national welfare state (if such a thing actually exists) were firmly planted before the depression decade. In the final theme, if there is retrospective continuity, the author also finds continuity for our own time. He suggests that the tradition of urban liberalism should have meaning for both today's urban ethnics and for middle-class liberals.

The book, which is largely a synthetic work, focuses on reform legislation passed ir. the industrial states of Massachusetts, Connecticut, Rhode Island, New York, New Jersey, Pennsylvania, Illinois, and Ohio. Sometimes chapters tend to become repetitive as the fate of individual reforms is played over and over again state by state. Since state legislatures are the focus, roll call analysis of voting might have permitted the author to make more precise statements regarding the legislative behavior of urban representatives and senators. Electoral

voting return analysis, such as that cited by the author and undertaken for California progressivism by Michael Rogin and John Shover, might have been attempted to highlight urban ethnic working-class political behavior in other parts of the nation.

Nevertheless, by placing into historical perspective the relationship of the urban ethnic working class to liberal reform, John Buenker implies that Archie Bunker might not have been such a bad fellow after all. As a result, his is a most useful and suggestive study providing insight into the meaning of liberalism in American society and raising many questions still to be answered.

BRUCE M. STAVE
University of Connecticut

LAWRENCE R. GUSTIN. Billy Durant: Creator of General Motors. Grand Rapids, Mich.: William B. Eerdmans Publishing Company. 1973. Pp. 285. \$8.95.

One of the significant gaps in the literature of automotive and business history is the lack of a definitive full-length biography of William C. Durant—the founder of General Motors and Chevrolet, the "leading bull" in the great stock market boom and crash of the late 1920s, and probably the most flamboyant entrepreneur in modern business history. Durant's contributions to the development of our most important industry rank with those of Henry Ford and Alfred P. Sloan, Jr.; and his legendary market manipulations make him a central figure in the history of Wall Street. Yet Durant's fame progressively waned after his brokers sold him out and his last automotive venture, Durant Motors, collapsed at the onset of the Great Depression. By the time of his death in 1947 the once indomitable creator of business empires was an obscure personality outside the automobile industry and his home town of Flint, Michigan. And his historical reputation remains clouded by conjecture and myth largely because researchers continue to be stymied by the sparseness and inaccessibility of essential primary

Gustin—the automotive editor of the Flint Journal—attempts to resolve this problem by going beyond a decent synthesis of the standard sources and a scrupulous search of the public record to base his account of Durant "in part on unpublished manuscripts and documents and on interviews with his widow, two of his personal secretaries and others who knew him well." Unfortunately, this turns out to mean

heavy reliance on a 600-page industrial history of Flint compiled in the early 1940s by Frank M. Rodolf, then a reporter and librarian for the Flint Journal, and on Durant's fabled memoirs, which, according to Gustin, consist of "seven short type-written chapters, plus scattered notes . . . not always accurate-much of it written in the 1930s and 1940s, decades after the events described." It seems obvious from Gustin's text and sparse footnotes that he has examined few of Durant's personal papers beyond these virtually useless memoirs and that he has done almost no research in the voluminous papers of John J. Carton, Durant's personal attorney until 1921. The interviews with those close to Durant also fail to shed much new light on the central questions about significant aspects of Durant's career—although Gustin does correct a number of minor factual errors. As one might expect from Gustin's sources, Durant is far too uncritically interpreted from the myopic perspective of Flint local history.

While Gustin's fact-laden book undoubtedly will be welcomed by Durant devotees and anyone interested in the history of Michigan, it falls short of satisfying the need of automotive and business historians for a definitive Durant biography. Whether this need will ever be satisfied depends on scholars obtaining access to Durant's personal papers and the General Motors archives. Neither seems likely to occur in the near future.

JAMES J. FLINK
University of California,
Irvine

JOHN L. NETHERS. Simeon D. Fess: Educator & Politician. Brooklyn, N.Y.: Pageant-Poseidon. 1973. Pp. xvii, 427. \$8.95.

Describing Simeon D. Fess as one of the "lesser great" of history, Professor John L. Nethers promises his readers "something more than a mere historiographical-biographical exercise." Seeking to justify his study of a "relatively unknown historic figure," he pledges to unveil the economic, political, and social forces of the Fess generation by discovering the relationship of the man to his generation.

There is no need to justify even a mere biography of a man who spent ten years as president of Antioch College, ten years in the lower house of Congress and twelve years in the upper house. Fess might not be a household word in the 1970s, but it was in the 1920s, for he was a man of considerable power and influence.

There is a need for a thorough analysis of a man who had the ear of three presidents and sat high in the councils of the Republican party, but Nethers has not provided it.

The major weakness of the book is inadequate research. Hamstrung by a dearth of personal papers, the author utilizes with some skill those remaining as well as the *Congres*sional Record and numerous Ohio newspapers. Other primary sources, especially the manuscript collections noted in the bibliography, are only used sparingly. Nethers relies extensively on the suspect reminiscences of two Fess children, a family retainer, and two former secretaries, all quite elderly and of questionable objectivity and authority. A one-day interview with the eldest son is cited over fifty times.

In addition, for someone who promises to relate Fess to the great forces of his generation, Nethers expresses little appreciation for the secondary sources that could help illuminate those forces. His citations are devoid of the numerous studies of individuals, events, and issues of the Fess era published in the last decade, for example, Robert Murray's Harding Era: Warren G. Harding and His Administration (1969), Albert Romasco's Poverty of Abundance: Hoover, the Nation, the Depression (1965), or Roger Daniels's The Bonus March: An Episode of the Great Depression (1971). In their place are references to dated sources and to survey texts and readers. The study was originally a dissertation completed in 1964 and almost nothing has been done in the intervening years to improve and to refine it.

Because of the inadequate research, the reader is not even offered a biography of substance. The personal life of Fess beyond his childhood is scantily discussed in spite of the author's apparently close relationship with the family. Little or nothing is said about his role in Ohio politics; nor is there any elaboration on his rise to positions of power in the Senate and the Republican party. His relationship to other significant political figures is either handled superficially or ignored. Rarely does Nethers get inside the man and plumb the depths of motivation. When he does, as in his examination of how Fess reacted to the New Deal, the subject comes alive, but generally the reader is left with a flat, one-dimensional portrait of a complex individual.

The book sorely needed editorial assistance. Poor organization led to needless repetition, sometimes within the same page. The prose is pedestrian, often unnecessarily wordy, and sentences tend to be strung out. Within chapters,

the author jumped from topic to topic with minimal effort at transition and continuity.

Nethers is to be credited, however, for his objectivity and for his uncovering of some interesting facets of Fess's life. The revelation that J. Franklin Jameson drove Fess from the history department of the University of Chicago for failure to complete his doctorate is an interesting commentary on the professional standards of the day. But overall, the book does not live up to the author's promises and anyone interested in Fess could probably do as well by consulting Everett Walter's summary in the Dictionary of American Biography and William C. Murphy's "Preceptor Senatorium" in American Mercury (Dec., 1928).

LAWRENCE L. MURRAY State University of New York, Fredonia

RICHARD FITZGERALD. Art and Politics: Cartoonists of the Masses and Liberator. (Contributions in American Studies, number 8.) Westport, Conn.: Greenwood Press. 1973. Pp. xiv, 254. \$14.50.

This book is one of several—there will doubtless be others—that leave one vaguely uncomfortable with its major premise of radical sympathies. Joyce L. Kornbluh's Retel Voices: An I.W.W. Anthology (1964), though filled with passionate antiestablishment materials, nevertheless, in format and gay colors looked made for the Yule season booklist and heaps of merry laughter and foragings under the Christmas tree. Art and Politics is dedicated "to the bunch at Ryan's Tavern who still quite justifiably blame the boss rather than themselves," but its acknowledgments of those who helped bring this book to fruition include slaves of the Ford Foundation, and staffs of the Hoover Institution, Lilly Library, and other salt mines.

The text, too, reflects the paradox of American largess and dissent, and this fact is compounded by the problem of art. The author has researched his cartoonists adequately. Art Young, Robert Minor, John Sloan, K. R. Chamberlain, and Maurice Becker evolved styles and themes that tell us much about the radical temper of the 1910s and beyond. But the author is not aware that there are problems of assessment. His footnotes are mere façades; he brings back from them only what he brought to them. "The American Communist Party of the 1930's had a view, known as 'socialist realism,' that art either had to be a

spontaneous expression of proletarian life or had to conform with the ideas of 'dialectical materialism.' . . . The relative mediocrity of such artists as John Sloan comes from their compulsion to reflect life rather than to create new forms. Becker, Minor, and Young on the other hand are interesting because they were not caught in the esthetic trap of having to reflect life" (p. 23). And again: "Even [Sloan's] . . . disapproving 'Women's Night Court' only says that the system of justice is hypocritical; it fails to say that the basic values of society are wrong" (p. 140).

The author is not aware that there are those who think, or thought, that the basic values of society were right, and that to demonstrate their invalidity and the proved rightness of his alternative values requires greater subtlety and a sounder grasp of artistic and human principles than is here indicated. His chapter on Robert Minor is particularly interesting, since he is aware that Minor became a notorious example of one who had abandoned art to become an exact carbon copy of anything the communists at home or abroad had decided to say that day or hour. His defense of Minor is worth quotation: "Minor was better at drawing than journalism, but there is a fine line between cartooning and journalism. His writing was a different side of the same coin, was not 'banal and boring,' and showed the same stark contrasts as his illustrations" (p. 110).

The record will show that knowledgeable individuals in the land believe that Sloan was a great artist, that the fine line is not between cartooning and journalism, but between treadmill drawing and writing and principles on one side, and art and humanity on the other, and that Minor's contributions as a political hack were not only banal and boring, but, on numerous occasions, and with due credit to Stalin's contributions to socialist realism, horrifying. Our twentieth-century decades have not been tranquil, and their art shows it. We will need more confrontations in the cultural spheres than we presently have, before we can hope for a more adequate sense of its failures and successes. Meanwhile, some sixty cartoons here, well reproduced, provide us with artifacts for substantive debate.

LOUIS FILLER
Antioch College

JERVIS ANDERSON. A. Philip Randolph: A Biographical Portrait. New York: Harcourt Brace Jovanovich. 1973. Pp. xiv, 398. \$12.50.

This sympathetic biography of A. Philip Randolph is a useful first account of the "presence emeritus" among today's black leaders. The material first appeared as a profile in the New Yorker. As a book it shows the strengths and weaknesses of its origins. Though there are no footnotes, Anderson makes it clear that he drew on published material, on manuscript sources, and on extensive interviews he conducted with Randolph and his close associates. The result is a clearly presented, sometimes superficial account of Randolph's career.

As biography it is strongest in the earlier chapters. Here we see Randolph growing up in turn-of-the-century Jacksonville, Florida, his move to New York and Harlem in 1911, and his early career as editor of the socialist magazine, The Messenger. Anderson does an effective job of setting each scene, and he is especially adept at creating evocative vignettes of such Randolph colleagues as Chandler Owen, W. A. Domingo, and George Schuyler. The author is much less successful in revealing his hero's basic psychological make-up. The reader is unsure, for example, of Randolph's self-image, which must have been positive to carry him through repeated failures.

Anderson's account flattens out after Randolph abjures socialism and begins in the mid-1920s to organize the sleeping-car porters into a viable union. As in the earlier chapters there are some crisp characterizations of Randolph and his co-workers as they struggled, unsuccessfully until 1937, to garner recognition from the Pullman Company. The story of how Randolph finally achieved recognition for his union is essentially a page in New Deal labor history. The centrality of the New Deal in Randolph's career goes a long way toward explaining his persistent efforts in the 1960s to make blacks an effective element in the coalition between the AFL-CIO and the liberal wing of the Democratic party.

Anderson's discussions of the first march on Washington movement, of Randolph's role in the AFL-CIO as a "critical but loyal ally," and of his influence in the civil rights movements of the 1950s and 1960s are neither descriptive enough to recapture the excitement of the era nor analytic enough to deepen our understanding of the man or the movements. The author indicates in his preface that he subtitled his book A Biographical Portrait to emphasize his modest goal of presenting selective aspects of Randolph's career. The next

biographer of A. Philip Randolph will have to risk more in order to achieve more.

EUGENE LEVY
Carnegie-Mellon University

ROBERT G. WEISBORD. Ebony Kinship: Africa, Africans, and the Afro-American. Foreword by FLOYD B. MCKISSICK. (Contributions in Afro-American and African Studies, number 14.) Westport, Conn.: Greenwood Press. 1973. Pp. xi, 256. \$10.95.

Although black nationalism in its emigrationist form is the theme of this book, Professor Weisbord never undertakes an analysis of the concept of nationalism and exhibits a willingness to accept uncritically the plethora of black nationalist slogans that have emerged in the twentieth century. In an attempt to demonstrate continuity of "back to Africa" schemes, the author does considerable violence to historical reality as he loosely associates the activities of such figures as Martin R. Delany, Edward M. Blyden, Benjamin Singleton, and Marcus Garvey to create the impression of a "movement" with similar historical origins and political purposes. The gross distortions resulting from this approach are magnified further by Weisbord's insistence on discussing these disparate efforts in a historical vacuum without significant reference to social and political changes in the United States. The failure to place black nationalist ideas and their organizational manifestations in the context of United States history is not compensated by any corresponding depth of analysis of a narrowly defined "black history." There is little evidence of research in previously unpublished sources, and it is not unusual for the interpretations of anecdotal evidence to be marred by astonishing glosses like "Harlem is unquestionably the heart of Black America . . ." (p. 204). The sporadic "back to Africa" enthusiasms of Harlem during periods of economic stress are therefore presented as being more representative of the black population than they were. Weisbord declines to offer any explanation as to why Garveyism failed to attract significant support in the South, or why there was a disproportionately high participation of West Indians in the movement. Even though the greater portion of the book is concerned with Marcus Garvey, the author has not produced a new or clearer understanding of Garvey or the impact of his organization, the Universal Negro Improvement Association. Equally disappointing is the treatment of the

historical origins of related movements. The links between black American, Caribbean, and African developments remain virtually unexamined. Weisbord writes, for example, in his rambling discussion of the impact of Ethiopia on New World blacks, that to "understand the origins of Rastafarianism one must recall that it was to Jamaica that Garvey had gone after his deportation from the United States in December 1927. He remained there until 1935 and it was during that period that Rastafarianism was born. The actual connection between Garvey and the Rastas is nebulous" (p. 123). Perhaps the least satisfactory part of the book is the breezy discussion of the impact of African political independence and "black power" slogans on black Americans in the 1960s. Punctuating his loose narrative by condescending familiarity with his protagonists such as "Stokely" and "Eldridge," Weisbord also trivializes vast changes in cultural and intellectual outlook by concentration on clothing fashions among some black Americans, characteristically attempting to support his argument by reporting that in 1971 "forty-nine military barbers and beauticians underwent a hairstyling course under the direction of a well-known black hair stylist" (p. 194).

Measured by contemporary standards of historical scholarship, this book does not represent a contribution to a subject worthy of professional research and analysis.

MICHAEL R. WINSTON-Howard University

WILLIAM H. HARBAUGH. Lawyer's Lawyer: The Life of John W. Davis. New York Oxford University Press. 1973. Pp. xvi, 648. \$15.00.

This book is a superb biography that may be read to advantage by any student of twentiethcentury United States history. John William Davis, who served as a West Virginia legislator, two-term congressman, solicitor general, ambassador to Great Britain, 1924 Democratic candidate for president, and chief spokesman for the anti-FDR Liberty League, hardly qualified as a faceless corporation lawyer. Yet on December 7, 1953, when he stepped to the bar to argue his 140th case before the United States Supreme Court, he had all but faded from popular memory. Within the legal profession, however, he had become a living legend, and not the least of his achievements was to have orally argued more cases before the Supreme Court than any other twentieth-century attorney. In this, his final appearance, he argued

for South Carolina that segregated education did not violate the Fourteenth Amendment. As evidence of his substantial commitment to that cause, he appeared without fee. Opposing counsel Thurgood Marshall, who hardly shared Davis's social and constitutional views, later described him as "a great advocate, the greatest." In a similar vein, and long after Davis had identified himself with persistent opposition to federal regulation and a tolerance for concentration of private wealth and power even beyond the consciences of some corporate clients, Felix Frankfurter wrote him that "as law teacher and judge I have often referred to you as one of the finest exemplars of . . . the public profession of law."

Professor Harbaugh first confronted this puzzle in 1958, when John Davis's daughter made her father's papers available to him and a grant from the Davis estate enabled him to organize them and conduct interviews. In spite of these initial connections with Davis heirs, this was never intended as an official biography. In the course of his research Harbaugh examined over thirty-five manuscripts and oral history collections and spent 1960-61 as a Senior Fellow at Yale Law School where he audited courses and read law. He concluded that Davis was the greatest solicitor general ever, but is generally critical of the extent of his subsequent commitment to conservative monied interests. As Davis insisted, however, even J. P. Morgan was entitled to counsel.

SAMUEL B. HAND
University of Vermont

ABRAHAM HOFFMAN. Unwanted Mexican Americans in the Great Depression: Repatriation Pressures, 1929–1939. Tucson: University of Arizona Press. 1974. Pp. xv, 207. Cloth \$9.75, paper \$4.75.

This valuable monograph—developed from a 1970 dissertation—is not only an important contribution to Mexican-American history, but also illuminates the history of deportation and U.S.-Mexican relations. The subject is the more than 450,000 persons of Mexican ancestry who returned to Mexico from the United States between 1929 and 1937, some 13,000 of them in special trains chartered by Los Angeles County. These returning immigrants were "pulled" by their cultural ties to Mexico and by promises, unfortunately not adequately fulfilled, of land by a Mexican government that had an ideological commitment to repatriating all members of la raza, and "pushed" by eco-

nomic depression, discrimination, and the anti-Mexican activities of federal and local government. Paul S. Taylor and Carey McWilliams wrote about this decades ago, but Hoffman has revised and filled out the story admirably with research in both local and national archives. Although his emphasis is on what happened in southern California, he shows that repatriation was a national phenomenon.

The repatriation movement involved several processes: federal deportation, formal and voluntary, voluntary return, federal government repatriation of destitute aliens, organized repatriation promoted by American welfare agencies to pare welfare rolls, by Mexican consuls and the local ethnic community, and, finally, coercive, forced or involuntary repatriation by which Mexicans were hustled out of the country. Among the nearly half million people involved were thousands of children who were citizens of the United States by birth. Repatriation often deprived these young Americans of an opportunity to claim that birthright. Many of the adult repatriates imagined that a subsequent return to the United States would be simple, but immigration officials had begun, in 1928, a stricter enforcement of the statutes so that it became increasingly difficult for poor Mexicans to enter the United States despite the absence of any restrictive quota.

Hoffman is acutely aware of the human reality behind the statistics. If once or twice the reader becomes bogged down in detail, it is almost a relief from the breezy impressionism that has characterized too much recent ethnic history. The documentation is quite thorough so that the failure, at one point (pp. 156–57), to consult the papers of California Governor Culbert Olson stands out all the more. These minor flaws do not materially detract from a solid work of scholarship.

ROGER DANIELS
State University of New York,
College at Fredonia

MARTIN DUBERMAN. Black Mountain: An Exploration in Community. New York: E. P. Dutton and Company. 1972. Pp. 527. \$12.95.

Black Mountain College was founded in 1933 in the mountains of North Carolina by John Andrew Rice, and the school managed to survive in spite of innumerable problems until it closed its doors in 1956. Rice and the original Black Mountain group came from Rollins College where Rice had irritated the administration

with numerous acts of nonconformity until the president, Hamilton Holt, forced him to leave. Perhaps no one, except those who were actually involved with it, ever had a clear picture of the Black Mountain movement. According to the legend it was a noble experiment in democratic communal education, free of all administrative tyranny traditionally associated with many American colleges. Martin Duberman's detailed history of the movement destroys the myth of an idealistic campus and describes instead an unstable college community torn by personal vendettas and passionate debates over academic policies.

Since Rice had a deep-seated prejudice against "constitutions, bylaws and all other creative restrictions on the freedom of creative effort" (p. 34), it was to be expected that the administration of the college would be as unstructured and as democratic as possible. However, the organization of the college was so loose that it became impossible to make decisions with the result that the institution seemed doomed to failure. At this point Rice, as rector, became a traitor to his own philosophy by seizing administrative power and in turn he was denounced by the other members of the community as an arbitrary leader. His questionable leadership and his affair with a student named Alice (whom he later described as a slut) were major factors in creating the chaos that developed at Black Mountain in 1938. Rice was relieved of his post but even under new leadership the college continued on a precarious course toward ultimate disenchantment.

It should be pointed out that inadequate leadership was only one weakness in the Black Mountain organization. A list of factors that contributed to the closing of the school would include the inability of the faculty to assimilate their pedagogical differences, the unbalanced curriculum, faculty purges, financial problems, student revolt, failure to gain accreditation, and the lack of real scholarship and intellectual stimulation that was supposed to come from the close association of teacher and student.

In spite of its short and insecure existence Black Mountain made some interesting contributions. If it did not invent the idea of independent study, it certainly gave a boost to this academic technique. The same is true with respect to the interdisciplinary seminar. The best teachers were concerned with art or music and all forms of art were taught as an approach to learning, not as a technique. In other words all students were encouraged to be imaginative and productive and to learn through ex-

perience. The college gave several artists and musicians an opportunity to develop their careers further. Among these were Josef Albers, the abstract painter who had escaped from Nazi Germany, the composer John Cage, and Buckminster Fuller, who perfected his now-famous geodesic dome while on the staff at Black Mountain. One of the most successful students who studied art at Black Mountair was Ruth Asawa, now a well-known sculptor and conductor of art workships in some of the California public schools.

Unlike some of Duberman's earlier prizewinning publications this book exhibits an uneven quality. The first chapters are well written and approach the high standards one expects from this author. But this excellence is not maintained and the latter part of the book, while showing cleverness and imagination, cannot be considered good history if the traditional criteria are to be employed. Throwing the cardinal principles of historical writing to the winds, he lets himself become personally involved with his subject in an attempt to write a new kind of history. He might have brought it off had he shown more wisdom in the selection and organization of his material. However, the book will probably remain the standard history of the Black Mountain movement for many years.

F. GARVIN DATENPORT, SR. Monmouth College, Monmouth, Illinois

RICHARD DYER MACCANN. The People's Films: A Political History of U.S. Government Motion Pictures. (Studies in Public Communication. Communications Arts Books.) New York: Hastings House. 1973. Pp. xviii, 238. Cloth \$11.50, paper \$6.75.

The motion picture has been well established as an integral part of the fabric of American life for over fifty years. Not until recently, however, has the motion picture become a respectable and legitimate object of study for historians. Richard Dyer MacCann became a pioneer of sorts in this field with his 1950 dissertation on government films, which in subsequent years slowly gained the status of an "underground classic" among those interested in the scholarly, historical study of film. The People's Films is a revised and updated version of this dissertation brought out in response to the increased interest in all things cinematic.

Despite the subtitle, Professor MacCann has

not attempted to write a full political history of U.S. government motion pictures. The book touches lightly on the bulk of government film production in favor of three dramatic and important documentary film ventures by the U.S. government: the productions of Pare Lorentz and the U.S. Film Service during the New Deal years, the World War II Armed Forces and Office of War Information films, and the U.S. Information Agency (U.S.I.A.) productions of 1962-67. These are presented in their political and governmental context and form the core of the book, allowing MacCann to focus upon the questions of how and why the films were made, upon the difficulties of creating cinematic statements within a bureaucratic and political framework, upon reconciling the public's need to know and the government's impulse toward propaganda, and upon the necessity of a free flow of information for a healthy democratic society.

MacCann's study is based upon published sources, personal interviews, and the films themselves, but not upon archival materials, although some of these have become available since the original research was done in the late 1940s. His commentary on the films is perceptive and informative, and he has not allowed the fact that he obtained a great deal of information from those associated with the production of the films to cloud his judgment.

In several ways, however, this is an uneven work. The second chapter is an essay on documentary films in England and Canada that stands relatively isolated from the rest of the book. Since the problem of "influence" receives minimal attention, and rightly so, it might have been better had this chapter been reduced to a few pages of background material in the introduction, rather than to divert the reader's attention for an extended period from the matter at hand. Likewise, the two short, final chapters, "Television for President and Congress" and "The Documentary Dialogue," seem tacked on to the end of the book, are speculative as much as scholarly, and detract from the impact of the book as a whole. Nevertheless, MacCann's treatments of Lorentz. the World War II films, and the U.S.I.A. productions are a real contribution to the growing body of historical film scholarship in print and will continue to be required reading for those interested in documentary films and in governmental attempts at communications and public relations.

FREDERICK D. JACKES
Philadelphia, Pennsylvania

FRANK FREIDEL. Franklin D. Roosevelt: Launching the New Deal. Boston: Little, Brown and Company. 1973. Pp. x, 574. \$15.00.

The fourth volume of Frank Freidel's ambitious biography of Franklin Delano Roosevelt differs from the previous three installments in several respects. It is the first to convey his protagonist beyond the threshold of the White House, for though it begins with a lengthy discussion of the interregnum between Roosevelt's election in November 1932 and his inauguration on March 4, 1933, the greater part of the book is devoted to the opening months of FDR's first term. It is even more densely researched than his earlier works, because Freidel has ranged from the Public Record Office in London to the Bancroft Library in California. The book is infinitely more detailed; whereas Freidel's third volume covered nearly four years in 414 pages, his new study requires 551 oversized pages for a mere eight months. It gives an unusual amount of attention to foreign affairs, from the labyrinthine, and not always enthralling, war debts controversy of the interregnum to the abortive, and more engrossing, London Economic Conference of July 1933, with which his account closes. And last, volume four is considerably more judgmental, though Freidel is better disposed toward Roosevelt than are many historians today.

Since Freidel has not written a chronicle of the birth of the New Deal but a component of a multivolume biography, he has inevitably concentrated his attention less on what now seems historically significant than on what preoccupied Roosevelt at the time. Hence, he gives an unusual amount of attention to topics like railroad legislation, which historians have traditionally neglected but that concerned the new president, while matters like the Home Owners' Loan Corporation and the Federal Emergency Relief Administration get short shrift. His conscientious exposition of the genesis of legislation robs the swiftly moving Hundred Days of an element of dynamism, and some readers may wish that he had addressed himself more directly to historiographical controversies like the watershed debate. Yet Freidel's meticulous reconstruction of FDR's world in these months gives us the most trustworthy account we have yet had of this subject, and, implicitly when not explicitly, he addresses, too, the weightier questions of interpretation of the New Deal.

New Left critics will find ample evidence in support of their conviction that the New Deal had decidedly conservative characteristics. Freidel underlines the continuities (as well as the discontinuities) between Roosevelt's policies and those of Herbert Hoover; he explores, in particular, the manner in which the emergency banking legislation of March 1933 derived from the labors of Hoover's financial advisers. He recounts, too, Roosevelt's hostility toward massive public works expenditures and his inclination toward regressive taxation. If these points are largely familiar, Freidel often places them in an instructive frame, especially when he observes how conservative decisions resulted from the persistence of the progressive mode. Thus he emphasizes that Roosevelt's conservatism on banking originated in part from a desire to punish Wall Street and to curb the recklessness of financiers, that his fiscal stance owed much to repugnance toward the profligacy of Republican administrations in the 1920s in aiding business, and that his resistance to public works spending may be traced in part to solicitude for the national patrimony.

Although Freidel takes pains to delineate the conservative aspects of the Hundred Days, he leaves no doubt that he regards FDR as a changemaker. Indeed, he portrays Roosevelt as "a believer in large-scale, long-range planning, both in foreign policy and domestic affairs." By April, Freidel notes, a British critic was complaining that Roosevelt "has driven a coach and four through the sanctity of contract, and appears to be anxious to destroy the vital spring of the capitalist system—the free market." That summer Roosevelt commented, "Orthodoxy may not be the only method for nations any more than individuals, to get to Heaven."

Freidel concludes that Roosevelt and the Congress of the Hundred Days "permanently and significantly modified" the economic system, "introducing the element of government intervention in the economy on behalf of the general welfare to a degree never before known in peacetime." He adds: "The great debate which was to wrack America through Roosevelt's remaining years in the White House and for almost a decade thereafter would not focus upon whether he had gone far enough but upon whether he had gone too far. What pained businessmen was what Fortune pointed out to them before the end of 1933, that Roosevelt 'does not propose to restore the world of 1929 and would not restore it if he could." In these carefully articulated summations as much as in the richly detailed exposition of events, Freidel has contributed to the literature of twentieth-century American history.

WILLIAM E. LEUCHTENBURG Columbia University

RICHARD D. MCKINZIE. The New Deal for Artists. [Princeton:] Princeton University Press. 1973. Pp. xii, 203. \$17.50.

FRANCIS V. O'CONNOR, edited and with an introduction by. Art for the Millions: Essays from the 1930s by Artists and Administrators of the WPA Federal Art Project. Greenwich, Conn.: New York Graphic Society. 1973. Pp. 317. \$22.50.

These two books complement each other well. One, a published dissertation, is a history of the still-controversial government-sponsored art programs that ran from 1933 to 1943. It follows Jane Mathews's book on the Theatre Project and Jerre Mangione's book on the Writers Project. The other work, an anthology, was written in the late 1930s by participants in another Works Projects Administration (WPA) agency, the Federal Art Project.

Richard McKinzie's book is a useful work whose flaws obscure its virtues. The writing is careless: "Prominently displayed in Rose's Club, The Diamond Horseshoe, 43,000 people saw it in three weeks." It also shows the influence of a prolonged reading of Time magazine. A museum director "withered in Benton's retaliatory blast" that was uttered as a "growl." And "one nationwide project, The Index of American Design, busied some 300 individuals for six years." There are breezy misleading oversimplifications: "Artists, flushed from elitist bastions by the depression and caught up in the debate by its dynamism, treated aesthetically what Americans everywhere seemed to be reassessing-the nature of American society." Whatever elitist bastions are, most artists, and especially artists concerned with the nature of society, did not live there. More annoying is the patronizing tone of amused condescension that permeates the descriptions of the anguished struggles of federal art administrators to keep their programs going in the face of insuperable difficulties. Finally, there are few factual errors. George Biddle was neither a Groton classmate of Roosevelt's nor were they at Harvard together. John Sloan certainly never sold a painting for \$50,000 before the depression.

McKinzie states at the outset that his focus "is much more upon social and political forces than upon the creative urge and its results."

This is a legitimate, if limiting, approach. Still, the chief point of the projects was to keep artists at work by paying them to produce painting, sculpture, and graphics on a large scale, and some analysis of the art produced would be helpful. In his discussions of the programs, McKinzie touches on subject matter and stylistic variations, but his account is too superficial and too dependent on received opinion to be of value. Indeed, the book as a whole is far too brief for this significant and innovative episode in American culture.

Although some of McKinzie's conclusions on such matters as the role of the Artists Union and his assessment of the ultimate effect of the New Deal's involvement in the arts are open to question, he performs a real service by distinguishing among and clearly describing the various and sometimes confusing Treasury and WPA programs. The WPA's Federal Art Project, the largest and best known of the art agencies, receives the most extended treatment, and while some of its major achievements—the Index of American Design, the Community Art Centers, the educational programs, and the development of the silk screen process—are given less credit than they deserve, as a summary history the book is useful. It is well illustrated and has a good rundown on the published and unpublished literature on the subject. McKinzie's grasp of the voluminous primary sources as reflected in the footnotes is impressive.

Art for the Millions is another kind of work altogether. It consists of sixty-seven short essays written by artists and administrators connected with the WPA's Federal Art Project. The original intention was to publish the anthology as a report on the work of the project, but owing to a series of delays followed by the collapse of the WPA, the manuscript disappeared into the personal files of the project's chief, Holger Cahill. Resurrected thirty years later and beautifully edited by Francis V. O'Connor, it now appears with helpful additions in the form of biographical notes on the writers, a descriptive inventory of the manuscripts, including a number not published in the text, some statistics on Federal Art Project accomplishments, and a list of Project Community Art Centers. In general, the volume follows the sequence and illustrations originally planned for it. The result is an authentic, refreshing, and occasionally eloquent expression of dedication to the work in which these men and women were engaged. Mr. O'Connor's introduction manages to combine sympathy, detachment, and insight.

GARNETT MCCOY
Archives of American Art

WILLIAM STOTT. Documentary Expression and Thirties America. New York: Oxford University Press. 1973. Pp. xvi, 361. \$12.50.

In recent years historians have been alerted to the neglected possibilities contained in studies of popular culture as a means of achieving a greater understanding of important themes in social and cultural history. Among the most important of these statements is one found in the erratic work of Marshall McLuhan-the noted Canadian English professor turned communications scholar. In his Understanding Media (1963), McLuhan has thrown out the provocative concept that the introduction of a new communications medium into a society drastically alters the way in which that society perceives itself. Unfortunately, few historians have really attempted to test McLuhan's probes, although there is quite a large body of indigested evidence to suggest that in this particular case he may well be pointing historians in the right direction.

William Stott, in this brilliant and seminal work, has given dramatic credence to Mc-Luhan's hypothesis by demonstrating that the "documentary expression" that emerged in the United States during the depression years of the 1930s did in fact allow the country to see itself in a totally new way. (In McLuhan's terms the "sense ratio" of American society was altered.) Stott has carefully researched and analyzed the manner in which the "documentary expression" emerged as a separate genre of artistic expression in answer to a specific need evident in American society during the thirties. Defining documentary as "the presentation of representation of actual fact in a way that makes it credible and vivid to people at the time," he has demonstrated that disillusion required a reaffirmation. Thus the "document," whether it be created by camera or pen (or even voice in the case of radio), allowed American society to re-examine itself to discover both the good and bad, but always with the view that exposition and truth were preferable to neglect or delusion.

Stott has delved deep into the works of the Works Projects Administration (WPA) writers and theater projects; the dance theater of Martha Graham; the emergence of radio and Life magazine; the "documentary tradition" in

social science in order to give more vivid reality to quantitative findings; radical reportage; and of greatest importance, the documentary book, which relied so heavily on new photographic sensitivities. Of course the classic example of documentary expression is Let Us Now Praise Famous Men (1972) by James Agee and Walker Evans, and Stott has rightly singled this book out for an extended analysis. Stott's choice of photographs go beyond their usual decorative function and form an integral part of the author's central thesis.

The one minor weakness, which in fairness to Stott has been treated quite extensively elsewhere, is the absence of analysis of the documentary cinema of the period. The author notes that he has deliberately chosen to ignore this genre in order to concentrate on those that required wider recognition. In all, this is a brilliant, original work and deserves to be widely read and understood.

GARTH S. JOWETT Carleton University

ARTHUR M. SCHLESINGER, JR. The Imperial Presidency. Boston: Houghton Mifflin Company. 1973. Pp. x, 505. \$10.00.

This is a large, bold, and quite persuasive effort to use history for political purposes. The book is concerned with one of the pressing problems of our day: the great power of the American presidency. The author ranges widely over the course of American history in order to throw light on the problem, and he appraises proposed solutions to it, including the impeachment of President Nixon, which he suggests may be necessary "to contain the Presidency and preserve the Constitution" (p. 417).

This is an informative book, but it is much

more than that. While emphasizing the warmaking power, the last thirty-three years, and Richard Nixon, the book supplies a rich body of information on the entire history of the presidency. More important, it helps us think about that history and that institution. Schlesinger presents three concepts for this purpose. One is in the title; the others are the constitutional presidency and the revolutionary presidency. The constitutional presidency can be very strong, but it shares the making of major decisions, such as the decision for war, with other institutions, above all Congress. The imperial presidency, which is largely a product of recent international crises and the reigning interpretations of them, monopolizes rather than shares the power to carry the country into war. The revolutionary presidency attempts to duplicate in domestic affairs the centralization of power established in foreign policy. Nixon became both imperial and revolutionary, but his revolutionary tendencies have been checked. Schlesinger writes that "Watergate was the by-product of a larger revolutionary purpose." "At the same time it was the fatal mistake that provoked and legitimized resistance to the revolutionary presidency" (p. 377).

The author admits that he contributed, at least in a small way, to the rise of the imperial presidency. He cites episodes in his career as scholar and publicist in which he championed claims of presidential power that now seem wrong to him. While one can admire his willingness to admit mistakes, one finds the episodes troubling, especially for the professional historian. They suggest that the author's study of history did not always serve him well. Why did a distinguished historian make such mistakes? Why did he draw conclusions from a narrow range of historical experience? Why was he forced to learn from the history he experienced firsthand?

Schlesinger has changed some of his ideas in response to historical experiences, yet he remains basically the vital-center liberal he has been for years. He has maintained his enthusiasm for Franklin Roosevelt and John Kennedy. Truman, Johnson, and Nixor, above all Nixon, not Roosevelt and Kennedy, are held chiefly responsible for the developments that horrify the author. He rejects proposals, such as Senator Ervin's, that would exalt Congress and subordinate the presidency. Believing that they would go too far and deprive us of needed presidential leadership, he advocates the middle ground or middle way he labels the constitutional presidency. And he refuses to endorse proposals to discard basic American institutions. He calls for more democracy, but what he means is a sharing of decision-making power by and a partnership between two established institutions, Congress and the presidency. His proposals reflect confidence in their ability to function successfully within the limits established by the Constitution. What is required, he suggests at one point, is "a chastened Presidency and a responsible Congress" (p. 376).

It seems to me that Schlesinger draws valid and valuable conclusions from two sets of historical experiences: those of the 193cs and early 194os and those of the 196os and early 197os. The experiences he taps seem to offer the guidance we need, and both sets, not just one, must be taken into account in making decisions about our institutions. Yet, after reading the book, I am troubled by the possibility that the future course of history will reveal that the two served us no better than the one served the 1940s and 1950s. Perhaps a representative of a specialty other than recent American history would be better qualified to appraise this book.

RICHARD S. KIRKENDALL Indiana University, Bloomington

IRWIN F. GELLMAN. Roosevelt and Batista: Good Neighbor Diplomacy in Cuba, 1933–1945. Albuquerque: University of New Mexico Press. 1973. Pp. 303. \$12.00.

United States interest in Cuba is long standing. Although the 1898 Teller Amendment disclaimed annexation of the island, the Platt Amendment and American investment confirmed close political and economic ties. Since events in Cuba after 1959 broke these traditional ties historians have sought explanations. Professor Gellman in his study of the New Deal era shies away from general interpretations for the recent period but notes anti-American feeling in Cuba to which the Good Neighbor policy contributed and that Castro later used. Gellman's story is not entirely new but he tells it in greater detail and from a wider range of primary sources than previous authors.

Good Neighbor diplomacy in Cuba did not change radically the diplomacy of preceding administrations. Gellman suggests that "Roosevelt and his advisers were not trying to implement a new program, but to discard outmoded elements of the traditional one." The Roosevelt administration wanted peace, law, and order in Cuba, goals which TR or Taft would not have faulted. Military intervention was outmoded. Hoover and Stimson would accept that from their experience in Nicaragua, Abrogation of the Platt Amendment did not give up the Guantánamo naval base nor were the Jones-Costigan Act and the reciprocal trade agreement (Hull's first) unmixed blessings for Cuba. Cuba, as Gellman makes clear, was still tied politically and economically to its big northern neighbor, and United States ambassadors in Havana remained as influential as in pre-1933 days. This situation, combined with the use of diplomatic nonrecognition as a political weapon against Grau and support of Batista because the embassy saw him as the only block to anarchy, helped make the United States, in the eyes of many Cubans, responsible and thus the scapegoat for much that was wrong on the island. The author agrees that the United States may be blamed for supporting inequities in Cuba, but he places greater blame on Cuban politicians who when out of office were too negative and when in office failed to advance positive programs of reform. What could the United States have done to avoid hostility that came its way? Here Gellman is not explicit, but he implies that a reduced United States presence would have helped.

Two minor points in this book may be misleading. The title Roosevelt and Batista is not accurate: President Roosevelt had final responsibility for decisions, but indications are that he thought little about Cuba and his role was relatively minor. The chief characters of the book are the ambassadors—particularly Welles, and Caffery—and Secretary Hull. One may also question the author's implication that the middle 1940s form a natural break in United States-Cuban relations. There may be an end of an era within each country, but how far this carries over to their foreign relations is doubtful.

WILLIAM KAMMAN
North Texas State University

VLADIMIR PETROV. A Study in Diplomacy: The Story of Arthur Bliss Lane. (Foundation for Foreign Affairs Series, number 14.) Chicago: Henry Regnery Company. 1971. Pp. 302. \$12.00.

RUSSELL H. FIFIELD. Americans in Southeast Asia: The Roots of Commitment. New York: Thomas Y. Crowell Company. 1973. Pp. xi, 417. \$10.00.

Each of the two books under review is a story of a failure—one personal, the other national. Arthur Bliss Lane succeeded in not one of his major diplomatic assignments. In Nicaragua, where he went as minister at age thirty-nine (the youngest career minister in the history of the foreign service), he was caught in a web of intrigue and rivalry among contending aspirants for the presidency, and he fared badly. He was accused of meddling in the country's affairs and even of complicity in the murder of one leader. Lane was unsuccessful in convincing the State Department to accept his recommendations (one of which was quite bad) and eventually became a diplomatic liability. He found Yugoslavia, where he was posted in 1937, of no interest to Washington, and he became irritable and dissatisfied because the post was unimportant. When the war began he tragically misread the ability of the Yugoslavs to resist the blandishments of the Nazis, and his efforts to arrest

the drift of the leaders into Germany's arms proved fruitless. After the coup that ousted the prince regent, Lane had no contact with the new government and did not keep the department advised on developments. His experiences in Poland, where he was accredited ambassador in 1944, were no happier. He was always at odds with Washington, which he accused of sacrificing Poland's independence and freedom for Russia's friendship. His insistence that Stalin be made to hold free elections as pledged at Yalta earned Lane the label "troublemaker and warmonger." His bombardment of the department with advice to get tough with the Soviets was viewed by his superiors as evidence of inflexibility. When it became clear to him that Poland was to be permitted to slip into the Soviet orbit, he left Warsaw and, failing to get another post to his liking, resigned from the service in bitterness, "having lost trust in the wisdom of the men who directed the policies of the United States."

No less a failure was the American experience in Southeast Asia. It is a sad record of missed opportunities and of drift into the morass of deep involvement. After the war the Philippines presented no problem to American diplomacy; the islands were turned loose. The Dutch similarly gave Indonesia independence as did the British to the Malay States. But the French clung tenaciously to their Asian holdings. President Roosevelt opposed the continuation of French rule in Indochina after the war and expected independence for the colony to follow a period of trusteeship. But the plan died with him. No one seems to know why it was abandoned. That was one opportunity missed. Another was the failure to convert Ho Chi Minh into an "Asian Tito." In 1945 and 1946 there was a considerable reservoir of good will on Ho's part for the United States. America stood for freedom and independence for dependent peoples, and Ho expected Washington to aid him in his aspirations. But Washington did not capitalize on the situation. Then there was the chance to pressure France into granting complete independence to her colonies when, in 1949, they were organized as autonomous and associated states in the French Union. It, too, was lost.

Those lost opportunities led logically and inexorably to supporting the French with money and equipment in their fight against Ho's communist forces and, eventually, to replacing them when they withdrew from the fight after Dienbienphu. The ultimate failure was that after eight years of cruel warfare and after the

expenditure of billions of dollars and the staggering loss of lives, the American effort may, after all, have been in vain. Bereft of American military support, South Vietnam may yet succumb to communist conquest or subversion. At best, our intervention bought time.

Fifield's book may not be the "landmark study" the dust jacket claims it to be, but it is a lucid, thoughtful, and extremely useful account of how and why the Indochinese debacle came about. There are no surprises; the story is well known but it is enriched by great detail drawn from a wide variety of sources and told with the sure touch of the professional. It is told, too, against a broad backdrop of events elsewhere-chiefly the rest of Southeast Asiaand in the context of the historical setting of the cold war. Fifield's explanations and conclusions are reasonable and intelligent. The assumption of American policy, he says, was that Indochina had to be saved as Korea was saved. The people and the land could not be permitted to fall to communism. We did not stumble into the war. Each successive president did what he thought essential at each stage to keep Indochina free. Truman and Eisenhower provided aid to the French to fight the fight and when they quit Kennedy and Johnson took over-the former moderately and clandestinely, the latter openly and massively. Fifield is particularly good on the struggle within the corps of American policy makers between those who wished to push the French to free their colonies and those who feared that such a move would alienate the French and endanger the solid front against the Soviets in Europe. If his account has any weakness, it is his failure to consider the public attitudes. He deals with congressional reactions and participation in decision making adequately but leaves unexplored public opinion, which counted for so much in the end.

Petrov's account is not nearly as sophisticated as Fifield's nor as analytical or convincing. He does not prove his major contentions—that Lane's record was outstanding, that he was successful in sizing up the frame of mind of the White House and of the people in the department, and that he had an uncanny knowledge of human nature and the ability to exploit the weaknesses of individuals. Petrov's narrative is marred by an unexplained failure to cite specifically the manuscript material he used in the Lane Papers and in the Department of State archives. His credibility is thereby impaired. It is strange that he cites specifically from printed sources. To his credit is his critical approach to

his subject. There is no hero worship in the book, and Petrov recognizes Lane's limitations. He characterizes Lane's reports as "rarely profound," and finds him on occasion to be less than candid. To Petrov's credit, too, is his analysis of the historical circumstances in which Lane operated, particularly in Poland. As a Russian emigré, Petrov knows that part of the world well.

ARMIN RAPPAPORT
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San Diego

PAUL A. VARG. The Closing of the Door: Sino-American Relations, 1936–1946. [East Lansing:] Michigan State University Press. 1973. Pp. x, 300. \$10.00.

JOHN M. ALLISON. Ambassador from the Prairie: Or Allison Wonderland. Boston: Houghton Mifflin Company. 1973. Pp. xiv, 400. \$7.95.

In an editorial for February 19, 1943, the New York Times observed "how much the thought and aspiration of China are like our own . . . we are both fighting for the same purpose." The occasion was the triumphal appearance of Madame Chiang Kai-shek before the United States Congress. Professor Varg barely mentions the visit, for he is intent on showing that American and Chinese purposes were not and had never been the same. The popular image of the United States as a defender of China was a myth. Prior to World War II, American policy was that of an imperial power; during the first three years of the war, policy centered on the defeat of Japan. Chinese concerns, on the other hand, were internal; the contest for power between the Nationalists and the Communists. Differing purposes created tension, but America's major blunder, in Varg's thesis, came when the United States intervened in the domestic

For Varg the turning point came in 1944. Increasingly disillusioned with Kuomintang conduct of the war, Roosevelt first tried to force Stilwell on China and then sent an observer mission to the Communists in Yenan. The warm welcome given the mission encouraged the United States to think that coalition government was possible in China. Chiang, apparently to avoid giving Stilwell command of the Chinese army, requested a special envoy. Patrick Hurley arrived and, after some vacillation, strongly supported the Nationalists. He did so despite the warnings of experts like former Ambassador Gauss, Service, Vincent, and Davies. Thus, Varg concludes, Hurley helped "lead his

country down the tortuous path that led to twenty-five years of support of a regime long after it had lost all credibility in its own territory" (pp. 165–66). The Marshall mission, despite an auspicious beginning, never had a chance, for Hurley had destroyed whatever credibility the United States had with the Communists.

Drawing upon recently available American archives, Varg has written a balanced, scholarly narrative. A number of questions, however, remain unanswered: was there any real possibility before Hurley for a political coalition; what was the basis for Washington's belief that Chiang could unify China; what effect did growing American suspicion of Russia have on China policy; how influential were pro-Chiang public-opinion leaders like Congressman Walter Judd and Henry Luce of Time-Life? It is clear, as Varg shows, that the United States did not "lose" China. Americans, including her policy makers, have long preferred myths about China to looking beneath the surface at the realities.

Ambassador Allison is a kindly man; there are no evil governments or individuals in his book. During a thirty-year Foreign Service career, Allison served in Japan and China in the 1930s, was interned in Japan for a few months, spent the war in London, served with the American delegation to the United Nations, helped John Foster Dulles negotiate the Japanese peace treaty, was assistant secretary for the Far East in the early 1950s, ambassador to Japan from 1953 to 1957, then minister to Indonesia for eleven months, rounding out his career as ambassador to Czechoslovakia. His spritely reminiscences abound with anecdotes about major figures, MacArthur, Dulles, Nixon, Edgar Snow, Christopher Morley, and fond memories of ducknetting parties and cormorant fishing.

The most valuable portions historically are Allison's experiences in Japanese-occupied Nanking in 1938, negotiations leading up to the Japanese peace treaty, and the frustrations of Sukarno's Indonesia. Allison was one of the first officials to land in Nanking after the Panay incident. The peace negotiations of 1950-51 were his most interesting and satisfying period, for he was closely involved in the making and implementing of policy. Indonesia was "fascinating" despite the fact that internally "nothing seemed to work, and no one really seemed to care . . . " (p. 294), and Washington consistently ignored his cables.

Allison obviously enjoyed his Foreign Service work, especially the times in Japan. His internment was even almost pleasant. There is no

bitterness toward American policy makers who too often ignored their career officers; he even has a few kindly words for Patrick Hurley. Allison ends optimistically, believing there is a role for the Foreign Service in bringing nations together. For the ambassador from Nebraska, his career was a "wonder"-land.

JAMES M. MCCUTCHEON University of Hawaii

GEORGE C. HERRING, JR. Aid to Russia, 1941–1946: Strategy, Diplomacy, the Origins of the Cold War. (Contemporary American History Series.) New York: Columbia University Press. 1973. Pp. xxi, 365. \$15.00.

In this well-researched, judicious, and cohesive study Professor Herring discusses lend-lease and postwar aid to Russia within the larger context of Soviet-American relations during the developing cold war. He defends the "unconditional aid" policy for lend-lease, arguing that it was necessary because of Russian needs and sacrifices and the importance of keeping the alliance together by minimizing tension. He grants that Americans suffered indignities at the hands of Stalin and crude Russian bureaucrats, that wastage occurred, that Russia enjoyed favors, and that FDR did not demand political concessions in return for the ten billion dollars. Lendlease was a test of good faith at a time when the second front was being delayed and when FDR refused to meet any of the Soviet requests for territorial concessions. Above all else, concludes Herring, American generosity served the calculated national interest: the defeat of Germany and the preservation of American security. FDR is pictured as a realist quite conscious of his goals, constantly stymied by a resistant bureaucracy. One of the more interesting stories in this book is that of the debate within the administration over the generosity of lend-lease policy. FDR, Hopkins, and Stettinius outdueled admirals, generals, and Ambassador William Standley. FDR realized that his "commitments" on aid to Russia were infrequently fulfilled because of bureaucratic footdragging, domestic shortages, and immense shipping problems.

Ambassador W. Averell Harriman is the link between lend-lease and postwar aid, for he began in 1944 to gain support in the administration (and later with Truman) for his position that all aid should be used as a diplomatic weapon to force Soviet agreement on international issues. Herring is certain that postwar aid (via a large loan) would not have made

much difference in the cold war. His conclusion is curious given his other statement that Russia was antagonized by the abrupt cutback in lendlease and the American handling of the loan question. He gives little attention to the possibility that aid might have helped smooth relations to permit the solution of other issues in a less tense atmosphere. And, importantly, he ignores the connection between Soviet intransigence on German reparations and the lack of American postwar aid. He is persuasive in pointing out the shift to a tougher aid policy in early 1945, wherein American statesmen spoke of the possible diplomatic benefits derived from economic pressure, and in explaining the delay and public silence in handling the Russian loan request from August 1945 to February 1946. In the latter case, however, he fails to note that Washington activated it in early 1946 in part to gain Soviet concessions over the World Bank and the Iranian crisis.

One consistent theme is that "domestic politics" placed "sharp limitations" on the presidents' freedom. Herring cites polls and quotes disparate congressional voices that questioned a generous policy or any aid to Russia. But he never demonstrates concretely that such questioning was decisively influential. To cite the National Grange as evidence that farmers opposed aid to Russia is inadequate. Then, too, when Congress was moving to a more critical position on lend-lease, FDR was moving along his own parallel path (as did Truman later). Who led whom? Indeed, the book is confusing on this fundamental point because at times it asserts the primacy of public pressure and at other times it presents evidence to illustrate that "considerations of diplomacy" were uppermost in aid decisions.

Herring's treatment of the cutback in lendlease illustrates the problem. First, it should be noted that the Foreign Economic Administration chief Leo Crowley was not simply acting as a legalist following the legislative act to the letter. Crowley related aid to other issues in Soviet-American relations, as did most American leaders. Second, the Lend-Lease Act was explicit about ending it at the termination of the war, but a different interpretation of the act was possible from May to August 1945, as Acheson pointed out in his memairs. The Truman administration was justified by law in curtailing lend-lease so abruptly in May, but its interpretation of the law derived from the assumption that economic power as a diplomatic weapon would make the Russians more pliable; a different assumption might

have produced a different interpretation of the law.

These questions do not diminish Herring's contribution to cold-war history. Its research, synthetic accomplishment, and fair-mindedness are welcome in a field too often marked by shrill, intemperate shouting. His portrayal of FDR as a diplomat and manager of the foreign affairs bureaucracy and his careful interweaving of political, military, and diplomatic history are noteworthy.

THOMAS G. PATERSON
University of Connecticut

RICHARD W. STEELE. The First Offensive, 1942: Roosevelt, Marshall and the Making of American Strategy. Bloomington: Indiana University Press. 1973. Pp. ix, 239. \$8.50.

Steele lucidly traces the tangled threads of decision making that led to Torch, the Allied invasion of Northwest Africa in November 1942. On no other strategic decision did General Marshall disagree so strongly with President Roosevelt. It has been said that the Torch controversy divided the Combined Chiefs of Staff and sorely tested the Anglo-American military coalition. Critics of Torch have frequently pointed out its effect upon the duration of the European war in causing the postponement of the "Second Front."

Most previous writers have stressed the difference in strategic outlook between the Allied positions, with the British strategy of attrition emerging temporarily triumphant over the American strategy of annihilation. Steele, however, maintains that the Torch decision "involved little conflict between American and British military strategists," but resulted from Marshall's efforts "to accommodate his own military judgments to the politico-military demands of President Franklin D. Roosevelt" (p. vii). After the Arcadia conference in the winter of 1941-42, Marshall was increasingly concerned that British persuasiveness and American political pressures would influence Roosevelt to favor a Northwest-African venture, which the War Department considered unwise. Marshall subsequently became dedicated to Sledgehammer, a plan for a limited cross-Channel invasion in late 1942 to draw German pressure from the Soviet front. Not until Roosevelt, facing vociferous British objections to a premature attack on France, committed the United States to Torch in July 1942 did Marshall drop the Sledgehammer scheme.

Steele emphasizes that the Sledgehammer

attack would have ended in certain disaster. and he is severe in criticizing Marshall: "He was in a position to know the facts that made SLEDGEHAMMER a false vision, yet continued to press for the operation long after the high costs and probable negative results were apparent. To the extent the President depended on Marshall for military advice on the first offensive, he was ill-served" (p. 181). Steele believes that Marshall was serious about Sledgehammer, but motivation in this type of case is difficult to delineate conclusively. The Marshall disciple could respond that the Chief of Staff took such an extreme stance not because of dedication to Sledgehammer but in order to avert Torch and thereby save Roundup, his cherished plan for a cross-Channel attack in 1943.

With thorough research, a vigorous style, and a thought-provoking thesis, Steele, who is a historian at California State University, San Diego, has done an excellent job of presenting the military, political, and personal factors underlying the decision to launch the first Anglo-American offensive against the European Axis.

D. CLAYTON JAMES
Mississippi State University

PETER CLECAK. Radical Paradoxes: Dilemmas of the American Left: 1945-1970. New York: Harper and Row. 1973. Pp. x, 358. \$11.95.

Peter Clecak has written a provocative book, not about the dilemmas of the American Left from 1945 to 1970, as the title asserts, but of the responses to those dilemmas in the work of four leading Marxist theorists. He wisely selects C. Wright Mills, Paul Baran, Paul Sweezy, and Herbert Marcuse, described as "plain Marxists," that is, independent and undogmatic critics who use Marxism as a method, not as a "body of sacred texts." All are men of reason; all are committed to a democratic socialism; all led lives of anguish trying to reconcile those commitments to the realities of world power.

They examined the overriding questions radical critics have yet to answer: in the face of massive state power and stability, what kind of socialist movement can be created in the United States; how is a socialist society to be created and sustained so that humane and democratic commitments are honored; what socialist visions permeate the ideological Left? Clecak traces in their work the evolution from optimism to gloom, from what he describes as

a realistic socialist model to an apocalyptic communist future that presupposes a new socialist person and a society free of alienation. All of them failed, says Clecak, all of them end as prophets of doom, living on the periphery of society, clinging desperately to an unrealizable and dangerous vision of a utopian mirage that will inevitably prove to be a "cruel political illusion."

In the last section, Clecak offers his own vision, a concrete model, a socialist society incorporating the work ethic, limited competition for material incentives, genuine social mobility, and greatly extended democracy. If critics are "freed from the illusions of a perfect society and an infinitely perfectible man," then a democratic socialism, consistent with the cultural life of America, is feasible.

This is a serious and thoughtful book, but it is not without flaws. The intimate and organic connection between the work of these men and the world in which they are actively engaged is missing, so that their failure, as defined by Clecak, appears as a personal and intellectual one. Clecak's realistic alternative carries with it two unexamined assumptions: first, that the two visions, utopian and realistic, are necessarily incompatible; and second, that a feasible solution is possible now. In fact, all four of his critics started out where Clecak is now. By focusing, somewhat unfairly, on their ultimate visions, he often fails to demonstrate the inadequacy of their specific analyses. Sweezy and Baran, for example, are economists, and yet Clecak does not challenge their economic critique, which forms the basis of their gloomy conclusions. Clecak's strategy, coming after trenchant criticism of others, is tepid and disappointing. His four critics would undoubtedly answer Clecak by saying that they have seen his future and it does not work.

ANN J. LANE
John Jay College,
City University of New York

ROBERT A. PACKENHAM. Liberal America and the Third World: Political Development Ideas in Foreign Aid and Social Science. Princeton: Princeton University Press. 1973. Pp. xxii, 395. \$15.00.

In his estimate of the political situation in the Dominican Republic after the assassination of Trujillo in 1961, President Kennedy could perceive only three possibilities for the Dominican people: a decent democratic regime, a continuation of the Trujillo regime, or a Castro regime.

"We ought to aim at the first," he observed, "but we really can't renounce the second until we are sure that we can avoid the third." Puerile thinking of this sort-and the consequences it produced-provide the frame of reference in which Robert A. Packenham historically and analytically examines foreign aid as an aspect of American policy toward the third world. Designed to promote constructive political development in the third-world countries (economic growth, political and social stability, democracy), the aid program instead contributed to uneven economic development, political instability and violence, and authoritarian rule. In the face of the obvious failure of their approach, aid officials and the social scientists whose theories underpinned the program clung stubbornly to a conviction that what worked in Europe after World War II would surely work elsewhere in the world. Why?

Packenham finds the answer in the predominance of the liberal tradition among the theorists and practitioners of foreign aid. Acknowledging his intellectual debt to Louis Hartz, he argues that the "American exceptionalist" frame of mind provides perhaps "the single most important ideological and perceptual lens through which Americans perceive, evaluate, and understand political development in poor countries." Packenham does not claim that all foreign aid policies are explicable in terms of the liberal tradition; but he insists that the doctrines and theories were significantly influenced by at least four absolutist premises derived from this nation's historic and uncritical love affair with liberalism: that change and development are easy, that all good things go together, that radicalism and revolution are bad, and that distributing power is more important than accumulating power. He concedes that economic needs and demands occasionally helped to determine American policies on political development, but they fall far short of explaining all that needs to be explained.

In his insistence that security perceptions (whether correctly focused or not) and liberal ideology conditioned American efforts to effect political changes overseas, Packenham offers a sound and useful corrective to some of the more sweeping assertions of revisionist historians about the pre-eminently economic thrust of American policy in the cold war. It is good to be reminded, for example, that this nation's policies did not develop in an ideological vacuum. Just as it is impossible to understand Soviet policy in political development outside Russia without understanding Marxist-Leninist

ideology, so it is futile to explain American doctrines and policies in political development without reference to the less explicit, less self-conscious, yet overwhelmingly pervasive liberal tradition.

With that enviable license political scientists enjoy, Packenham prescribes for the future; and while his suggestions are reasonable ones, they are somewhat beside the point. American aid policy must be reoriented in the direction of a low profile, neutral diplomatic approach. This country should "get out of the way" of "certain kinds" of third-world political developments. It should learn to accept radical and revolutionary modes of political change-"under certain circumstances." This prescription, of course, presupposes a radical shift in American attitudes toward social change both at home and abroad. In the end, Packenham must rely optimistically upon the realism of American politics to produce the results he desires in foreign policy, a very thin—and traditionally liberal-reed upon which to lean.

JOHN G. SPROAT
University of South Carolina

LEONARD GREENBAUM. A Special Interest: The Atomic Energy Commission, Argonne National Laboratory, and the Midwestern Universities. Ann Arbor: University of Michigan Press. 1971. Pp. xxii, 222. \$10.00.

This well-written book, based upon meticulous and exhaustive research, deals not with one but with a congeries of special interests. The big picture that Greenbaum presents requires a two-part caption: first, we see a scientific community trying to maintain its public image as an antiseptically objective, and thus united, elite group whose only motivation is selfless service to Knowledge; second, we find a tireless struggle by contending supplicants within the scientific community for a limited, albeit very large, bounty of government funds.

The focus of the narrative is the Argonne National Laboratory in Illinois, one of several laboratories established after the end of the Second World War by the Atomic Energy Commission. The burden of the narrative is the way in which the AEC resisted for years the pressure of high-energy physicists in two dozen Midwestern universities to build for them at Argonne or elsewhere a giant accelerator that would be at least as big and costly as those already being built on the East and West coasts. In 1947 the AEC decided to concentrate its nuclear reactor development program, which

included the submarine propulsion reactor, at Argonne. The chief result was that the laboratory was interested primarily in development of actual reactors; "pure" research, which is what the university physicists were interested in, was squeezed aside by work the laboratory management and the AEC considered more urgent.

When, in 1955, an accelerator was to be installed at Argonne, the physicists wanted a bigger machine; furthermore, they wanted it under their control, not Argonne's. The physicists were supported in their lobbying by their university presidents, who apparently swung sufficient weight to force the AEC to listen. The AEC's response, in order both to save face and to get the presidents off their backs, was to go ahead with the Argonne accelerator while promising the Midwestern universities, some time in the indefinite future, a "dream machine"-"the best accelerator the world has ever seen." Sixteen years and five hundred million dollars later, the dream machine was installed some thirty miles from Argonne, in Weston, Illinois, under separate management.

Weston marked the political triumph of the Midwestern physicists over Argonne, which nevertheless continued to maintain its own empire intact. The only losers have been the public. Greenbaum does not say all this in so many words, because his purpose was not to be as dogmatic as this reviewer but to provide an even-handed treatment of an unbelievably complicated story of twenty-five years of lobbying, negotiations, confrontations, and evasions. Nevertheless, Greenbaum has a particular point of view, which comes through clearly; he is simply wise enough to recognize that other valid points of view are possible. In any case, he has done well a very considerable task. In one way, the book required far too much work for the importance of its subject. This Midwestern accelerator controversy is hardly worth more than a footnote in the postwar scientific pork-and-caviar-barrel operations. Yet such a book serves a useful purpose in describing, in a believable microcosm, the whole scientific enterprise. The kind of painstaking detail that Greenbaum supplies in his book is needed to convince most of us that the scientific community can in fact be so self-serving while it protests publicly its disinterest and objectivity.

Greenbaum leaves to more consistently polemical authors, such as Daniel S. Greenberg (e.g., in his *Politics of Pure Science* [1967]), the general proposition that scientists, in common with other interest groups, are guided less by

the logic of scientific patterns than by the availability of money, but that they will nevertheless fight hard to maintain and expand whatever field of specialization they may have chosen at the outset of their careers. In its own way, A Special Interest makes a more convincing case than do polemical works. It will probably have a longer shelf life, too.

EUGENE S. FERGUSON
University of Delaware
Hagley Museum

RICHARD F. HAYNES. The Awesome Power: Harry S. Truman as Commander in Chief. Baton Rouge: Louisiana State University Press. 1973. Pp. vii, 359. \$12.95.

This book has the misfortune to be sandwiched between the feisty gossip of Merle Miller's Plain Speaking, and the elegant revisionism of Bert Cochran's Harry Truman and the Crisis Presidency. It lacks the flair to compete with either-more's the pity, for it is a competent work, logically organized and carefully researched. But it tends to be corroborative rather than groundbreaking, and since it adds nothing really new to the controversy between the cold-war revisionists and their counterrevisionist cousins, both are likely to ignore it. Indeed, Haynes abjures judgments in most cases, speculative or otherwise, and so remains curiously aloof from the thicket, which is unfortunate, for his subject lies at its heart.

Aside from a few minor errors (the author seems unaware that Negro aviation units fought in Europe, for instance), there is little to criticize except Haynes's hesitancy. For example, Truman's refusal to seek congressional sanction for the Korean intervention is puzzling. Even an informed guess about Truman's motives, given Haynes's obvious familiarity with archival sources, would be valuable. All we get, however, is a statement that Truman's actions were "difficult to comprehend" (p. 183). Similarly, Truman's advocacy of universal military training (UMT) is interesting because it tells us something about his character, which was a blend of emotional egalitarianism and impetuous authoritarianism. The totalitarian overtones of UMT bothered Truman not at all, thus illustrating the snappish cocksureness that enabled him to urge the drafting of strikers, to threaten mayhem on music critics, and to rest easy ever after with the A-bomb decision.

In retrospect, something like UMT might have prevented front-line units in Vietnam from being predominantly poor, black, or both, but probably not. The progeny of the privileged, like the Bundys and the Rusks, would surely have had the wits to emerge from UMT in staff, liaison, and intelligence billets, thereby leaving the dying to the sad sacks and the hot bloods. Still one wonders, and Marshall's fear that selective service "imposed on too few the entire burden of military service" (p. 87) seems eerily accurate. Perhaps Truman had something, though of course he did not regard military service as a burden but rather a privilege, which indeed it was for him, since it lifted him out of the obscurity of his first thirty-three years. Unfortunately, Haynes leaves us to draw most of these conclusions for ourselves.

Haynes sees Truman as the first of the "new" presidents (or as Schlesinger would say "imperial" presidents) because he handled major crises without formal congressional advice and consent. Indeed, Haynes suggests that Truman acted decisively in times of crisis because, hamstrung as he was domestically by the conservative coalition, they were the only times he could act. Such opposition as Truman encountered came mainly from conservative Republicans like Robert A. Taft, who usually proceeded to cut the ground from their own position by approving Truman's actions in practice, while complaining about them merely in principle. In short, Truman set the stage for the notion that the president might properly make his most crucial decisions in lonely isolation. It would take the agonies of the Johnson and Nixon years before substantial numbers of congressional critics would begin to quarrel with that notion.

GEORGE E. HOPKINS
Western Illinois University

Foreign Relations of the United States, 1948. Volume 9, The Western Hemisphere. (Department of State Publication 8626.) Washington: Government Printing Office. 1972. Pp. xii, 801. \$6.75.

This ably edited volume reveals how the Latin American nations reacted to the priority that the United States had given by 1948 to strengthen Western Europe. Latin America had its own priorities—for export-import loans, for economic aid to improve infrastructure and alleviate dollar shortages, for military assistance and arms purchases, for support in disputes with rival neighbors, plus a host of other issues ranging over the wide spectrum so characteristic of relations between "the one and the twenty."

Secretary of State Marshall and other State Department officials were sympathetic and often helpful in responding to Latin American concerns. But the word went out—and at times had to be repeated for emphasis—that American resources, expended so lavishly during the war, were not limitless, that commitments to Europe would come first, and that Latin America would have to adjust accordingly.

Even so, Secretary Marshall hardly turned his back on Latin America. He dutifully attended the Ninth International Conference of American States at Bogotá, where rioting mobs, triggered by a political assassination but exploited to result in planned violence and arson, probably communist-inspired, temporarily endangered the American delegation. The disturbances damaged but did not destroy the conference. Bogotá proved, perhaps, that diplomacy by formal inter-American conference was a risky nineteenth-century legacy to a United States now heavily burdened with foreign obligations. In spite of its many difficulties Bogotá produced the Charter of the OAS, an achievement, Mecham persuasively has argued, that represented a cumulative result of all the endeavors of inter-American diplomacy since 1889.

What complicated inter-American diplomacy further, of course, was that the United States had its own special interests and goals to advance in bi-lateral negotiations-settlement of wartime lend-lease accounts, retention of certain key military bases, encouragement to private American oil investment and exploration by lessening Latin American restrictions, achievement of the proper response to governmental overthrows (should nonrecognition be a diplomatic weapon?), and the long-standing hope that Latin America could combine political stability with meaningful domestic reforms. What stands out in today's world is the Pentagon's insistence, strongly supported by the State Department, that now was the time to make the Western Hemisphere self-sufficient in oil and Secretary Marshall's realistic argument, stated in several despatches, that the best means of defeating communism in Latin America was to institute internal political and economic reform.

The overall portrait that emerges is not that of a United States overemphasizing the Communist threat and trying hard to bend inter-American relations to counter it, as Connell-Smith has maintained. It is rather that of a nation alert to the possibility, in Brazil and Chile especially, that the Communist threat

did exist, and should be watched. Clearly beset by 1948 with mounting problems in a restive world, the United States was discovering how limited its own capabilities were in meeting them, perhaps especially so in Latin America.

This volume contains interesting evidence to suggest that some Latin American countries tried to enlist in the cause of anticommunism as a means of winning support from the United States. But there is not much evidence to prove that the State Department responded eagerly to such gambits. In a revealing interview Perón confessed that he had ordered his foreign minister at Bogotá to "play a little poker" with the United States. The American embassy's analysis and assessment of Perón's famous "third position" stand up as realistic, professional diplomacy at its best.

Much of American diplomacy in the hemisphere during 1948 related closely to economic and military issues. Thus exchanges of correspondence with officers in the Pentagon, with Defense Department specialists, and, above all, with Treasury and other officials charged with economic policy, take on special importance. The editors have selected such documents skillfully and judiciously, giving another impressive demonstration why historians of American diplomacy are so much in debt to the Foreign Relations series.

RUSSELL H. BOSTERT
Williams College

ELMO RICHARDSON. Dams, Parks & Politics: Resource Development & Preservation in the Truman-Eisenhower Era. [Lexington:] University Press of Kentucky. 1973. Pp. 247. \$11.25.

With a thoroughness reminiscent of Donald C. Swain's Federal Conservation Policy, 1921-1933 (1963), Elmo Richardson has plunged with both hands into the history of the politics of conservation in the decade following World War II. There will be many, even in the historical profession, for whom Dams, Parks and Politics will constitute more than they could ever want to know about natural resource administration from 1946 to 1956. But for the specialist in the environmental or political history of the Truman-Eisenhower years, the book is a fascinating microscope. Unquestionably Richardson is unique in his ability to explain (not just describe) resource-oriented decisions in the period of his concern. For the nonspecialist the generalizations that occasionally rise from Richardson's details are the significant contribution. He helps build historical understanding of the characteristics of the Truman and Eisenhower administrations, their relation to each other as well as to the preceding Franklin D. Roosevelt years. Richardson also helps in the currently vital task of understanding how wise and unwise decisions regarding the environment are made.

On balance, the failings of Truman and Eisenhower resource policies outweigh the achievements. Richardson's record of bureaucratic inertia, profiteering, partisan politics, and shallow ecological understanding constitutes a handbook on how not to manage the environment. After tracing the demise of plans for the Columbia Valley Authority, Richardson argues that Eisenhower's election in 1952 constituted a "mandate for change." This was nowhere more evident than in resource policy where two decades of relatively tight federal control by the Democrats left private economic interests with prodigious appetites for exploitation. The presence in the post of secretary of the interior of onetime Oregon Chevrolet dealer Douglas "Giveaway" McKay did little to stem the tide. Suddenly in the early 1950s it appeared that dam builders, loggers, grazing interests, and oilmen would be given a free rein. Even the national park and monument system was threatened. The achievements of a half century in nature preservation and ecologically sound, federally controlled use of the public domain seemed about to be lost in the surge away from the New Deal.

The test case, and the major episode in this book, involved a dam proposed by the Bureau of Reclamation within Dinosaur National Monument on the Colorado-Utah border. Only an all-out effort by a coalition of friends of national parks and wilderness blocked Echo Park Dam in 1955. This, in many opinions, saved the national park system in the form we know it today. Complete understanding of the Dinosaur issue depends on a broader grasp of the intellectual context than Richardson reveals, but by detailing the political infighting of the controversy he supplies a vital part of the whole.

RODERICK NASH
University of California,
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TOWNSEND HOOPES. The Devil and John Foster Dulles. (Atlantic Monthly Press Book.) Boston: Atlantic—Little, Brown. 1973. Pp. xiv, 562. \$15.00.

Shortly after resigning as under secretary of the air force in 1969, Townsend Hoopes wrote Limits of Intervention. In it he described policy making in the Indochina War of the late 1960s and suggested that the road to sensible American foreign policy had been lost years before. In his new book, The Devil and John Foster Dulles, Hoopes attempts to pinpoint the origins of calamity by providing a full-scale portrayal of one of this country's most important foreign policy makers.

This book is a good narrative and provides the most elaborate and careful chronology yet available of American foreign policy during the Eisenhower years. Whether the book is much more than chronology is something else again. As an analysis of American foreign policy, and a key policy maker, it remains too much mired in soggy old clichés that have been a mainstay of liberal criticism. Hoopes demonstrates no familiarity with the extensive work on U.S. foreign policy cone by historians, political scientists, and economists in recent years and his work remains theoretically immature as a result. Even in source materials, he has relied almost exclusively on long afterthe-fact interviews and secondary works, by and large ignoring the rich resources of Dulles's personal papers and the Eisenhower Library.

Beneath his chronology, for example, Hoopes describes the towering figure he takes as his focus with a traditional amalgam of unattractive characteristics: an enormously ambitious striver determined all his life to be secretary of state; a devout Presbyterian and a rabid anticommunist, a man of total inflexibility in negotiations with the infidels in Moscow or Peking; and a shrewd and ruthless lawyer given to tactical preoccupations rather than long-range planning. Under the sway of this man, American foreign policy during the 1950s took all the wrong directions. The firmness of Truman and Acheson, justified during the 1940s, Hoopes argues, was maintained to the point of rigor mortis. From Guatemala and Cuba, to Egypt and Iran, to Vietnam and Quemoy-Matsu, communist devils were seen plotting-and called forth blistering rhetoric and missile-rattling maneuvers. Hoopes maintains that Dulles and those he dominated never grasped the complex drives of nationalism or the heterogeneity of the world of the 1950s. A little more reasoning perceptivity might have opened more roads to world peace and avoided tragedies like the Indochina War-and at Dulles's door, Hoopes lays heavy responsibility for it.

What a relief it would be for critics of American foreign policy if it were all this simple—if American foreign policy in the

1950s owed all its dismal failings to a megalomaniacal anticommunist christer and to the weakness of Eisenhower and all those who let him hold American foreign policy by the throat. No, Hoopes has created his own devil in looking back at that era. Describing Dulles reacting to every crisis by glowering at Moscow or Peking, Hoopes might just as well be describing his own tendency to see Dulles's religious anticommunism at the root of all U.S. policies. Nowhere does he come to grips with the question of what real U.S. interests were served by vehement anticommunism in these years; for example, while Hoopes mentions Dulles's long career as an international business lawyer, he makes no attempt to tie this background in with such crucial matters as policy toward Germany, the Middle East, and Japan. In this regard, he shows no awareness of the materialistic side of the Calvinist coin. Nor does Hoopes seriously consider the utility of anticommunism in the 1950s—the way in which it could be used to gain congressional or public approval for administration policies as it was in the 1940s.

For all of Dulles's clear susceptibility to criticism, in other words, something more solid seems to be called for, something more than a series of Herblock cartoons put into prose. The dichotomy of a good Truman-Acheson team versus a bad Eisenhower-Dulles team is too simple: it ignores the complexity of foreign policy during the 1950s and, as well, the possibility that the tragedy of American diplomacy may go back further than Dulles's tenure.

RONALD W. PRUESSEN Temple University

STEPHEN R. WEISSMAN. American Foreign Policy in the Congo, 1960-1964. Ithaca: Cornell University Press. 1974. Pp. 325. \$13.50.

This provocative study is a comparative analysis of the Congo policies of the United States during the Eisenhower, Kennedy, and Johnson administrations. The author interviewed many of the decision makers on Congo policy during the years 1960-64, and made good use of abundant other source materials. He is critical of both the moderate Republicans and the liberal Democrats for their fixation on the danger of communism as a motive for intervention in the Congo. In his own view it is "practically impossible to imagine the Congo slipping into the Soviet camp" (p. 279). He believes that some of the liberal Africanists stressed the danger of Communist penetration,

not because they believed it, but in order to goad the United States into action. He points out, however, that the anti-Communist policy of the United States in the Congo was qualified and even shaped by our deference to Belgian policy, "a deference which was in part the customary nod to NATO partners and in part the conservative ideology of the Eisenhower administration."

In a series of interesting biographical vignettes, he suggests that the mistaken assumption that the Congo would slip from chaos into communism was the result of the upper-class business and government backgrounds of the decision makers, which clouded their understanding of the fervor of nationalist leaders and the complexities of Congo politics. While he blames both conservatives and liberals, he is particularly caustic on the "Messianic complex" of the liberals, and concludes that "the Congo was only an instance of Kennedy's most glaring failure: his incapacity as a statesman." He considers President Kennedy guilty of an overcautious attempt to balance incompatible policies not only in the Congo but in the Cuban invasion and in Vietnam as well. Too often Kennedy's compromises "were the product of pure caution."

In one brief passage (p. 256) the author acknowledges that the liberals succeeded in helping to end the Katanga secession, thereby making a contribution toward maintaining the territorial integrity of the Congo. Other historians value this achievement more highly than Weissman does.

Viewing his Congo story as a case study of American intervention in the third world, the author asserts that "some fashionable explanations" of this intervention are inadequate (p. 300). Economic interpretations do not explain why the United States helped crush the Katanga secession "over the protests of some interested business men with good political connections." And he questions whether our economic stake in the Congo was big enough to provoke a sizeable politico-military intervention." He concludes that the "socio-historical perspectives" of the decision makers were more relevant than economic expansionism in explaining why the United States intervened.

VERNON MCKAY
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ANATOLII ANDREIEVICH GROMYKO. Through Russian Eyes: President Kennedy's 1036 Days. Washington: International Publishers. 1973. Pp. xviii, 239. \$9.95.

This book, while disappointing as a scholarly work, is extraordinary in many ways. Professor Gromyko's father has been the foreign minister of the Soviet Union since 1957, and he himself is the head of the Foreign Policy Section of the Soviet Academy of Science's America Institute. Moreover, the author is regarded as one of his nation's leading experts on American affairs. Thus, his opinions can be deemed to represent the attitudes of an important segment of the Russian political and academic hierarchy, which makes his myopic view of American society and policy all the more distressing.

Although Gromyko's mathematics are more precise than Arthur Schlesinger's, his assessment of John F. Kennedy's troubled days as president is infinitely less persuasive. The author surveys in a cursory fashion Kennedy's thorny path to the White House. He states that Kennedy "totally suited the monopolists of the northeastern U.S." (p. 49), whose Machiavellian machinations he feels control virtually every facet of American life; and he observes that Kennedy soon learned that "an American political leader must know how to be cunning and to maneuver" (p. 22).

Gromyko characterizes Kennedy as little more than a lackey of more powerful and sinister forces in American society. The author's constantly reiterated thesis is that Kennedy, both as senator and president, was "subservient to the general interests of the American monopolies [and] rendered active assistance to the ruling class in the attainment of its goals domestically as well as abroad" (p. 83).

It is logical that the book focus principally on the foreign policy of the Kennedy administration, for this was truly its cynosure, but the delineation of the issues is a masterpiece of obfuscation. Kennedy's performance in the international arena, particularly vis-à-vis Cuba and Vietnam, is quite a legitimate subject of debate, and many American commentators, such as Richard Walton, Richard Barnett, and Ronald Steele, have recently offered caustic critiques of it, but Gromyko's assessment often sinks virtually to the level of a blatant Soviet propaganda tract. His dogmatism, shibboleths, and simplistic approach to very complex matters unfortunately vitiate what might have been an enlightening commentary by a knowledgeable foreign observer. Furthermore, considering Gromyko's scholarly eminence in his native land, the book reveals some regrettable facts about the nature of contemporary Soviet historiography; and even in the current heady aura of détente, one must, perhaps, remain skeptical of the depth of the Russian commitment to rapprochement—both on the political and intellectual level.

E. BERKELEY TOMPKINS

National Historical Publications Commission

G. POPE ATKINS and LARMAN C. WILSON. The United States and the Trujillo Regime. New Brunswick: Rutgers University Press. 1972. Pp. viii, 245. \$10.00.

ABRAHAM F. LOWENTHAL. The Dominican Intervention. Cambridge, Mass.: Harvard University Press. 1972. Pp. x, 246. \$10.95.

Caribbean specialists recognize dictatorship and revolution as historically endemic area characteristics requiring analysis in the manner of other phenomena. To scholars inured to despotic excesses, the events of the Trujillo regime, the interregnum, and the intervention of 1965 are nonetheless examples of gross human behavior. These two works are important contributions to the flourishing bibliography of American involvement in Dominican affairs.

Although the two studies consider different topics and incorporate different approaches, the impact of Trujillo and the overwhelming presence of the United States in the events described are significant common factors. Both works also transcend their immediate subjects in a search for explanations of past failures and for new understandings that might improve future performance.

The Atkins and Wilson study is the cooperative work of two young political scientists and reflects contemporary methodological thinking in that discipline. The authors believe that their work is the first general survey of United States relations with the Trujillo regime; a more accurate description might be that it is the first dispassionate survey of United States-Dominican relations with particular reference to Rafael Leonidas Trujillo Molina and his legacy. Their findings are plausible and probably valid. The pattern of intervention, nonintervention, and intervention, they argue, demonstrates more than anything else the limitations on the capabilities of the United States to influence the development of Latin America, politically or otherwise.

Many of the ideas that the writers articulate and document will come as no surprise to diplomatic historians. The policies of maintaining political stability and encouraging representative democracy have proven incompatible. The dilemma emerged with the acceptance of the principle of nonintervention in the 1930s.

Underwriting dictators to assure stability has offended the idealism of latinos; the resort to intervention in the name of democracy has impugned their honor. The Trujillo case suggests that the United States should behave more realistically in its relations with caudillos. It should, perhaps, indirectly attempt to modify dictatorships, but it should not support them in the belief that they are more stable than other forms. The authors also maintain that stability in the long run depends on an open society, and with the other Latin states, American policy should encourage democracy but subordinate ideology. It must be understood that failure is a possibility.

Unlike the broad survey approach of the first work, Abraham F. Lowenthal's impressive account focuses on a single event. As a development specialist in the Dominican Republic at the time of the 1965 intervention, he capitalized on the fortuitous circumstance of his mission to probe and assess the extraordinary event he witnessed. His meticulous reconstruction emphasizes the pre-emptive theme in the policy of the United States and sets the stage for a provocative conclusion.

Identifying and refuting three interpretations of the intervention, Lowenthal suggests an alternative explanation, fragments of which some analysts have previously applied to other American diplomatic fracasos. The official, radical, and liberal views are inadequate because they do not account for all the evidence. The real culprit, the author believes, is the "rational policy model." In its stead he posits an inhibiting procedural-organizational-preceptive hypothesis. Human frailties abound in this conception, running the gamut from accidents and indecision through fear, jealousy, and intrigue to reliance on misleading analogies and wishful thinking. Operating within the established conceptual framework, officials on many levels made a series of decisions, the cumulative effect of which led to the intervention.

Both works have valuable comprehensive bibliographies, although Lowenthal's use of interviews and other unpublished sources is more creative. His narrow topic allows a richness of analysis that is particularly satisfying. Both works are nonpolemical, identify policy weaknesses, and include valuable insights regarding the diplomatic process. Both implicitly reject economic motivation, which will upset New Empire publicists. And a common penchant for model building heightens a tract-of-the-times aspect.

But neither contribution is without its lim-

itations. The Atkins-Wilson investigation lacks the penetrating and sustained historical analysis necessary to support its level of generalization. At its weakest it becomes an outline of formal policy development, highlighting the official actions of diplomats, presidents, legislators, admirals, and generals. The authors also seem to have overemphasized the extent to which the policy of the United States has attempted to promote democracy in the Caribbean. Stability, a function of security, has certainly received far more emphasis. Nor should the democratic impulse be related to the Western Hemisphere Idea, which in its pristine form holds that self-determination is the basis for the hemispheric community. Deletion of the first chapter, a heavily laden methodological statement, would be an improvement.

The obvious limitations on the historical treatment of the recent past, the unavailability of significant sources, and the lack of perspective especially apply to Lowenthal's work. A second major problem concerns the author's stress on irrationality, which assumes deterministic proportions. To reject the liberal premise that the statesmen of an enlightened republic can make rational decisions is to reject two centuries of Western wisdom. A simpler explanation of the Dominican blunder is that the intelligence function failed and that the political leadership in the United States, inexperienced in the arena of foreign affairs, overreacted.

Six years after the assassination of an archetypal Dominican dictator, the American special commissioner in his confidential report to the president stated that the deceased's rule had been a despotism. "Brutal cruelty, insatiable greed, moral degeneracy, were the man's personal characteristics, and they shaped his political conduct and his administrative activity. . . . it was the peace of a merciless terrorism, not the quiet of civil government." Jacob H. Hollander's remarks to Theodore Roosevelt on Ulises Heureaux could equally be applied to Rafael Trujillo. The failure that led to the intervention of 1965 is a classic confirmation of the Santayanan nightmare of dooms of repetition for those who do not learn from history.

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JOSEPH M. SIRACUSA. New Left Diplomatic Histories and Historians: The American Revisionists. (National University Publications, Series

Canada 543

in American Studies.) Port Washington, N.Y.: Kennikat Press. 1973. Pp. viii, 138. \$6.95.

This small book has a large purpose: "to analyze recent trends in the writing of American diplomatic history, paying particular attention to the origin, nature and significance of the so-called 'New Left.' . . ." The result is disappointing. Siracusa's discussion of "recent trends" is limited to the works of a dozen or so well-known revisionists, and his "analyses" (frequently relying upon published reviews) are seldom more than superficial observations. At least half of the text consists of summaries, in paraphrase and quotation, of studies by the following historians: Williams, "the master"; LaFeber and McCormick on the nineteenth century; Levin, Mayer, and Parrini on World War I; Gardner and R. F. Smith on the New Deal; and Alperovitz, LaFeber, Kolko, and Gardner on the origins of the cold war. Although Siracusa would have liked to emulate Robert W. Tucker's critical yet perceptive The Radical Left and American Foreign Policy (1971), his obvious aversion for the New Left results in a pedestrian assault more akin to The New Left and the Origins of the Gold War (1973), the literal-minded, virulent attack by Robert James Maddox.

The first chapter of New Left Diplomatic Histories and Historians reveals the quality of analysis throughout. Seeking "origins and antecedents," Siracusa is satisfied with such observations on the New Left as they were "inspired by" their reaction to the cold war climate of opinion; their ideas were provided by Beard and Marx ("the New Left's nonliving heroes"); they have created "an historiography of protest"; they assume a direct connection between America's capitalist political economy and expansionism; they are economic determinists who prefer a nearly self-sufficient democratic socialism. Some of these familiar observations help to explain some aspects of some of the studies Siracusa examines, but they are irrelevant and decidedly unhelpful for explaining others. Merely to observe at this time that the New Left "turned the Realist critique inside out," seeing design where George Kennan et al. saw bungling, reveals nothing new about the relationship between the two dominant analytical models used in the study of American diplomatic history.

Beyond the partisan debate and the rigid defense of a single view engendered by it lies the difficult problem of sorting out and explaining the strengths and advantages—as well as the weaknesses and limitations—that each explanatory paradigm brings to the task. The hot and cold wars of our time raised new questions about American foreign policy that the liberal-realist perspective did not, in fact could not, accept. In history as in science insufficiency rather than error is generally responsible for the passing on, or revising of, theories, and the historiographical evolution we are observing in diplomatic history is a case in point.

Although the New Left's explanatory paradigm has clear limitations, it already has made significant contributions, as R. W. Tucker has shown. Siracusa, however, disagrees. As evidence for his position he summarizes, in his fifth and final chapter, the opinions of "various well-known and respected practitioners in the field of United States diplomatic history." "In sum," he writes, "much of the New Left diplomatic history that emerged in the 1960's lacked intellectual validity." Unhappily, his evaluation of their work is more appropriate for his own.

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### CANADA

JOHN L. FINLAY. Social Credit: The English Origins. Montreal: McGill-Queen's University Press. 1972. Pp. 272. \$8.50.

Social Credit parties presently flourishing in some Canadian provinces have little more than their name to connect them with doctrines espoused by Major C. H. Douglas, founder of the doctrine. This book is a pioneer investigation of what Keynes called the "underworld" of economic reform movements in Britain in the half century before the Second World War, a kaleidoscope of obscure groups ornamented by not a few well-known personages. Finlay finds background for social credit in theories of underconsumption and distributism that were debated at the turn of the century, but he appears to accept Douglas's claim to original inspiration. He also shows considerable sympathy for Douglas's belief that a system of controlled credit developed by payment of a national dividend, coupled with the fixing of "just price" based on a computation of consumption as well as of cost of production, could put an end to poverty and economic slumps and create an age of plenty.

Finlay shows that social credit was strongly

tinged with anarchism and was also curiously subject to dichotomy because it attracted persons from widely different philosophies and schools of thought; but he holds that it was smeared to an unjustifiable extent as fascist and anti-Semitic. He attributes its failure and present sterility to innate weakness, especially to inability to produce a sequential line of thought that could attract and maintain general support. Social credit theory, which aimed at fighting the evils caused by power groups exploiting liberal capitalism, was unable to convince enough people that it offered a better solution than socialism, its arch rival. Partial remedies-for instance, Keynesian programsby alleviating problems that social credit aimed to eradicate, made it possible for interest groups to ridicule Douglas and his theories. Douglas's inadequacies as a leader, his opposition to organization until too late, his arbitrary actions when he reluctantly agreed to move on the political level, and his jealousy of competing associates are described by the author but are discounted.

This book is important not so much as a background for minority party development in Canada as for the fact that when Keynesian programs have been shown to be insufficient, developments like proposals for a guaranteed income, which resemble Douglas's national dividend, have become practical politics. Finlay concludes that we are all now to some extent Social Crediters.

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#### LATIN AMERICA

STUART B. SCHWARTZ. Sovereignty and Society in Colonial Brazil: The High Court of Bahia and Its Judges, 1609-1751. Berkeley and Los Angeles: University of California Press. 1973. Pp. xxvii, 438. \$17.50.

Those who have followed recent developments in Brazilian colonial history will be aware that Dr. Schwartz has struck a rich and rewarding vein with this book, of which we had a preview in his article, "Magistracy and Society in Colonial Brazil" (Hispanic American Historical Review, 50 [1970]: 715-30). This article came as an eye opener to me and probably to most other readers, since it had been widely assumed that the judges and magistrates of the Portuguese colonial world did not attain anything like the importance of those who played such a vital

role in colonial Spanish America. This book confirms in well-documented detail the importance of the magistracy in the society of colonial Bahia and, by inference, in the rest of the Portuguese empire.

Perhaps the judicial and administrative powers of the Relações or High Courts of Goa, Bahia, and Rio de Janeiro did not quite measure up to those exercised by the Audiencias of Mexico, Lima, and Bogotá, but they were certainly great. As Schwartz observed, "the politico-administrative powers of the Relação of Bahia, while great, were to a large extent negative. Many aspects of colonial government lay in the hands of other institutions, but the High Court's ability to check, limit, or delay the policies and actions of virtually everyone else in the Colony placed it at the fulcrum of power. The power to prevent something was as crucial as the power to act, and it gave the Relação enormous leverage in the conduct of government" (p. 359).

Schwartz's task was facilitated to some extent by the fact that all the judges (desembargadores) who served in the Relação had to be law graduates from Coimbra University, and the records concerning their admission, matriculation, and subsequent postings are pretty well complete. This has enabled him to analyze their social origins and follow their careers in enviable detail. He is particularly informative on the interpenetration of these judges with the planter society of Bahia and the resulting network of business and social relationships formed by intermarriage and compadrio (godparentage).

It is hoped that this original and well-researched book, marred only by occasional infelicities of style and awkward turns of expression, will stimulate further research on related subjects, such as the older High Court of Goa (founded in 1544) and on the colonial magistracy in other regions of Brazil.

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D. C. M. PLATT. Latin America and British Trade, 1806–1914. (The Merchant Adventurers.) New York: Barnes and Noble. 1973. Pp. xi, 352. \$12.50.

This work combines attention to entrepreneurial decision making and business history with the broad brush strokes painted by the economic historian. As such, it serves those who seek rich detail on British-Latin American trade

relations and the options that faced the British businessman.

A United States audience should particularly benefit from the references to works available to the author in England. Platt has made copious use of the United Kingdom trade and navigation accounts, British consular reports, and published sources of limited availability. The book is happily dotted with thirty-three statistical tables and six figures drawn from scholarly works, government reports, and reviews of trade. Because the author assiduously avoids analysis or synthesis of data (an exception being figure 5), the materials in the statistical tables can be used with profit by others who wish to compute percentages, distributions, averages, and so forth, that for some purposes sharpen the focus on the statistical facts assembled in the course of research.

Despite the merits of the work, one might have wished for a somewhat more "modern" approach. Capital and labor, migration, and foreign investment are treated much as they would have been treated by George Paish and Michael Mulhall over a half century ago. Yet the literature on investment in human capital, migration, and economic growth is now more than a decade old. Platt properly calls attention to the deficiencies of existing estimates of foreign investment; it also would have been useful for him to have explored the balance of payments implications of profit and capital remittances associated with those investments.

More basic than these conceptual problems is the absence of an analytical scheme tying the book together. Platt introduces such a scheme in his final chapter but then only in a negative form; he argues that Latin America never was the open arena of world trade in which British skill in exporting was tested against German and American rivals. Anyway, British businessmen had more lucrative markets within the Empire; hence the decline in Britain's share of Latin American trade need not be viewed as evidence of British failure to keep up with the competition. But surely the Latin American experience does not support the alternative hypothesis, namely, that all was well with British manufacturing in the late nineteenth century.

Is it fair to ask an author to write a book different from the one he has written? Probably not. The value of this work lies in its meticulous scholarship and the author's decision to remain close to the facts.

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JOHN LYNCH. The Spanish American Revolutions, 1808–1826. (Revolutions in the Modern World.) New York: W. W. Norton and Company. 1973. Pp. xxvii, 433. \$15.00.

A unified account of the Spanish American independence movement has long been needed, though there is no lack of histories of the revolutions in individual countries, many of them in the familiar "patriotic" genre. In this volume Professor Lynch, whose qualifications are widely recognized, fills a gap.

He presents the overall movement as converging sweeps of a vast pincers. The southern arm swung west and north from the River Plate, the northern west and south from Venezuela. The two joined in Peru 175 years ago for the final victory. He regrets that since those glorious days, history has seen the anachronistic colonial societies survive intact, complete with rigid social stratification, economic underdevelopment, the pernicious hacienda system, caudillos, military governments, and granite ruling classes as obdurate against social change as the last Bourbon monarch Fernando VII. Lynch seems to hope that renewed revolutionizing of social structure and economic organization will one day bring Spanish America the modern and just societies for which most of the independence generation fought.

No differentiation in political philosophies is drawn between the southern and northern arms of the revolutionary pincers, though some of the data presented might justify this. The power flow from Buenos Aires never lost its pro-oligarchy character—witness San Martín's hope of establishing a monarchy in Peru and Argentine collusion with Peru in subverting Sucre's populist, and therefore threatening, regime in Bolivia. In contrast, the force emanating from Venezuela was populist in intent and rhetoric and, where possible, in action. Santander's administration in Bogotá is, like Sucre's in Chuquisaca, an example.

The thesis that economic factors were all-important stimuli to revolt is, to me, exaggerated. Of more critical nature were regional and personal pride, demand for social mobility, and burning resentment against discrimination and exploitation. Surely men will fight more fiercely in raging fury against social, economic, and political injustice than in defense of such prosaic things as full stomachs and a desire to preserve material possessions. If so, twin revolutionary principles may be posited: social injustice, where it really exists, will sooner or later be eliminated, and the popular struggle

against it, possibly protracted over several generations, will end only when the injustice is ultimately removed. This suggests that genuine social revolutions are, in the long run, bound to win. Policy makers should take note.

In a work as broad in scope as this, specialists are likely to find minor faults. The balanced chapters on the breakup of the Viceroyalty of the River Plate (now Argentina, Uruguay, Paraguay, and Bolivia), Chile, Colombia, and Mexico are, in my opinion, more complete and accurate than those on Venezuela, Ecuador, and the later period in Peru. In Venezuela, for example, Santiago Mariño is all but ignored, but this more or less equal rival of Bolívar from 1813 on, whose political ideas eventually won out, deserves more than brief dismissal as a "minor caudillo." Fuller treatment might have been given to Sucre, who liberated Ecuador, prevented the disintegration of Peru, and won deserved praise for his progressive administration in Bolivia. There is virtually nothing on the struggles in Central America, and, considering the transcendental issues involved, the destruction of Bolívar's Colombia might have been dealt with more extensively.

The unfortunate limitation of this study to the Spanish American revolutions leaves out the dramatic and terror-inspiring black upheaval in French Haiti, the minuet-like ousting of Portugal from Brazil, to say nothing of the pattern-setting events in the English thirteen colonies. The two continent-wide independence story could be told to advantage in a single volume.

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RONNIE C. TYLER. Santiago Vidaurri and the Southern Confederacy. [Austin:] Texas State Historical Association. 1973. Pp. 196. \$8.00.

Santiago Vidaurri, a "regional caudillo" (p. 61) of northeastern Mexico, made himself governor of Neuvo León in 1855 during the Liberal uprising that eventually brought Benito Juárez to the presidency of Mexico. By shrewd politico-military maneuvers Vidaurri expanded his power in the adjacent states until he controlled a semifeudal, subnational principality from his palace at Monterrey.

When the American Civil War began, Vidaurri and the Confederates drew together in an informal entente, Vidaurri hoping for support against the centralizing aims of Juárez and the Confederates desperately needing a bypass around the Union blockade. During 1862 and

1863 Confederates and Mexicans, led by Vidaurri's Irish-born son-in-law, the merchant Patricio Milmo, maintained a flourishing trade in cotton, munitions, and many other necessities and luxuries through Matamoros. This trade was carried on for a time in Confederate ships flying the Mexican or the British flag. the latter with the compliance of the local British consul. At the end of 1863 this mercantile idyll ended with the Union capture of Brownsville, Texas (opposite Matamoros), and in the following year Juárez, raiding into northeastern Mexico, chased Vidaurri into temporary exile. He soon returned to become one of Maximilian's Mexican coterie, but his palmy days of local autonomy were over, and when Maximilian was overthrown, Juárez had Vidaurri shot with the rest. Tyler concludes that the Monterrey caudillo was "something of an anachronism" (p. 156) in a Mexico that, under Juárez's heroic though erratic leadership, was struggling toward a national consciousness in which Viduarri had no place.

The author has not attempted a full-scale biography of Vidaurri but has concentrated on what is, for Americans at least, the most significant chapter in his life. Tyler's research is thorough, balanced between American and Mexican sources; his style is direct, clear, and pithy; and he has not tried to inflate an episode into an Iliad. The result is a neatly organized little monograph, useful to students of Mexican history and of the American Civil War. It is handsomely illustrated with contemporary portraits and local scenes and includes a good map of northern Mexico and the border area. The publishers are also to be complimented for putting footnotes where they belong.

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DAVID RONFELDT. Atencingo: The Politics of Agrarian Struggle in a Mexican Ejido. Stanford: Stanford University Press. 1973. Pp. viii, 283. \$10.00.

This study traces the long struggle between a privately owned sugar mill and a varying but sizable number of campesinos for the control of land, labor, and production in the Atencingo ejido and its annexes, which constitute one of Mexico's more important and larger collective agricultural enterprises. Many authorities claim that the constant troubles experienced in this ejido are atypical of Mexican rural conditions and politics; others "claim

that Atencingo's history of struggle, though extreme, reveals much about agrarian political processes in Mexico" (p. 216).

Located in the state of Puebla, the Atencingo ejido evolved from privately owned sugar haciendas that William Jenkins, a former United States consul, acquired and consolidated after the Revolution. Gradually, and not without delaying action, Jenkins was forced to relinquish control of his estates to satisfy landhungry peasants and demands issuing from agrarian reformers entrenched in the government. From the late 1930s, when the ejido was finally created, disputes developed that centered around the desire of many ejidatarios to be economically independent of the mill and to divide the one giant tract into separate entities, parcel the land to individual farmers, and diversify agriculture to include not only sugar but also subsistence and other cash crops.

Ronfeldt organizes his study chronologically to trace the evolution of the issues that lay at the core of the struggle; to show how the various contending forces, from grass-roots peasant organizations all the way up to the presidency, came into play; and to analyze the measures used-work stoppages, protest marches, propaganda campaigns, assassinations—to forward the cause of one or the other factions. A final summary chapter sets the politics of the agrarian struggle in Atencingo in the broader context of agrarian conflict as an abstract phenomenon. To date, reform has been postponed consistently in favor of economic stability and high levels of production through government cooptation and control.

The study demonstrates how the agrarian reform thrust of the Revolution of 1910 has taken a long time in some areas of Mexico in fulfilling promises made long ago. Let him who views the Revolution as a solution to Mexico's agrarian problems read this book carefully and ponder the story of Atencingo.

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ROBERT JONES SHAFER. Mexican Business Organizations: History and Analysis. [Syracuse:] Syracuse University Press. 1973. Pp. xi, 397. \$15.00.

ROBERT W. RANDALL. Real del Monte: A British Mining Venture in Mexico. (Latin American Monographs, number 26. Institute of Latin American Studies, the University of Texas at Austin.) Austin: University of Texas Press, for the Institute. 1972. Pp. xvi, 257. \$8.50.

Within the past few years the writing of the history of Mexico's economic development has assumed new forms. No longer are we only following the ghost of Zapata galloping over the hills of Morelos in search of Professor Frank Tannenbaum's bucolic paradise. That vision of history was interred by Germán Parras and James Wilkie's analyses of the development of industry and Mexico's real socioeconomic policy. The abominable conservatives have begun to emerge as the true fathers of modern industrialized Mexico where, in parody of the old Russian slogan, electricity plus revolution power equals Pan Bimbo. The persistence and importance of capitalism in Mexico's economic evolution is slowly being recognized. Luis González's Pueblo en vilo (1968) may well be a monument to a now-passing phase of interest in backward peasant communities. While the broad picture of the economy's growth has been sketched for us by Leopoldo Solis and Clark Reynolds, Fernando Rosenzweig and David Brading, Alejandra and Enrique Florescano, Barbara and Stanley Stein, Pablo González Casanova and Roger Hansen, and Moisés González Navarro and Frank Brandenburg, we are also beginning to see the appearance of economic histories on another level. Along with Flavia Derossi's The Mexican Entrepreneur (Paris, 1971), Robert Shafer's Mexican Business Organizations gives us in the manner of the social sciences what Carlos Fuentes has been writing in novelistic style, and the milieu of Mexican private enterprise is now coming into better focus.

Having explored an operational muddle in his Mexico: Mutual Adjustment Planning (1966), Shafer has now essayed a study of Mexican businessmen's organizations to judge the power and influence of private enterprise in the intricacies of Mexican politics. The propaganda efforts of their various chambers have been unsuccessful in making "business" acceptable in a society endlessly mouthing revolutionary slogans. But since Mexicans are profit-oriented, business still offers a much sought-after career. The study is exhaustive and exhausting: the footnotes, in miniscule type, are as voluminous as the text, but they cannot be overlooked for they are studded with nuggets of information and sharp insights. The author's textual commentary is undisguised; he shows a basic sympathy for the Revolution and the masses while evenhandedly praising and blaming all parties concerned. Shafer has meticulously perused volumes of newspapers. More important, he has opened a

new archival source by his use of the publications of businessmen's organizations, and his notes provide an introductory guide to these materials. Furthermore, Shafer has interviewed several hundred businessmen. While he has not included summaries of these conversations as Derossi has, they are constantly referred to and should constitute a source of oral history wherever they are deposited. The strongest part of the book deals with these organizations' doctrines and programs and with their "hard history": the organization of the commercial and industrial chambers and their related civil associations and their changing legal status since 1874. Originally founded to aid merchants and industrialists, chambers were encouraged by post-Revolutionary governments in order to provide a locus of communication with the business community. Membership in a chamber was made obligatory in 1936. Equally valuable is the exploration of the alphabet soup of organizations-CONCANACO, CONCAMIN, COPARMEX, CNIT, and so forth-tracing their labors, relationships, squabbles, and policies. Most enlightening is the discussion of the role of the Confederación Nacional de Industrias de Transformación as the government's claque and apologist by virtue of having its members assigned to it and forced to pay dues by official fiat, thus doing away with the need to consult the membership. For reasons obvious to anyone researching recent Mexican history and battling the conspiracy of silence and ambiguity, the sections dealing with the role of the chambers in influencing government policy and the extent to which they are consulted are less satisfactory, for much is gathered by implication. Shafer concludes that the initiative for forming economic policy is held by the president, the bureaucrats, and the official party. However, businessmen are consulted more often than ever before and their complaints are listened to and heeded by contemporary administrations. Despite a high level of government "collaboration," business organizations have managed to keep their independent decision-making power while constantly increasing their influence in the public sector as spekesmen for the private sector.

To the few works we have on the economy and industry of early republican Mexico, notably Robert Potash's Banco de Avio (1959) and Romeo Flores Caballero's La contrarevolución (1969), we can now add Robert Randall's study of British attempts to exploit the Real del Monte silver mines. Randall found a completely neglected source: a room full of com-

pany records at the mine office. He filled out his research there with visits to general archives, private collections, and the perusal of printed documents, journals, and contemporary and modern studies. The result bears comparison with the works of Clement Motten and Modesto Bargalló. While the general story of the magnificent failure of this enterprise is well known in outline, Randall has been able to put it forth in full detail with voluminous documentation. We are now able to follow the plunge of British investors into the fairyland of Mexican mines with Alexander von Humboldt acting as unwitting Pied Piper through the translation of his Essai Politique on New Spain. Randall recounts the financial negotiations for the British lease, the character of the managers and the Cornishmen sent over to teach the Mexicans how to mine (!), the vicissitudes met in transporting the very latest power machinery, whether suitable for Mexican conditions or not (shades of AID programs), mine finances, financial struggles in London, fights over working methods, attempts to bail the company out by investing in other Mexican mines, quarrels with the Mexican government over the company's exemption from mintage obligations, and a most interesting chapter on labor disputes in the 1820s and 1840s. While the center of attention is the mine, the reader learns much about the Mexican economy, business operations, government, and social customs. Despite the production of \$11,000,000 in silver, the enterprise lost \$5,000,000 between 1824 and 1849, and the value of its stock fell from £1,479 a share to 63 pence. In the end the mines were sold to a Mexican syndicate for a song. The English had taken over and rebuilt a ruined enterprise on the most modern principles and then were unable to see their plans through because of a lack of capital when public confidence collapsed. The new Mexican owners, freed of the burden of the British investment, were able to complete the work and bring the mine into a bonanza, leaving the intriguing question of who exploited whom. It is indeed unfortunate that this book was in process when David Brading's Miners and Merchants appeared, for not only would Brading's material have added to the discussions of the colonial period, but it would have provided a base for a discussion of the decisive role of tenacious capitalists with abundant capital in making Mexican mining enterprises successful.

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Buffalo

## Communications

A communication will be considered only if it relates to an article or review published in this journal; publication of such a communication or of any part of it is solely at the editors' discretion. Limitations of space require that a communication, whether concerning articles, review articles, or reviews, be no longer than 300 words. The schedule of publication and the time needed to send a communication to the author of the article or review in question for such reply as he may care to make virtually preclude the possibility of publication in the issue following that in which the original article or review appeared. Unless, in the editors' judgment, some major scholarly purpose is served, rejoinders will not be published.

#### TO THE EDITOR:

Scholarship usually implies some objectivity, not the hatchet job that Gerda Lerner did on The Subordinate Sex: A History of Attitudes toward Women (AHR, 79 [1974]: 1138-39). Though it is probably useless to take a reviewer to task, I do so in the hope that some might read the book and judge for themselves.

The book was intended to be a study of attitudes toward women, not a comprehensive history of women, and this is clearly stated. Since most of that which has survived on the subject of women has been written by men, it is essentially a study of male attitudes toward the female, which on the whole is a rather depressing topic. Professor Lerner complains that women's history is not just the history of male attitudes toward women, and I heartily agree, but in my opinion it is one aspect of history, and an important foundation for any more comprehensive study of women. Most of her remarks thereafter are directed at a book I did not write, and while I agree with some of her digressive remarks and disagree with others, they are not particularly pertinent to the book.

She complains about the conceptual frame-

work, but what she really means is that I am. not pushing any kind of theory but reporting the rather consistent and depressing theme of the male tendency to look down on the female as subordinate. This was the stated purpose of the book. My greatest objection, however, is to her statement, "Given the absence of primary sources," which is the ultimate putdown that one historian can make about another. The book is a result of many years' study of the primary sources, and these are cited throughout the book in footnotes at the bottom of the page. True, the bibliography did not list them, but it did refer the interested reader to the notes. The "Guide for Further Reading" was what it implied, material in my judgment that the interested general reader might find helpful. It also referred readers to several recently published bibliographies on women's history, some of which now are several hundred pages long. It also referred readers to other books with complete bibliographies on specialized topics such as an earlier book by my wife and me on nursing.

In short the book is not an all-encompassing study of women in history but is a more specialized study based upon primary sources of male attitudes toward the female, toward female sexuality, and toward women as wife and mother, with occasional replies by women themselves. The readers will have to judge for themselves whether or not the book is helpful, valuable, or trash. This, however, was the book that I wrote, not the one reviewed by Professor Lerner.

vern L. Bullough Galifornia State University, Northridge

#### PROFESSOR LERNER REPLIES:

I am quite aware of the fact that Professor Vern L. Bullough's book is "a study of male attitudes towards women." In fact I recommended "... its usefulness. As a survey of the way in which, men have regarded women ...." The author may, of course, object to the reviewer's judgment, although wisdom and discretion would dictate that such objection be couched in more judicious language than calling what was a qualified endorsement of the book a "hatchet job." Further, I consider it not only the reviewer's prerogative, but his or her duty, to discuss the limitations of the scope of a work and to discourse on the implications of its conceptual framework.

Unfortunately Professor Bullough's disclaimer regarding my objection to the absence of primary sources is unimpressive in face of the evidence. The book has neither scholarly bibliography nor adequate footnotes. There is no listing of primary sources, which would aid the

scholar and interested student. Professor Bullough's footnotes, despite his claim that the reader go to them for primary sources, reveal merely that most of his work is based on secondary sources and that there is an almost complete absence of reference to manuscript and unpublished primary sources. Further, the bibliographies to which Professor Bullough refers his interested reader in "The Guide to Further Reading" are, with a few exceptions, unscholarly and addressed not to historians and the student of history, but to the general public. There is something to be said for compiling that kind of guide to sources in a book for popular consumption, but it certainly does not entitle the author to praise for his historical scholarship.

GERDA LERNER
Sarah Lawrence College

# Recent Deaths

JOHN T. APPLEBY, associate editor of the American Historical Review, died at his home in Washington, D.C. on December 19, 1974. He was born on June 10, 1907, in Fayetteville, Arkansas and received his A.B. from Harvard College in 1928. He then went to Paris, where he studied at the Sorbonne and worked as a reporter for the Paris Times. On returning to the United States, he settled in Washington, D.C., writing a column, "Post Impressions," and book reviews for the Washington Post. When the war came, he enlisted and served as a trainer in celestial navigation for the Eighth Air Force, visiting England for the first time in 1945. After the war he returned to Fayetteville, where he combined writing with operating an apple orchard. The writing proved more successful, and in 1953 he once more settled in Washington to devote himself full time to scholarship. In 1959 he became membership secretary at the American Historical Association. While he continued to maintain the register of Ph.D. dissertations for the AHA, he soon moved primarily to the AHR, which he served until his death.

His scholarly interest lay in England in the years between the death of Henry I and the death of John. In 1963 he published an edition of The Chronicle of Devizes of the Times of King Richard the First, and he published four narrative histories of the period-John, King of England (1959); Henry II, the Vanquished King (1962); England without Richard, 1.189-1199 (1965); and The Troubled Reign of King Stephen (1969)—which display his elegant prose and his concern to interest an audience of scholars and laymen alike. But his first book, Suffolk Summer, published in 1948, may be the most remarkable. Growing out of his visit to England at the end of the war, it says much about the topography and spirit of that lovely county, about England in the hopeful days of 1945, and—though he was a very private person—about the author himself.

It seems strange, indeed unjust, that so able a scholar should have spent his most productive years in a post that, at first sight, amounted to a superior clerkship, and a clerkship that for most of the time paid a mere pittance. In addition to the dissertation register, he compiled the index to the AHR and superintended the entire, complex operation of AHR book reviewing from the receipt of books to the receipt of reviews. His record-keeping systems were sophisticated, ingenious, and as good as infallible; his working of them was a marvel of efficiency and meticulousness. The post gave him, however, time for his writing and allowed him to pursue his profound love of music, the one indulgence in a life that was simple and even austere.

But, as his staff colleagues and the four editors he served and guided can abundantly testify, he was no mere clerk. Although he steadfastly maintained that his world ended in 1215, he had an immense knowledge of all fields of history and brought to his tasks a knowledge of the historical profession in the United States and England that was probably unrivaled anywhere. He was in a better position than most to observe the foibles and failings of historians, and illiteracy, pretentiousness, or delinquency could call forth scathing comments in the privacy of his office; yet he was unfailing in his tact and helpfulness to everyone, quick to appreciate merit and style, and committed in the most serious way to the historical enterprise. Everyone on the staff—and others who got to know him personally-respected, were even awed by, the combination of precision, intellectual grasp, and high wit that was the hallmark of his distinctive regime at the AHR, as they appreciated his kindness, wiscom, and cheerfulness (none of us knew how painfully he was suffering from leukemia) and watched with admiration his heroic struggles with—and invariable besting of—the computer.

Given his odd and virtually unknown career, his quiet colorfulness, his devotion to his dogs, his mock rages against failings, scholarly or political, one might be pardoned for thinking that Jack Appleby was an eccentric. But the balance was too fine for that. A devout and liberal Roman Catholic, a scholar in the most humane interpretation of the word, he was a gentle man and a gentleman in a sense that has almost vanished.

R. K. WEBB
American Historical Review

GILBERT OSOFSKY, professor of history at the University of Illinois at Chicago Circle for the past eleven years, died on August 26, 1974, in Rockville, Maryland, after a long illness. He was born on March 14, 1935, in the Williamsburg section of Brooklyn, New York. He was educated in New York City at Brooklyn College (B.A. 1956), New York University (M.A. 1958), and Columbia University (Ph.D. 1963). Professor Osofsky's first academic appointment was at Hunter College from 1961 to 1963, when he came to Chicago to join the University of Illinois for the remainder of his tragically brief career.

Gilbert Osofsky's first book, Harlem: The Making of a Ghetto, 1890-1930 (1966), established his reputation as a leader in a new generation of urban and racial historians in America. Excerpted extensively and widely quoted, it has served as a model for those seeking to understand black experience in the city. His second book, The Burden of Race: A Documentary History of Negro-White Relations in America (1967), traced American race relations from the colonial period to the black-power movement of the 1960s through a fascinating array of documents carefully woven together by a narrative thread. His third book, Puttin' on Ole Massa: The Slave Narratives of Henry Bibb, William W. Brown, and Solomon Northrup (1969), made available to a wide audience three slave narratives of the 1840s and 1850s, introduced by an essay that placed them in a wider perspective. His numerous contributions to the journal literature are widely known. Some have yet to appear.

Gilbert Osofsky's untimely death at the age of thirty-nine came when he was in the midst of writing a book on the ideology of the abolitionists. This topic, which he had explored inten-

sively, was leading him in turn to develop an original approach to the meaning of nationalism and its relationship to ethnicity, which would have shaped his future scholarship.

His contribution to building our department and the Chicago Circle campus of the University of Illinois was invaluable. His teaching and personal impact inspired both students and colleagues, establishing permanent bonds of affection and emulation. He was a man of deep compassion and empathy, and his social commitment embraced his entire professional life and also made him an activist for social justice beyond the walls of the university.

We were privileged to know and work with Gil, and he will be sorely missed in the lives of those he touched.

RONALD P. LEGON
University of Illinois,
Chicago Circle

MARY LATIMER GAMBRELL, president emeritus of Hunter College of the City University of New York and professor emeritus of its history department, died in New York on August 19, 1974. She achieved the high distinction of being the first woman to assume the presidency of a coeducational college of more than 25,000 students. Her conception of the purpose of the college and of the pre-eminent responsibility of the faculty to decide academic policies was reflected in college administration during the three decades of her leadership. She served as chairman of the history department (1948-61), dean of faculties (1962-65), and as acting president and president from 1966 to July 1967 when she retired.

Miss Gambrell, for so she was known to most of us, was born in 1898 at Belton, South Carolina, to a distinguished Southern family. She received her A.B. degree from Furman University and her doctorate in history from Columbia University. As a result of her work in Dixon Ryan Fox's seminar, she published in 1937 Ministerial Training in Eighteenth-Century New England. After she joined the department of history in 1937, her overriding concern was to broaden and deepen the quality of education offered by the college and to secure for the faculty a determining voice in all academic matters.

Her ideal for the faculty, reflected in her own career, was the practice of academic statesmanship. To her the faculty was "the very embodiment of the college—its heartbeat." Theirs was the responsibility to set the goals and implement them if their professional aspirations were to be

fulfilled. Contributing to her own positive leadership were qualities of character that inspired confidence: an unshakeable integrity, loyalty to colleagues and the college, an adherence to established channels and open procedures so as to avoid even the semblance of arbitrary action, and a willingness to "shake the pillars of authority" if an important issue was at stake.

Hers was a commanding presence. Innate dignity and a sense of authority and purpose-fulness evoked immediate recognition of her high office. She set exacting standards for herself, and in their attainment she brought distinction to the college.

NAOMI C. MILLER
Hunter College,
City University of New York

IRVIN GORDON WYLLIE died at the age of fifty-four in Kenosha. Wisconsin, on October 25, 1974. His academic training was at Westminster College (Pennsylvania), Oberlin, and Wisconsin, where he received the Ph.D. in 1949. A gifted interpreter of American history, he taught at the University of Maryland, the University of Missouri, where he was president of the local chapter of the AAUP, and at Wisconsin, where he was the first Gordon E. Fox Professor of American Institutions and chairman of the history department. Wyllie was also a Ford Fellow at Cornell and a Fulbright lecturer at Gothenburg and Lund.

Wyllie's contributions to American social and cultural history included studies in the cult of success (The Self-Made Man in America: The Myth of Rags to Riches [1954]), a concise but well-documented and brilliantly conceptualized monograph; essays on the legal and ideological aspects of American philanthropy; and the relationships, or lack of them, between Social Darwinism and business thought. His interdisciplinary interests were reflected in an illuminating study of what he saw as a subculture in

southeast Missouri. Professor Wyllie set high standards for and was a stimulating teacher of graduate students. This is borne out, among other ways, by the tributes they have paid to him in their publications in the history of science, architecture, social welfare, foundations, and related subjects.

In 1966 Wyllie was named chancellor of what was to become the University of Wisconsin-Parkside. When he began, it was without a name, buildings, staff, and educational plan. With remarkable drive and persistence he was responsible for what is the most impressive academic architecture in Wisconsin. He was also the leading figure in the development of a vigorous institution with a sound, and in some ways, innovative program, especially in its relationship to the interests and needs of the Kenosha-Racine area. Within six years of its legal establishment Wisconsin-Parkside received the unconditional accreditation of the North Central Association. Chancellor Wyllie's interest in high academic standards was also evident in the work he did on the North Central's consultation and evaluation teams at several colleges and universities.

Irvin Wyllie will be remembered not only for his teaching, scholarship, and university building, but also as a generous colleague and as the warm friend of historians throughout the country. The University of Wisconsin—Parkside has initiated a fund in his memory.

MERLE CURTI University of Wisconsin, Madison

Other members of the association who have died recently include: Howard L. Briggs, professor emeritus of Frostburg State College in Frostburg, Maryland; Ralph Carey of Spring Arbor, Michigan; H. M. Eikenbary of Dayton, Ohio; Robert A. Kress of Mankato State College in Mankato, Minnesota; Elizabeth Rogers of New York City; and S. J. Sluszka of Babylon, New York.

# Festschriften and Miscellanies

These volumes, recently received in the AHR office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other Festschriften and similar volumes that are amenable to reviewing will be found in the review section.

BIDDLE, MARTIN, et al. Anglo-Saxon England. Volume 2. Edited by PETER CLEMOES. New York: Cambridge University Press. 1973. Pp. x, 333. \$19.50.

JOHN MCN. DODGSON, Place-names from hām, distinguished from hamm names, in relation to the settlement of Kent, Surrey and Sussex. KENNETH HAR-RISON, The beginning of the year in England, c.500-900. GERALD BONNER, Bede and medieval civilization. MARY ANNE O'DONOVAN, An interim list of episcopal dates for the province of Canterbury 850-950: part II. CYRIL HART, Athelstan "Half King" and his family. MICHAEL DOLLEY, Some Irish evidence of the Crux coins of Æthelred II. RUTH MELLINKOFF, The round, cap-shaped hats depicted on Jews in BM Cotton Claudius B.iv. ELIZABETH OKASHA, A rediscovered medieval inscribed ring. P. M. KORHAMMER, The origin of the Bosworth Psalter. D. G. SCRAGG, The compilation of the Vercelli Book. J. E. cross, Portents and events at Christ's birth: comments on Vercelli V and VI and the Old English Martyrology. M. R. GODDEN, An Old English penitential motif. MICHAEL D. CHERNISS, The cross as Christ's weapon: the influence of heroic literary tradition on The Dream of the Rood. P. J. FRANKIS, The thematic significance of enta geweorc and related imagery in The Wanderer. PHILIP B. ROLLINSON, The influence of Christian doctrine and exegesis on Old English poetry: an estimate of the current state of scholarship. Allegorical, typological or neither? Three short papers on the allegorical approach to Beowulf and a discussion. Bibliography for 1972.

Camden Miscellany Vol. XXV. (Camden Fourth Series, Volume 13.) London: Royal Historical Society. 1974. Pp. 278. £3.00.

BARRETT L. BEER and SYBIL M. JACK, editors, The letters of William, Lord Paget of Beaudesert, 1547-63. P. D. G. THOMAS, editor, The Parliamentary Diary of John Clementson, 1770-1802. JOSEPH BARCLAY PENTLAND and J. VALERIE FIFER, editor, Report on Bolivia, 1827.

CLEMOES, PETER, editor. Anglo-Saxon England 3. New York: Cambridge University Press. 1974. Pp. x, 270. \$22.50.

R. DEROLEZ, Cross-Channel language ties. NIGEL F. BARLEY, Old English colour classification: where do matters stand? MICHAEL HUNTER, Germanic and Roman antiquity and the sense of the past in Anglo-Saxon England. VIRGINIA DAY, The influence of the catechetical narratio on Old English and some other medieval literature. KATHRYN HUME, The concept of the hall in Old English poetry. JOHN C. POPE, Second thoughts on the interpretation of The Seafarer. COLIN CHASE, God's presence through grace as the theme of Cynewulf's Christ II and the relationship of this theme to Christ I and Christ III. BRUCE HAR-BERT, King Alfred's æstel. RAYMOND J. S. GRANT, Laurence Nowell's transcript of BM Cotton Otho B. xi. MECHTHILD GRETSCH, Æthelwold's translation of the Regula Sancti Benedicti and its Latin exemplar. EARL R. ANDERSON, Social idealism in Ælfric's Colloquy. H. M. TAYLOR, The architectural interest of Æthelwulf's De Abbatibus. MICHAEL DOLLEY, Towards a revision of the internal chronology of the coinages of Edward the Elder and Plegmund. CHRIS-TINE FELL, The Icelandic saga of Edward the Confessor: its version of the Anglo-Saxon emigration to Byzantium. H. R. LOYN, Kinship in Anglo-Saxon England. NICHOLAS BROOKS, Anglo-Saxon charters: the work of the last twenty years. MARTIN BIDDLE, ALAN BROWN, T. J. BROWN, PETER A. CLAYTON, and PETER HUNTER BLAIR, Bibliography for 1973.

Doklady Kongressa: Izdany Natsional'nym Komitetom istorikov SSSR pri finansovoi podderzhke Akademii Nauk SSSR i Iunesko [Reports of the Congress: Published by the National Committee of Historians of the USSR with Financial Support from the Academy of Sciences and UNESCO]. Volume I, part 7. (XIII Mezhdunarodnyi Kongress istoricheskikh Nauk, Moskva, 16–23 avgusta 1970 goda.) Moscow: Mezhdunarodnyi Komitet istoricheskikh Nauk. 1974. Pp. 340.

Mezhdunarodnaia komissiia po istorii sotsial'nykh dvizhenii i sotsial'nykh struktur [International Commission on the History of Social Movements and Social Structures]. Enquête sur les mouvements paysans dans de monde contemporain (de la fin du XVIIIº siècle à nos jours). Rapport général: ALBERT SOBOUL, Mouvements paysans contre le feodalite (fin XVIII -début XIX siècle). PHILIPPE vigier, Mouvements paysans dans le cadre de l'agriculture et de la societe rurale traditionelles. PIERRE BARRAL and YVES TAVERNIER, Mouvements paysans visant a adapter l'agriculture a l'economie de marche. JACQUES DROZ, Mouvements paysans et mouvements nationaux (de la fin du XVIIIe siècle à nos jours). PIERRE VILAR, Mouvements paysans en Amerique Latine. En guise de conclusion: Reflexions sur les mouvements paysans et les revolutions au XXe siècle.

Mezhdunarodnyi komitet po istorii vtoroi mirovoi voiny [International Committee on the History of the Second World War]. Trudiatsissia massy v period vtoroi mirovoi voiny [The Working Masses in the Period of the Second World War]: P. A. ZHILIN, Usiliia narodov SSSR v dostizhenie pobedy vo vtoroi mirovoi voine [The Efforts of the Peoples of the USSR to Achieve Victory in the Second World War]. A. BLUM, Soldier or Worker: an American Manpower Dilemma during the Second World War. M. GOWING, The Organisation of Manpower in Great Britain during the Second World War. D. PETZINA, Die nationalsozialistische Mobilisierung deutscher Arbeitskräfte vor und während des Zweiten Welt-

krieges. K. DROBISH and D. EICHHOLTZ, Die zwangsarbeit ausländischer Arbeitskräfte in Deutschland während des Zweiten Welkrieges.

Zakliuchetel'noe zasedanie kongressa [Concluding Session of the Congress]: Discours du professeur Paul Harsin, Président du Comité International des Sciences Historiques. Discours du professeur Michel François, Secretaire général du Ccmité International des Sciences Historiques. Vstuplenie predsedatelia Sovetskogo orgkomiteta kongressa akademika A. A. Gubera [Address of the President of the Soviet Organizing Committee of the Congress, Academician A. A. Guber]. v. N. LAZAREV, Iskusstvo srednevokoi Rusi i Zapad (XI–XV vv.) [The Art of Medieval Rus and the West (XI–XV centuries)].

SERJEANT, R. B. and BIDWELL, R. L., editors. Arabian Studies I. Totowa, N.J.: Rowman and Littlefield, for the Middle East Centre, University of Cambridge. 1974. Pp. viii, 182. \$20.00.

A. F. L. BEESTON, New Light on the Himyeritic Calendar. T. M. JOHNSTONE, Folklore and Folk Literature in Oman and Socotra. R. B. SERJEANT, The Cultivation of Cereals in Mediaeval Yemen (A Translation of the Bughyat al-Fallahin of the Rasulid Sultan, al-Malik al-Afdal al-'Abbās b. 'Alī, composed circa 1370 A.D.). J. c. wilkinson, Bayāsirah and Bayadir. G. R. TIBBETTS, Arabia in the Fifteenth-Century Navigational Texts. PETER BOXHALL, The Diary of a Mocha Coffee Agent. c. R. SMITH, The Yemenite Settlement of Tha'Bat: Historical, Numismatic and Epigraphic Notes. J. H. STEVENS, Man and Environment in Eastern Saudi Arabia. ABDULLAH 'ANKAWI, The Pilgrimage to Mecca in Mamlük Times. MICHAEL L. BATES, Unpublished Wajīhid and Büyid Coins from 'Uman in the American Numismatic Society. BRIAN DOE, Ancient Capitals from Aden.

# Other Books Received

Books listed were received by the AHR between October 1 and December 1, 1974. Books that will be reviewed are not listed, but listing does not preclude subsequent review.

#### GENERAL

BAUMGART, WINFRIED. Vom europäischen Konzert zum Völkerbund: Friedensschlüsse und Friedenssicherung von Wien bis Versailles. Erträge der Forschung, no. 25. Darmstadt: Wissenschaftliche

Buchgesellschaft. 1974. Pp. x, 181.
BETHELL, NICHOLAS. The Last Secret: The Delivery to Stalin of Over Two Million Russians by Britain and the United States. Introd. by HUGH TREVOR-

and the United States. Introd. by HUCH TREVOR-ROPER. New York: Basic Books. 1974. Pp. xiv, 224. \$8.95.

CASSELS, ALAN. Fascism. New York: Thomas Y. Crowell. 1975. Pp. xiv, 401.

CHURCHILL, WENSTON, et al. If It Had Happened Otherwise. Ed. by J. c. squire. Introd. by SIR JOHN WHEELER-BENNETT. New York: St. Martin's Press. 1974. Pp. xiii, 320. \$8.95.

CLUTTERBUCK, RICHARD. Protest and the Urban Guerrilla. New York: Abelard-Schuman. 1974. Pp. x, 800. \$10.00.

COLLIER, RICHARD. The Plague of the Spanish Lady: The Influenza Pandemic of 1918-1919. New York:

Atheneum. 1974. Pp. 376. \$10.00. COLSON, ELIZABETH. Tradition and Contract: The Problem of Order. The Lewis Henry Morgan Lectures/1973 presented at the University of Rochester, Rochester, New York. Chicago: Aldine. 1974. Pp. xi, 140. \$6.95.

DARRACOTT, JOSEPH (selected and ed.). The First World War in Posters: From the Imperial War Museum, London. Dover Art Collections. New York: Dover Publications. 1974. Pp. xiii, 74 plates, xvii–xxiii. \$4.95.

DE ROOVER, RAYMOND. Business, Banking, and Economic Thought in Late Medieval and Early Modern Europe: Selected Studies. Ed. by JULIUS KIRSHNER. Chicago: University of Chicago Press. 1974. Pp. viii, 383. \$13.00.

DURNBAUGH, DONALD F. (ed.). Every Need Supplied:
Mutual Aid and Christian Community in the Free
Churches, 1525-1675. Documents in Free Church
History. Ph:ladelphia: Temple University Press.
1074. Pp. xir. 288. \$18.00.

1974. Pp. xiv, 258. \$15.00.
EISENSTADT, S. N., et al. Post-Traditional Societies.
Ed. by S. N. EISENSTADT. New York: W. W. Norton.
[1974.] Pp. xi, 257. Cloth \$10.00, paper \$3.50.

ELKANA, Y. (ed.). The Interaction between Science and Philosophy. The Van Leer Jerusalem Foundation Ser. Atlantic Highlands, N.J.: Humanities Press. 1974. Pp. xvii, 481. \$17.50.

Press. 1974. Pp. xvii, 481. \$17.50.

ESLER, ANTHONY (ed. and with an introd.). The Youth Revolution: The Conflict of Generations in Modern History. Problems in European Civilization. Lexington, Mass. D. C. Heath. 1974. Pp. xxiii, 173. \$2.95.

GAY, PETER, and WEXLER, VICTOR G. (eds.). Historians at Work. Vol. 3. New York: Harper and Row. 1975. Pp. xi, 325. \$15.00. GUNST, PÉTER (ed.). Bibliographia Historiae Rerum

GUNST, PÉTER (ed.). Bibliographia Historiae Rerum Rusticarum Internationalis, 1969–1970. Budapest: Museum Rerum Rusticarum Hungariae. 1974. Pp.

HARRYHAUSEN, RAY. Film Fantasy Scrapbook. 2d rev. ed.; South Brunswick, N.J.: A. S. Barnes. 1974. Pp. 142. \$12.00.

HEER, FRIEDRICH. Challenge of Youth. University: University of Alabama Press. 1974. Pp. 224. \$8.50. HOBBES, THOMAS. De Homine: Traité de l'homme. Tr. and commentary by PAUL-MARIE MAURIN. Fondazione "Giorgio Ronchi," 23. Paris: Librairie Scientifique et Technique Albert Blanchard. 1974. Pp. 204. 30 fr.

HOLDEN, BARRY. The Nature of Democracy. Nelson's Political Science Library. New York: Barnes and Noble. 1974. Pp. xvii, 240. Cloth \$13.75, paper

International Bibliography of Historical Sciences. Vols. 39-40, 1970-1971, including some publications of previous years. Ed. with the contribution of the national committees by MICHEL FRANÇOIS and NICOLAS TOLU for the International Committee of Historical Sciences, Lausanne. Published with the assistance of UNESCO and under the patronage of the International Council for Philosophy and Humanistic Studies. Paris: Armand Colin, 1973, Pp. xxviii, 567.

Colin. 1973. Pp. xxviii, 567.

KLONSKY, MILTON. The Fabulous Ego: Absolute Power in History. New York: Quadrangle/New York Times Book Co. 1974. Pp. xii, 436. \$15.00.

LATREILLE, ANDRÉ. L'ère napoléonienne. Collection U. Paris: Armand Colin. 1974. Pp. 383. LIDDELL HART, B. H. Strategy. Signet Book. 2d rev. ed.;

New York: New American Library. 1967. Pp. xxi, 426. \$2.25. See rev. of 1st ed. (1954), AHR, 60 (1954-55): 641.

MARX, KARL. The First International and After. Ed. and with an introd. by DAVID FERNBACH. Political Writings, vol. 3. New York: Random House. 1974. Pp. 417. Cloth \$12.95, paper \$2.95.

MCLAUGHLIN, ELIZABETH T. Ruskin and Gandhi. Lewisburg, Pa.: Bucknell University Press. 1974. Pp. 202. \$10.00.

MODELL, SOLOMON. A History of the Western World. Vol. 2. Englewood Cliffs, N.J.: Prentice-Hall. 1974.

Pp. xxv, 831. \$9.95.

MORSE, GRANT W. Complete Guide to Organizing and Documenting Research Papers. New York: Fleet

Academic Editions. 1974. Pp. 156. \$15.00.

MOSSE, W. E. Liberal Europe: The Age of Bourgeois Realism, 1848-1875. History of European Civiliza-tion Library. New York: Harcourt Brace Jovanovich. 1974. Pp. 180. \$3.95.

NITTOLO, GEORGE. Victim of Environment. New York: Vantage Press. 1974. Pp. viii, 229. \$6.95.

O'CONNOR, JOHN E., and JACKSON, MARTIN A. Teaching History with Film. Discussions on Teaching, 2. Washington: American Historical Association. 1974. Pp. 74. \$1.00.

Oil and Security. A SIPRI Monograph: Stockholm International Peace Research Institute. New York: Humanities Press. 1974. Pp. 197. \$10.00.

ROBERTS, MICHAEL. Macartney in Russia. The English Historical Review, supplement 7. London: Long-

man. 1974. Pp. 81. £1.25.

SALTOR, JORGE E. La crisis de la noción de verdad: A propósito de algunas investigaciones del empirismo lógico. Cuadernos de humanitas, no. 39. Tucumán: Universidad Nacional de Tucumán, Facultad de Filosofía y Letras. 1972. Pp. 122.

SILAGI, MICHAEL. Henry George und Europa: Zur Entstehungsgeschichte der europäischen Bodenreformbewegungen. Munich: Etana. 1973. Pp. viii,

193. DM 25.

SOBEL, B. Z. Hebrew Christianity: The Thirteenth Tribe. Contemporary Religious Movements: A Wiley-Interscience Ser. New York: John Wiley and Sons. 1974. Pp. xi, 413. \$12.50.

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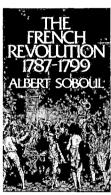
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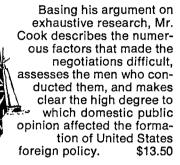




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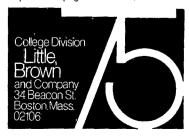
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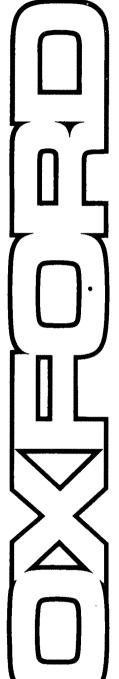
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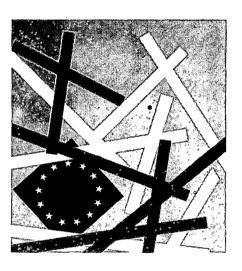


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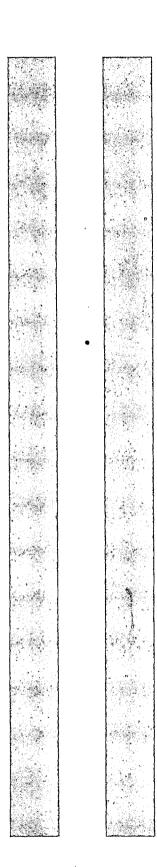
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American Historical		National Geographic Society 4	15
Association 46, 48,	49	New York University Press	32
Barnes & Noble Books	47	Oxford University	
Cambridge University Press	21	Press 2d Cover, 25, 3	}1
Columbia University Press 12,	13	Pantheon Books	38
Cornell University Press	29	Prentice-Hall 26, 2	:7
Dorsey Press	22	Princeton University Press 4th Cove	er
Doubleday & Co.	24	Schocken Books	8
Greenwood Press	42	Charles Scribner's Sons 4	14.
Harper & Row, Publishers	15	Stanford University Press 2	23
Harvard University		St. Martin's Press	9
Press 3d Cover, 19,	39	University of California Press	5
Indiana University Press	11		37
International Congress of			
Historical Sciences	3	University of Minnesota Press	4
Johns Hopkins University Press	36	University of Texas Press 4	ŧο
Alfred A. Knopf 10, 33, 34,	35	Viking Press ' 2	8:
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Little, Brown & Co. 7,	30	Yale University Press	6

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